THURSDAY, AUGUST 28, 1890.

THEORETICAL BALLISTICS.

A Revised Account of the Experiments made with the Bashforth Chronograph, to find the Resistance of the Air to the Motion of Projectiles, &c. By Francis Bashforth, B.D., late Professor of Applied Mathematics to the Advanced Class of R.A. Officers, Woolwich, and formerly Fellow of St. John's College, Cambridge. (Cambridge: University Press, 1890.)

ROBINS, in the last century, revolutionized the science of artillery by his invention of the ballistic pendulum; and in our own times Mr. Bashforth has accomplished the same thing for modern rifled artillery, by the aid of electricity and by his own chronograph.

Previous to Robins's experiments, the vaguest ideas prevailed as to the velocity of cannon shot and musket bullets: it was never supposed that such a light medium as the air could offer the enormous resistance it does; and the resistance of the air being supposed almost insensible, and Galileo's parabolic theory being applied, the velocity of projectiles was very much underestimated. At the same time, to reconcile Galileo's theory with the observed ranges in practice, it was usual to suppose the first part of the trajectory to be a finite straight line, the *point-blank range*, and to add the parabola at the end of the straight line.

The ballistic pendulum of Robins enables us to dilute the velocity of the bullet so as to make it easily measurable; and by firing at the pendulum from different distances, and calculating the loss of velocity through the air, we are able to obtain a fair estimate of the resistance. Robins found in this manner that the resistance of the air to a bullet, three-quarters of an inch in diameter, weighing one-twelfth of a pound, is about 10 pounds, or 120 times the weight of the bullet at a velocity of about 1600 feet per second. By firing with a charge of powder half the weight of the ball at the ballistic pendulum at ranges of 25, 75, and 125 feet, he found that the mean velocities of impact were respectively 1670, 1550, and 1425 f.s.

Now denoting by R the average resistance in pounds over the first 50 feet, in which the velocity fell from 1670 to 1550, the principle of energy gives, in foot-pounds,

$$50R = \frac{1670^2 - 1550^2}{2 \times 32{\cdot}2 \times 12}, \text{ or } R = 10.$$

Robins proceeds to theorize by the principle of mechanical similitude, and shows that a 24-pound cannon-ball fired with a charge of 16 pounds of powder, should acquire a velocity of 1650 f.s., and that the resistance of the air would then amount to 540 pounds, or nearly twenty-three times the weight of the shot. He is now able to clear up the difficulty of the supposed point-blank range, the distance during which the shot is conceived to fly in a straight line. To reconcile the parabolic theory of Galileo with the observed very small curvature of the trajectory at the outset, ancient writers on ballistics were in the habit of making a concession to the vulgar opinion (an opinion not yet extinct, although Tartaglia pointed out its fallacy) that the path of a shot was a straight line for a certain distance, called the point-blank range, during which the shot "flyeth violently," the *motus violentus* of old writers.

But now Robins is able to show that, in consequence of the much higher velocity of the shot, and the much greater resistance of the air than was ever considered, a 24-pound shot fired with two-thirds of its weight of powder, will, at a distance of 500 yards from the piece, be separated from the line of its original direction by an angle of little more than half a degree, so small an aberration as not to be noticeable with crude artillery appliances; and generally that the track of the shot departs greatly from the parabola, and is much more closely imitated by the combination of *motus violentus* in a straight line, *motus mixtus* in a curve or circular arc, and *motus naturalis* in a vertical line, the vertical asymptote of the true path, as taught by the old writers on artillery.

The treatise of Robins, "New Principles of Gunnery," 1742, attracted immediate attention, and was translated with a commentary by Euler.

The ballistic pendulum employed by Robins weighed about 56 pounds, and was used only with musket bullets; and to this day it will probably be found the most efficient instrument for measuring the velocity and retardation of small-arm projectiles; the threads or wires of the electric screens being easily missed by bullets, or, if struck, being apt to deflect them.

Experiments were made at Woolwich by Hutton in 1775 and by Gregory in 1815, and by Piobert, at Metz, in 1839, to apply the ballistic pendulum to cannon-balls; and although not such an accurate instrument on a large scale, in consequence of elasticity and vibration, still it was the only means at hand till the invention of the electric telegraph. The application of electricity to the measurement of the time of flight of the cannon-ball immediately suggested itself to various minds—Wheatstone, Konstantinoff, and Bréguet—and a chronograph was soon produced, capable of registering two instants of time, and thence one velocity; as performed at present by the Boulengé chronograph, now in universal use for the determination of muzzle velocities and the proof of powder.

Notwithstanding the obvious advantages of electricity so late as 1855 a monster ballistic pendulum was constructed to the order of the Government, and first set up at Shoeburyness, then at Woolwich, and finally dismantled without ever having been used in any course of experiments. The model alone of this instrument, shown at the Exhibition of 1862, is reported to have cost £800; but for all practical purposes the pendulum could have been replaced by a large box rammed with sand, and suspended by chains about 6 or 8 feet long, and the indications would probably have been more accurate. The experimenters who followed Robins would have succeeded better if they had expended all their care and ingenuity upon experiments on a small scale; and really with all their trouble it is found that, when checked by electric records, their results are not so accurate as the original observations carried out by Robins.

The problem of the electric chronograph was occupying Mr. Bashforth's mind when he received the appointment of Professor of Mathematics to the newly instituted Advanced Class of Artillery Officers in 1864; where he was well placed for carrying out his experimental ideas, with the assistance of his enthusiastic pupils.

The conditions to be secured which Mr. Bashforth set before himself were—

(1) The time to be measured by a clock going uniformly.

(2) The instrument to be capable of measuring the times occupied by a cannon-ball in passing over at least nine successive equal spaces.

(3) The instrument to be capable of measuring the longest known time of flight of a shot or shell.

(4) Every beat of the clock to be recorded by the interruption of the same galvanic current, and under precisely the same conditions.

(5) The time of passing each screen to be recorded by the momentary interruption of a second galvanic current, and under precisely the same conditions.

(6) Provision to be made for keeping the strings or wires of the screens in a uniform state of tension, notwithstanding the force of the wind and the blast accompanying the ball.

To secure these conditions practically, Mr. Bashforth had to invent his own chronograph, for a detailed description of which the reader must refer to the book; but it consists essentially of a brass cylinder provided with a heavy fly-wheel movable about a vertical axis; and of two markers tracing spiral lines on paper placed on the cylinder, and giving an indication by a jerk on the spiral corresponding to the cutting of one of the electric screens by the shot, or to half-seconds of the clock.

The fly-wheel being spun by hand, and the clock making continual half-second records, the word is given to fire the gun, and then the screen records are registered on the paper by the screen-marker. When the paper is full, after five or six rounds, the cylinder is transferred to a micrometer instrument, and the records read off with a vernier and microscope as accurately as possible.

We may take it that the average travel of the paper on the cylinder is an inch for about a tenth of a second, so that, with screens 150 feet apart, an average velocity of 1500 f.s. would give screen records at intervals of about an inch. Readings of tenths of an inch will give hundredths of a second, and of hundredths of an inch will give thousandths of a second, which is about as far as can be seen or measured with this instrument. But, by treating the last significant figure as indeterminate, and smoothing down irregularities by differencing and interpolation, Mr. Bashforth is able to assign probable values to the 4th and even 5th decimal of the second, in the instant at which any screen is cut.

Any improved instrument which would give a velocity to the paper of ten times or one hundred times of Mr. Bashforth's velocity would increase the recording accuracy theoretically to the same extent; but, as Mr. Bashforth claims for his instrument, he has located the shot at any instant to within about one foot of range, an error comparable with inaccuracies in the measured distance between the screens, inclination of the screens, and bending or stretching of the screen wires before breaking.

The chronograph having given us the instants of time, say $t_1, t_2, t_3, \ldots$ at which screens 1, 2, 3,... at equal intervals of l feet were cut by a shot, we have to employ the methods of Finite Differences for converting these records into expressions for the velocity and retardation at any point.

It will be noticed that the chronograph records give, by interpolation, t as a function of s, not s as a function of t, so that the velocity v is the reciprocal of dt/ds, while the retardation is $\dfrac{d^2t}{ds^2}v^3$; and if the shot weighs W pounds, the resistance of the air is $W\dfrac{d^2t}{ds^2}v^3$ poundals, or $W\dfrac{d^2t}{ds^2}v^3 \div g$ pounds: the shot flying so fast that, practically, we may take it as moving in a horizontal line.

Formulas of the calculus of Finite Differences will give us the values of dt/ds and d^2t/ds^2 in terms of the successive differences $\Delta t, \Delta^2 t, \ldots$ of t; those employed by Mr. Bashforth being—

$$l\frac{dt}{ds} = \Delta t - \tfrac{1}{2}\Delta^2 t + \tfrac{1}{3}\Delta^3 t \ldots\ldots$$

$$l^2\frac{d^2t}{ds^2} = \Delta^2 t - \Delta^3 t + \tfrac{11}{12}\Delta^4 t \ldots\ldots$$

To take a simple numerical illustration, suppose it was found by the chronograph that a shot weighing 70 pounds, flying horizontally, cut three equidistant screens 150 feet apart at instants of time 2·3439, 2·4325, 2·5221 seconds. The time from the first to the third screen being 0·1782 second, the average velocity over this 300 feet is $300 \div 0{\cdot}1782 = 1684$ f.s.; and we may take this as being the velocity at the middle screen—an assumption which is accurately true if the resistance varies as the cube of the velocity—that is, if d^2t/ds^2 is constant.

Again, $\Delta^2 t = 2{\cdot}3439 - 2 \times 2{\cdot}4325 + 2{\cdot}5221 = 0{\cdot}001$; so that $d^2t/ds^2 = 0{\cdot}001 \div (150)^2$; and therefore the resistance of the air is $70 \times (1684)^3 \times 0{\cdot}001 \div (150)^2 = 14{,}850$ poundals, or 464 pounds.

Experiment confirmed the reasonable hypothesis that the resistance of the air is proportional to the cross-section, or to d^2, if d is the diameter in inches; so that, for similar projectiles, Bashforth introduces his coefficient K, defined so as to make the resistance of the air at a velocity v f.s. to a projectile d inches in diameter to be—

$$d^2 K\left(\frac{v}{1000}\right)^3 \text{ poundals, or } d^2\frac{K}{g}\left(\frac{v}{1000}\right)^3 \text{ pounds;}$$

while, if the weight of the shot is W pounds, the retardation due to the resistance is

$$\frac{d^2}{W}K\left(\frac{v}{1000}\right)^3 \text{ celoes;}$$

and thus

$$\frac{d^2}{W}K = 10^9\frac{d^2t}{ds^2}, \text{ or } K = \frac{W}{d^2}10^9\frac{d^2t}{ds^2}.$$

The coefficient K is now found experimentally to be the same for all similar projectiles, whatever the weight, W pounds, or diameter, d inches; and the factor of mechanical similitude W/d^2, now called the ballistic coefficient and generally denoted by C, enables us to generalize the experiments made on one scale to projectiles of all sizes.

We now see the convenience of splitting up the resistance of the air into two factors, one of them being the cube of the velocity; for in the retardation the other factor is d^2t/ds^2, which is given very simply in terms of $\Delta^2 t, \ldots$

It is very often asserted that "Bashforth assumed the resistance of the air to vary as the cube of the velocity"; whereas in reality Bashforth found it convenient to take out the cube of the velocity as one factor of the resistance, and to tabulate the other factor, as a slowly varying quantity.

Practically, we find that $\Delta^2 t$ is about one-thousandth of a second when $l = 150$, the distance between the screens in feet, so that d^2t/ds^2 is a decimal beginning with six or seven zeros. Mr. Bashforth avoids this inconvenience by writing the retardation—

$$10^9 \frac{d^2t}{ds^2}\left(\frac{v}{1000}\right)^3,$$

equivalent to reckoning the velocity in thousands of feet per second.

We have explained this notation at some length, as Mr. Bashforth has taken this and all other notation for granted as known, which is already given in his "Motion of Projectiles." The numerical values of K from the experiments are given in Table XI. for spherical and in Table XII. for ogival-headed projectiles; these two tables containing the complete theoretical deduction of all the author's numerous experiments.

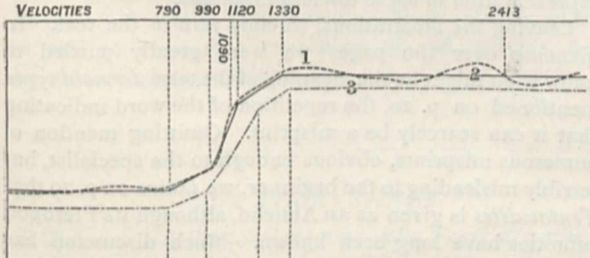
But as for very high or very low velocities the Newtonian law of resistance, varying as the square of the velocity, is more likely to be near the truth, the author has converted his coefficients K for the cubic law into coefficients k for the Newtonian quadratic law, tabulated in Tables I.–VI.; here, again, he has omitted to explain the formula required in the use of k; but it is easily inferred to mean that the resistance of the air is

$$d^2k\left(\frac{v}{1000}\right)^2 \text{ poundals, or } d^2\,\frac{k}{g}\left(\frac{v}{1000}\right)^2 \text{ pounds,}$$

so that the relation connecting k and K is $1000k = v\mathrm{K}$.

In the practical use of the tables, we choose the one in which k or K is the more nearly constant and changes the slower.

The value of k for ogival-headed projectiles has been plotted graphically in the following diagram by Mr. A. G. Hadcock, quoted by Mr. Bashforth on p. 149; curve 1 being drawn from the result of Mr. Bashforth's experiments; curve 2 from the empirical laws of General Mayevski deduced from Bashforth's experiments; and curve 3 from the empirical laws of Captain Ingalls, drawn up to represent the resistance of the air according to Krupp.



The diagram is interesting as showing how far the Newtonian law is true for very low and very high velocities, and it confirms in a remarkable way the change in the value of k as we pass through the velocity of sound, so that its final value is about three times its initial value, as found out by Robins; insomuch that the resistance of the air to a 12-pound shot moving at 1700 f.s., which, according to the experiments of Newton on slow motions ("Principia," lib. ii., Props. 38–40), ought to have been 144 pounds, was found by Robins to be 433 pounds, or three times as much.

At velocities less than that of sound the projectile is always moving among its own waves; at greater velocities the point is supposed to be cleaving undisturbed air, like a swift steamer on the water; and now the chief element of resistance arises in the energy drawn off in the waves in the wake, waves which have been photographed by Mach and Salcher, according to an article signed "B." in NATURE.

Recently it has been discovered that with high velocities the shot carries the sound of the gun along with it, while backwards and sideways the sound is propagated at its ordinary rate; this phenomenon is sufficient to destroy the utility of range-finders based upon the observation of the velocity of sound.

Curve 3 indicates that the resistance of the air to Krupp's projectiles is about 10 per cent. less than to ours; this may be attributed to the sharper point, better centering obtainable with breech-loading, and a slightly less standard density of air; but Mr. Bashforth points out, with some justice, that the resemblance of Krupp's curve 3 to his own is rather suspicious, considering the small number of published experiments upon which Krupp's experiments are based.

Mr. Bashforth honestly prints all the values of K derived from his experiments, values often exhibiting great discrepancies among each other, and takes their mean as the true value; whether more delicate chronographs and improved electrical manipulation will enable us to refine on Bashforth's results remains to be seen, as a correction in the first decimal place of the value of K, depends upon the millionth of a second—a refinement we are very far off from having attained. Mr. F. J. Smith has given an account of a chronograph of his own invention, and in the August *Phil. Mag.* a description of a method of eliminating the latency in electro-magnetic records in chronographs, which may prove very useful. A chronograph to read directly to one ten-thousandth of a second is now the great desideratum: when chronographs were first brought out, the millionth of a second was glibly talked about, but so far, the thousandth is very good work indeed.

The experimental part of work is concluded when the value of K is obtained; but on these experiments Mr. Bashforth is able to build up his tables of T and S (XXIII.–XXXIII.), which enable us to calculate beforehand the performance of any gun, and save thousands of pounds in gunpowder at the price of a little ink.

Knowing C, the ballistic coefficient of the gun, then the formulas

$$t = \mathrm{C}(\mathrm{T_V} - \mathrm{T}_v), \quad s = \mathrm{C}(\mathrm{S_V} - \mathrm{S}_v),$$

connect the distance s in feet and the time t in seconds, for any initial velocity V, and final velocity v.

An additional table, for D, invented by Mr. W. D. Niven, is not given by Mr. Bashforth, but is found of great practical use; it gives δ, the deviation in degrees in a vertical plane for a flat trajectory, by the formula—

$$\delta = \mathrm{C}(\mathrm{D_V} - \mathrm{D}_v).$$

Colonel Siacci, of the Italian artillery, has converted

Niven's D into circular measure, or natural tangents, and called it I; and has added another useful function, A, the altitude function. The use of these functions is indispensable in modern ballistics; but Mr. Bashforth does not mention them, as the chief purport of his book seems to be to put on record his own share of the work; and certainly, once the experimental part is done, it is a very easy matter to sit quietly indoors and theorize upon it.

A very searching test of Mr. Bashforth's tables was proposed in 1887, when it was decided to fire the "Jubilee rounds" from the 9·2 inch at elevations of 40°–45°, to see what is the extreme range attainable with modern artillery; and calculations were invited, to be sent in before the gun was fired. Mr. Bashforth prints the result of his calculations, which assigned a range of 19,426 yards with an elevation of 40° and an initial velocity of 2360 f.s. The range attained one day when the gun was fired was over 21,000 yards, and on another day was over 20,000, the difference being attributable to wind; so that, with no allowance for wind, and the fact that the initial velocity was really about 2375 f.s., we must consider that the calculation was close enough to show the value of Bashforth's coefficients; other calculators who allowed for the better shape and steadiness of the projectile obtaining even closer agreement. The calculation is interesting as showing the great height to which the projectile rises, and the consequent necessity for a frequent change in the coefficient of resistance due to the tenuity of the atmosphere.

Prop. VII., Robins's "New Principles of Gunnery," asserts:—"Bullets in their flight are not only depressed beneath their original direction by the action of gravity, but are also frequently driven to the right or left of that direction by the action of some other force."

This well-known effect in golf is still more marked in rifled artillery, especially with high-angle fire; and now in modern ballistic tables we have columns added for M and B, two functions calculated theoretically by General Mayevski, for assigning the value of this lateral deviation or drift.

Mr. Bashforth devotes chapter vi. to a popular exposition of this phenomenon, which is still somewhat wrapped in obscurity, in spite of all that has been written about it; a list of which writings is given by Captain Ingalls in his "Hand-book of Problems in Exterior Ballistics."

The stability of the axis of the projectile imparted by the rotation has the effect of making the head of the shot point slightly to the right of the vertical plane of fire with right-handed rotation, thus causing *drift*, and also of keeping the head a little above the tangent of the trajectory, so that in its descent the shot experiences a so-called *kite-like action*, tending to increase the range. It is well, however, for theorists to be on their guard in offering an explanation, as observers are not always agreed as to what really takes place.

Mr. Bashforth expresses a fear that, after all his labours, he will have produced very little effect; but we hasten to reassure him that his work is held in the highest estimation by those who have means of making a practical judgment.

A. G. GREENHILL.

BRITISH FOSSILS.

British Fossils, and where to seek them; an Introduction to the Study of Past Life. By J. W. Williams. Pp. 96, Illustrations. (London: Swan Sonnenschein and Co., 1890.)

AT the close of the introduction to this little volume, the author informs us that his object has been to convey to the young collector of British fossils the experience and knowledge acquired by others, whereby his own toil and labour may be lightened. The purport of this is admirable, but unfortunately the author has not succeeded in carrying out his good intentions. The volume is small, and merely a compilation; so there is no excuse for the number of errors and misprints by which its pages are disfigured.

The plan of the book seems to be to give a brief notice of each main geological horizon, with a list of some of the characteristic fossils, but we very much doubt whether long strings of generic names, like those given on p. 28, for example, are calculated to afford much assistance to the young collector, as there is practically no information as to what such terms really represent. None of the illustrations are original, the frontispiece being taken from Louis Figuier's "World before the Deluge," and most of the other figures from a well-known German work. And while on the subject of illustrations we should be glad to be informed why amphibians and reptiles like *Archegosaurus*, *Capitosaurus*, and *Placodus*, should have their skulls figured (as on pp. 45, 46) in a work on British fossils, when these genera are totally unknown from British strata. Such figures, as well as those on pp. 56, 57, may lead the inexperienced "young collector," for whom the book is avowedly written, to the conclusion that he may expect to meet with entire skulls and skeletons of fossil reptiles in his geological excursions. The proper course in these cases is, it need hardly be said, to give figures of teeth and some of the bones of such creatures, with which the tyro may be expected to meet, and to show how their generic affinities can be determined. Then, again, in reproducing the old figures of the Devonian fishes given on p. 33 the author might surely have alluded to the work of Dr. Traquair and other authorities showing how very far these figures are from being a truthful representation of these ancient creatures.

Leaving the illustrations, we may turn to the text. In glancing over the pages we were greatly puzzled to know what might be the meaning of the term *dermoid* types mentioned on p. 20, the repetition of the word indicating that it can scarcely be a misprint. Omitting mention of numerous misprints, obvious enough to the specialist, but terribly misleading to the beginner, we notice on p. 29 that *Tentaculites* is given as an Annelid, although its Pteropod affinities have long been known. Much discussion has taken place as to the affinities of the Palæozoic plant known as *Sphenophyllum*, but when on p. 43 the author calmly tells us that it is probably founded on the leaves of Calamites, he gives us a piece of information as new as it is erroneous. It is somewhat amusing to find the student referred, on p. 44, to the author's book on "Land and Fresh-water Shells," as if it were the only extant treatise on the subject; but when on p. 45 we are informed that Labyrinthodonts are characterized as a whole by the presence of "a ventral armour of oval scales," we again have to wonder at the author's sources of information. It is indeed true that many Labyrinthodonts have a ventral armour of bony scutes, but these can scarcely be described as oval, and in the typical Labyrinthodonts, to which some authorities restrict the term, such scutes are totally wanting. The essential features by which the Labyrinthodonts are characterized the author carefully refrains from mentioning. A trap is set for the unwary on pp. 51, 52—the shell mentioned on the one page as *Ammonites bucklandi* being alluded to on the next as *Arietites bucklandi*. Equally unfortunate with the author's mention of the Labyrinthodonts is his allusion on p. 59 to the Mesozoic mammals, where he repeats the exploded idea that *Stereognathus* was an Ungulate, thus carefully ignoring all the recent work relating to that peculiar group known as the Multituberculata, which appears to be allied to the Duck-mole. On the same page *Megalosaurus* is carefully separated from the Dinosaurs, to appear as a carnivorous lizard, whereas in the list on p. 62 it is placed in the former group. How totally out of date is the list on the page last-mentioned ought, moreover, to have been known to anybody acquainted with recent palæontological literature. Page 63 is noteworthy as containing at least six misprints in the spelling of scientific names; but perhaps the climax of blunders is attained on pp. 74, 75. Thus, on the former page we are gravely told that *Hyracotherium* is a hog; and if one fact has been repeated over and over again almost *ad nauseam*, it is that *Hyracotherium* is one of the early progenitors of the horse, being, in fact, identical with the American *Eohippus*, and we can hardly believe that the author wishes the student to understand that horses are descended from hogs! On p. 75 *Lingula* is carefully separated from the Brachiopods, while the well-known Crag Polyzoan *Fascicularia*—one of the commonest of Suffolk fossils—is stated to be a shell!

Finally, the glossary is an explanation of certain mineralogical and chemical terms having for the most part no sort of connection with British (or, for the matter of that, with any other) fossils. What connection can possibly exist between "astrakanite—a compound of magnesium sulphate and sodium sulphate deposited in winter time in the salt lakes near the mouth of the Volga," and the fossils of the British Islands, we are totally at a loss to imagine. A similar remark will apply to eclogite, which is said to be a rock consisting of red garnets and hornblende; although it really is one of the pyroxenes.

R. L.

OUR BOOK SHELF.

Il Teorema del Parallelogramma delle Forze dimostrato erroneo (con figure.) By Giuseppe Casazza. (Brescia: Stabilimento Tipografico Savoldi, 1890.)

IT is curious that the mathematical paradoxer should confine himself principally to the problem of "squaring the circle"—that is, to the attempt to prove that π is the root of a quadratic equation with rational coefficients, in algebraical language; while other simpler questions are at hand in which he might prove himself superior to the conclusions of ages, by solving the problems of the "duplication of the cube" and the "trisection of an angle."

Some paradoxers attain their own ends by a wrong result, for instance, in putting $\pi = \sqrt{10}$—a result easily tested by counting the revolutions of a railway carriage wheel of given diameter, in a journey of given length; others by ignoring the rules of the game, as Napoleon is reported to have played chess.

Our author must be congratulated upon having started a fresh question of controversy, which had till now been universally regarded as settled for about three hundred years.

The "parallelogram of forces" must have been known experimentally for thousands of years longer; but in the orthodox world, what is considered at the present time the best and simplest way of proving it theoretically in a strictly rigorous manner? The proof of our youth given by Duchayla is now voted cumbrous and antiquated; and only retained by veteran examiners as a searching test of logical power. Nowadays we cannot afford the time to linger over the elements, and it is customary to treat the "parallelogram of forces" as a corollary to Newton's second law of motion; but this cannot be considered perfectly satisfactory, as we are making the fundamental theorem of statics depend upon a dynamical argument.

Maxwell pointed out that, as we were concerned with a statical theorem, it was better in the proof to ignore the word "resultant," and to present a system of balancing forces at each step; and in this way he succeeded in framing a more simple rigorous statical proof, starting from the axiom that the resultant of two equal forces bisects the angle between them.

Again, by determining the conditions of equilibrium of three parallel forces, instead of as usual determining the resultant of two parallel forces, one figure will serve for all possible cases.

Practically it is the "triangle of forces" which we always work with, and not the "parallelogram," with the advantage in graphical statics of using only three lines of construction instead of five.

To return to our author, it is difficult to make out, with an imperfect knowledge of his language, whether he is writing ironically or not. His dynamical language is very loose; he uses "force" and "velocity" as convertible; and throwing his remarks into the style of Galileo's dialogues, he seeks to controvert all Galileo's conclusions. On p. 17 he provides the critic with an appropriate and characteristic quotation with which to conclude—"ho pero spesso dei momenti in cui gettando all' aria i libri che mi trovo sotto mano, esclamo: *Ma io sono un allucinato!*"

A. G. G.

L'Esprit de Nos Bêtes. Par E. Alix. (Paris: J. B. Baillière et Fils, 1890.)

Les Facultés Mentales des Animaux. Par le Dr. Foveau de Courmelles. (Paris: J. B. Baillière et Fils, 1890.)

THE writers of these two books have very much the same object in view. Their aim is to show that the mental life of animals differs only in degree, not in kind, from that of man. If anyone still thinks that animals are merely machines, or that they have no higher faculties than instinct, it would be well worth his while to consider what either Dr. Courmelles or M. Alix has to say on the subject. No impartial person could study the evidence brought together by either of the two writers, and continue to doubt that animals display intelligence in the strictest sense of the term, and that they share in varying degrees many of the emotions which are often supposed to be exclusively characteristic of the human race. Of the two works, the one by M. Alix is the more elaborate. In both books the materials are well arranged, and the authors have persistently sought to present their facts and ideas brightly and pleasantly.

Elementary Arithmetic. By C. Pendlebury, M.A., F.R.A.S., and W. S. Beard, F.R.G.S. (London: George Bell and Sons, 1890.)

In a book on elementary arithmetic it is necessary that there should be throughout a good and well graduated series of examples. The authors of the present volume have got together a large number of examples and problems for written work, and in addition they have arranged numerous sets for use in oral teaching form, a very important feature in an elementary work of this kind. The explanatory matter is written in intelligible and simple language, and great attention has been paid to the treatment of the money rules and the more important weights and measures.

This book is intended to serve as an introduction to the one on "Arithmetic for Schools," and the examples have been arranged so that they are all different from those given in the advanced work.

LETTERS TO THE EDITOR.

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British Association Procedure.

It has been to me a matter of surprise that a letter by Prof. O. Lodge published in these columns on October 17, 1889, did not elicit other similar communications, as the views he enunciated are undoubtedly those of a not inconsiderable number of active members of the British Association. Prof. Lodge pointed out that the British Association week undoes the benefit of the previous holiday, mainly because the conditions under which the work of the Sections is carried on are prejudicial to health. This I know from considerable personal experience to be the case; and in this and previous years I have had the remark addressed to me: "Surely you are not going to *that* British Association meeting to make yourself ill again."

Prof. Lodge suggested that the Sections should sit from 10 a.m. till 1 or 1.30 p.m., and that the Sectional Committees should meet afterwards. Such an alteration would doubtless mitigate the evils inseparable from the present system, and it is to be hoped that a determined effort will be made at the ensuing meeting to promote its adoption. A recommendation somewhat to this effect was, I believe, made to the Council by at least one of the Sections at the Birmingham meeting, but nothing came of it. This is, perhaps, not surprising; indeed, it is a question whether anything ever does come of recommendations to the Council—so some say.

Machinery devised 50 odd years ago is no longer capable of satisfactorily coping with modern requirements. We do not go to British Association meetings to sit for hours together to hear papers read such as we have listened to *ad nauseam* during the previous sessions—our main object is to meet and exchange views; but everything seems to be done to prevent rather than to promote this. Far fewer papers should be read; far more care should be devoted to the selection of papers; much more should be done to encourage discussion, especially between Sections; and ample time and opportunity should be given for conversation.

The Sectional Committees are absurdly unwieldy bodies, and in the case of some Sections, practically comprise the entire Section: an appeal was made to the Council by my Section to put a stop to a practice which enables all the nobodies to become members of the Sectional Committee, but without result: I believe we were told that we could do as we liked. Had this been the case, we should scarcely have troubled the Council. The Sectional officers, with at most half a dozen other members, would form a far more useful Committee than any larger number; but if it be thought otherwise, let the whole Section sit as a Committee.

Lastly, a word may be said as to the date of meeting. Could any time be more unsuitable than the beginning of September? Most of those who are engaged in advancing science are then in the very middle of their holiday, and can attend only at grave personal inconvenience. Henry E. Armstrong.

The Mode of Observing the Phenomena of Earthquakes.

The publication of Mr. Davison's paper "On the Study of Earthquakes in Great Britain," in Nature of the 7th inst. (p. 346) furnishes me with an opportunity of making a few *remarks*, followed by a *suggestion* as to the mode of recording the effects of seismic disturbances of the earth's crust on the apparent change of position, especially of vertical objects, in the field of vision of the observer.

Remarks.—It will, I think, be admitted that the descriptions of the alleged rocking to and fro of walls, towers, and chimneys, may not unfrequently convey an exaggerated idea of what really takes place; and, probably, the same is true of the narratives of personal experiences of reeling or rolling movements on the part of the narrator. I refer, of course, to the alleged *extent* of these movements, for no one can doubt their actual *occurrence* as the result of a *tremblement de la terre.* Such composite structures as walls, towers, and chimneys have a real flexibility and elasticity, as is shown, for example, by the opening and shutting up of cracks and fissures in their substance. But the extremely vivid accounts which we read of the swaying to and fro of solid buildings, as witnessed by persons in the upright position, and by others who are recumbent, suggest at least that some of these recorded disturbances of position in external objects may be more apparent than real, and may depend on some sudden uncontrollable movement of the head, and therefore of the optic axes of the observer's eyes.

It is well known that, when the head is moved swiftly to one side and back again to a vertical position, upright objects. seen in front, appear to shift from their vertical position in an opposite direction, and then back again. It is not here needful to explain scientifically this very obvious phenomenon. A similar apparent displacement of objects, though in a vertical direction, occurs when the head is nodded backwards and forwards. Movements of the head in intermediate directions produce intermediate effects; whilst rotatory movements of the head give rise to corresponding though mixed kinds of disturbance of objects in the field of vision. Lastly, if the observer is in a horizontal position, as in bed, for example, a sudden rolling over of the head to one side and back again produces like phenomena.

Now such disturbing movements of the optic axes must frequently occur in the case of persons suddenly subjected to the consequences of earthquake tremors, whether such persons are in a vertical or in a recumbent position; and it is difficult to understand how they should not *occasionally* seem to exaggerate the apparent effects of the disturbance of the earth's surface and the objects planted upon it. The equilibrium of the observer's head is suddenly disturbed in a given direction, and an opposite involuntary movement instantly occurs in order to restore the previous condition of things. Granting, then, the *objective* reality of the swaying movements of vertical structures during earthquakes, there seems to me to be reason to think that the effect of these is occasionally enhanced, and their record influenced by the *subjective* impressions due to movements occurring in the observer's own optical apparatus.

Suggestion.—Supposing this view to be correct (though I can furnish no direct proof of its truth from earthquake records), it appears to me that the *suggestion* of which I spoke at the commencement of this letter would be a useful addition to paragraph (*a*) of Section 2, Division A, of Mr. Davison's paper (p. 348), which relates generally to the "situation of the observer." This suggestion is that it should always be noted and stated towards which *point of the compass* the observer's face was directed at the moment of each observation, concerning the deflection of upright buildings, rocks, and so forth, especially of their lateral deflections. For it is obvious that if a sufficient number of such observations were recorded, it ought to happen, on my hypothesis, that persons who looked *across* the earth-waves would be moved up and down, and thus would have the vertical movements only of objects in front of them exaggerated, whilst persons who looked along the waves would be swayed sideways by the undulations of the soil, and would therefore have the extent of the lateral movements apparently increased. Under the former condition, the "little hills" might appear to dance; in the latter case, cliffs, towers, walls, and chimneys might seem to sway inordinately from side to side.

Many such observations, duly recording the variations in the apparent *extent* of the movements noted, together with the *positions* of the observer as regards the compass, would, when compared with the ascertained direction of the earth-waves, confirm or upset my general supposition. But, if this were found to be correct, such observations would furthermore constitute, even in the absence of special seismic instruments, a certain amount of evidence as to the actual direction of the earth-waves on any particular occasion.

Similarly, a record of the exact direction of recumbent observers in regard to the points of the compass, might, when compared with their respective descriptions of the movements of objects about them, serve a similar purpose.

Man himself would thus to a certain extent—that is, as regards the local direction of the earth-waves—be his own seismometer. Possibly, some evidence on this subject might even now be obtained by comparing the descriptions of the appearances with the ascertained directions of the outlook of different observers. JOHN MARSHALL.

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On a Problem in Practical Geometry.

IN treatises on practical geometry rules are given by which an arc and its chord or an arc and its tangent may be divided proportionally, but they leave an error which is often too great.

By the following method the points of division move step by step towards their required positions until errors are of less than any assigned amount.

Let GMH be the chord (Fig. 1), M its middle point, AOB the perpendicular diameter. In AOB produced take a series of points B′B″B‴..., determined thus: BB′ = BG, B′B″ = B′G... Then evidently circles with centres at BB′B″..., passing through G, form a series of which each has on its circumference the centre of the succeeding one. These circles cut the line AM in a series of points A′A″..., and the arcs

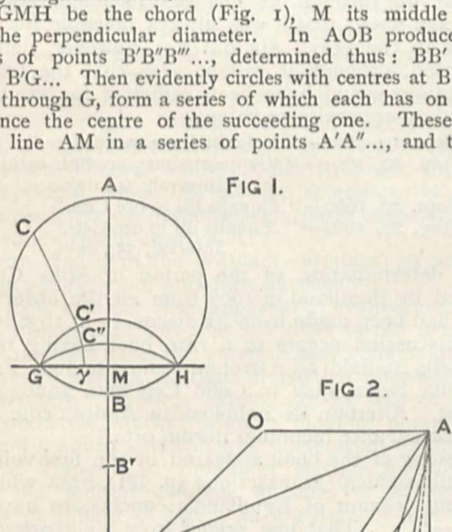


FIG I.

FIG 2.

GAH, GA′H, GA″H... get rapidly nearer the straight line GH. Any point C on the given arc GAH may now move to its destination γ on the line GH by stepping up to each circle in the direction of its centre; along a path CC′C″..., made up of CC′ tending to B, C′C″ tending to B′, and so on.

That all the arcs which this path crosses and the chord to which it tends are divided proportionally at the points CC′C″... γ follows from the almost obvious theorem that if the centre of one circle is on the circumference of another, lines drawn from that centre intercept arcs of the circles having a constant ratio.

If the circles become inconveniently large before the required approximation is reached, we may use the following: C′C″... are the centres of the circles inscribed in GCH, GC′H..., which are easy to construct. Also it may be noticed that the angle made with AO by each of the parts of CC′C″... is half that made by the preceding part, and the process may be brought to an end at any stage with progressive accuracy by making the last angle one-third instead of half of the preceding one.

In Fig. 1 the process is closed after the second stage by drawing the last line (C″γ) not towards B″, but towards the middle point P of B″B‴. This may be done as soon as the last arc GC″H comes to be less than a quadrant.

When the chord becomes the tangent AT at A, the points AA′A″... coincide, all the circles have AT touching them at A, the radius of each is half that of the succeeding one, the arcs intercepted AC, AC′, AC″... are equal, and so we get in the limit Aγ, the length of the arc AC laid out on the tangent.

But in Fig. 2 an alternative construction is shown. Bisect TAC by AC′, TAC′ by AC″, and so on. Draw CC′, C′C″... at right angles to AC, AC′... The process is shown closed after the third stage by drawing C‴γ at right angles, not to AC‴, but to a line AF such that C‴AF is one-third of C‴AT. In the result Aγ is equal to arc AC.

JOHN BRIDGE.

Caught by a Cockle.

I HAVE often intended writing to you describing a curious occurrence which I witnessed on the coast of Queensland in September 1889, but I have as often forgotten to do so when the opportunity came. While out shooting, along a sandy beach, I noticed a small muddy patch just covered by the rising tide. In this I observed a bird, a sand-piper, which seemed to be striving in vain to rise. I could not think how the bird had become caught, but on coming up to it I found that one claw of one foot was firmly held by a large cockle (about 1½ in. by 2 in.). Of course the bird would have been drowned eventually (though the benefit to be derived by the cockle seems rather problematical); and though it seemed to be aware of its danger, yet it had made no attempt to free itself by trying to bite through the claw, as one sometimes reads of animals doing when caught in traps. As I believe this is rather an uncommon incident, I must make that my excuse for troubling you. D. MCNABB.

H.M.S. *Dart*, New Hebrides, July 3.

ON STELLAR VARIABILITY.

ON the hypothesis of the meteoritic origin of the various orders of cosmical bodies there is a grand and orderly variation, both in light and colour, in the case of every undisturbed swarm during its condensation from its most nebulous condition to that of a cool dark globe.

As by virtue of the ordinary evolutionary process an *undisturbed* swarm successively passes through the changes the results of which define the various groups, the light will wax through Groups I. to IV., and then wane till it is finally extinguished; at the same time the colour sequences will be successively passed through. But with such a variability as this, compared with the period of which our *annus magnus* is but a point of time, we have now nothing to do. We have to deal really with *disturbed* swarms or with double or multiple swarms through their various stages of condensation.

Let us take the purely disturbed swarms first. Imagine a nebula, sparse, and therefore so dim as hardly to be visible at all. Then, further, imagine the appulse of another, or the approach of a meteoritic stream. We shall have the condition which must bring about increased luminosity; the outburst may be short or sudden; the greater luminosity may last a short or a long time; the dying down of the light may be fast or slow. In that we shall have the possibilities of new and dying stars.

If the spectrum of the light produced by this clashing be observed, we may not have precisely the same phenomenon as that observed in the various groups defining the result of the orderly condensation of a single swarm, for the simple reason that we shall have two swarms or bodies to deal with. Even if the very highest temperature is reached, we shall not have *exactly* the same spectrum as that presented by Group IV.

The most stupendous case illustrating the above remarks is to be found in the Pleiades, the true structure of which has been revealed to us by Mr. Roberts. The principal stars are not really stars at all; they are simply *loci* of intercrossings of meteoritic streams, the velocities of which have been sufficiently great to give us, as the result of collisions, a temperature approaching that of α Lyræ, so far as we can judge by the spectrum; but that the α Lyræ conditions are not present is evidenced by the fact that in Pleione the broad dark hydrogen lines have a bright thin line running down their centres, indicating that we have intensely-heated hydrogen outside that which is absorbing.

So long as these meteoritic streams are interpenetrating and disturbing each other, so long the Pleiades will shine; but their light may soon cease if the disturbance comes to an end, for we are not dealing with masses of vapour like α Lyræ. Indeed, one of them seems to have already become invisible. Of the seven daughters of Atlas, one has disappeared. The "septem radiantia sidera" are seven no longer. The seventh had vanished before the time of Aratus.

> "The Pleiades; small the region
> They fill, and pale the light they dart.
> Seven journeyers men call them
> Though only six are visible to ken.
> No star, I wis, has vanished from Heaven's floor
> Within mortal tradition, and idly is that number
> Fabled. Natheless seven the names they bear:
> Alcyone, Merope, Celæno, Electra,
> Sterope, Taygete, and stately Maia."[1]

At the beginning of the action to which I have ascribed the present light of the Pleiades, we should have the appearance of a "new star," and the greater the light produced and the more sudden the outburst the more certainly would the appearance of a new star be chronicled. Many such stars have burst forth, and the phenomena recorded have been entirely in harmony with the explanation afforded by the hypothesis; but, as the discussion of these phenomena is not yet complete, I shall not in the present article touch further upon them; but I may point out that, before the existence of "variable stars" was recognized, as it is now, the increase in magnitude of a variable at maximum, rendering visible to the naked eye what was before invisible, was attributed to the creation of a new star. Hence it is that the first work done on the periodicity of variable stars grew out of observations of so-called Novæ.

Leaving on one side, then, any question of Novæ, we will inquire into the growth of our knowlege of stars the light of which is known to wax and wane with more or less regularity, and see to what causes this variability has been ascribed. We have to consider those shorter periods of light-variation, well within human ken, light-changes which, instead of taking millions and perhaps billions of years, are undergone in a few days, or weeks, or months. Such changes have been abundantly chronicled from the earliest times and acknowledged to be among the most mysterious phenomena presented to us in the skies.

In this historical survey we must first consider the case of Mira or o Ceti. It is now nearly three centuries ago since Fabricius noticed this star (August 1596), thinking it to be a *nova*, and watched its disappearance in the following October.[2]

Not only Fabricius but Kepler looked upon Mira Ceti as a new star similar to those of 1572 and 1604. Indeed, it was regarded as such until 1638, when some observations by Phocylides Holwarda brought out for the first time the fact that the changes in magnitude repeated themselves. The work done by this astronomer is quoted by Hevelius.

Holwarda first observed the star in December 1638, when it was brighter than a third magnitude; he watched it decrease to the fourth, and disappear during the summer of 1639. On December of the same year he again observed it. There is no doubt, indeed, that Holwarda was the first to demonstrate by these observations that the light of stars is liable to periodic changes in intensity.

Fullenius, a teacher of mathematics at Franecker, was the next to observe Mira. He noted that the star was visible on September 23, 1641, and the same date in the following year. In August 1644, however, no trace of it could be made out.

Junquis, a professor at Hamburg, recorded that Mira was of the third magnitude on February 18, 1647, and was invisible from July 1648 to November of the same year.

It was Hevelius, however, who made the first detailed investigation into the variations of the light of this star. Beginning in January 1648 he assiduously watched the changes in magnitude until March 1662, and placed the question of variability beyond the possibility of a doubt.

During the fifteen years of observation Hevelius saw the star go through its changes in magnitude many times, and noted that it was always invisible for several months in the year. He did not, however, determine the period, although it will be seen that the following observations would have been sufficient for him to have deduced an approximate value:—

Sept. 10, 1660—"Instar stellæ 4 magn. fere."
Aug. 20, 1661—"Vix quartæ magnitudinis extitit."
Interval, 344 days.
Sept. 20, 1660—"Æqualis illi in ore Ceti."
Aug. 29, 1661—"Æqualis illi in ore Ceti."
Interval, 353 days.

The determination of the period of Mira Ceti was deduced by Bouillaud in 1667 from all the observations which had been made from its discovery in 1638 to 1660. This discussion occurs in a rare book having the title "Ismaelis Bullialdi ad Astronomos monita duo: Primum de Stella Nova, quæ in Collo Ceti ante annos aliquot visa est. Alterum, de nebulosa in Andromedæ cinguli parte Borea, ante biennium iterum orta."

A review of the book appeared in the first volume of the Philosophical Transactions (p. 381), from which the following account of Bouillaud's conclusions have been taken:—". . . That one *period* from the *greatest phase* to the next consists of 333 days; but that the interval of time betwixt the times of its beginning to appear equal to stars of the *sixth magnitude*, and of its ending to do so consists of about 120 days; and that its *greatest appearance* lasts about fifteen days: all which yet he would have understood with some latitude.

"This done, he proceeds to the investigation of the causes of the vicissitudes in the emersion and disappearance of this star, and having determined that the apparent increase and decrement of every lucid body proceeds *either* from its changed distance from the eye of the observer, *or* from its various site and position in respect of him, whereby the angle of vision is changed, or from the increase or diminution of the bulk of the lucid body itself; and having also demonstrated it impossible that this star should move in a *circle* or in an *ellipsis*, and proved it improbable that it should move in a *strait line*, he concludes that there can be no other genuine, or at least no other more probable cause of the emersion and occultation than this: That the bigger part of that round body is obscure and inconspicuous to us, and its lesser part lucid, the whole body turning about its own center and one axe, whereby for one determinate space of time it exhibits its lucid part to the Earth,[1] for

[1] Poste's translation, p. 13.
[2] Kepler, "De Stella," chap. xii. p. 112.

[1] Here we have the germ of Sir Wm. Herschel's reference to the action of varying amounts of spotted surface; Maupertius' idea of rotatory disks; and Prof. Pickering's suggestion of axes of different lengths.

another, subducts it, it not being likely that fires should be kindled in the body of that star, and that the matter thereof should at certain times take fire and shine, at other times be extinguist upon the consumption of that matter. . . ."

This, so far as I know, is the first proposed explanation of stellar variability on record.

The next star in which variability of light was observed was χ Cygni. Kirch's observations made in 1686 and subsequent years were communicated to the Royal Society. He observed the star with the aid of an eight-foot tube in August 1687. It became visible to the naked eye in October, increased in brightness and reached a maximum in November, and finally disappeared. This observer also found that the star had always the same brightness at a maximum, and in assigning it a period of 404½ days, he noted that this duration was irregular.

These observations bring us to the time of Newton, who at once saw that the cause of true Novæ must be distinct from that producing variability pure and simple. He ascribed the sudden appearance of new stars as possibly due to the appulse of comets :—

"Sic etiam stellæ fixæ, paulatim expirant in lucem et vapores, cometis in ipsas incidentibus refici possunt, et *novo alimento accensæ pro stellis novis haberi.* Hujus generis sunt stellæ fixæ, quæ subito apparent, et sub initio quam maxime splendent, et subinde paulatim evanescunt. Talis fuit stella in cathedra Cassiopeiæ quam Cornelius Gemma octavo Novembris 1572 lustrando illam cœli partem nocte serena minime vidit ; at nocte proxima (Novem. 9) vidit fixis omnibus splendidiorem, et luce sua vix cedentem Veneri. Hanc Tycho Brahæus vidit undecimo ejusdem mensis ubi maxime splenduit ; et ex eo tempore paulatim decrescentem et spatio mensium sexdecim evanescentem observavit."[1]

But with regard to the ordinary variables, he accepts Bouillaud's suggestion, and adds another :—

"Sed fixæ, quæ per vices apparent et evanescunt, quæque paulatim crescunt, et luce sua fixas tertiæ magnitudinis vix unquam superant, videntur esse generis alterius, et revolvendo partem lucidam et partem obscuram per vices ostendere. Vapores autem, qui ex sole et stellis fixis et caudis cometarum oriuntur, incidere possunt per gravitatem suam in atmosphæras planetarum et ibi condensari et converti in aquam et spiritus humidos, et subinde per lentum calorem in sales et sulphura et tincturas et limum et lutum et argillam et arenam et lapides et coralla et substantias alias terrestres paulatim migrare."

Both Montanari in 1669 and Maraldi in 1692 observed that the magnitude of β Persei or Algol was variable.

The information they gave with respect to changes of the star from the second to the fourth magnitude, though important, was not very definite, and it was left to Goodricke, an English astronomer, to discover, in 1782, the periodicity of these variations and to conclude:—(1) "That the star changes from about the second to the fourth magnitude in nearly three hours and a half and then back to the second magnitude again in the same time. (2) That this variation occurs about every two days and twenty-one hours."[2] Flamsteed observed the star in 1696, and found it to be of the third magnitude, and Goodricke, by comparing it with one of his own, deduced the more accurate value of 2 days, 20 hours, 48 minutes, 56 seconds. At the end of the observations Goodricke added the note: —"I should imagine that the cause of this variation could hardly be accounted for otherwise than either by the interposition of a large body revolving round Algol, or some kind of motion of its own whereby part of its body covered with spots or such-like matter is periodically turned towards the earth."

Another variable observed by Goodricke was β Lyræ.

[1] "Principia," p. 525 (Glasgow, 1871).
[2] "Phil. Trans., 1783, p. 474.

His first observations brought him to the conclusion that the star had a periodical variation of nearly six days and nine hours, but a further investigation showed that the true period was twelve days nineteen hours, there being two maxima and minima. At one minimum the magnitude of the star is between four and five, at the other between three and four.

Zöllner, in a relatively recent discussion advances very little beyond the views advocated by Newton. In considering the main causes of variability, he lays the greatest stress upon an advanced stage of cooling, and the consequent formation of scoriæ which float about on the molten mass. Those formed at the poles are driven towards the equator by the centrifugal inertia, and by the increasing rapidity of rotation they are compelled to deviate from their course. These facts, and the meeting which takes place between the molten matter, flowing in an opposite direction, influence the form and position of the cold non-luminous matter, and hence vary the rotational effects, and therefore the luminous or non-luminous appearance of the body to distant observers. This general theory, however, does not exclude other causes, such as, for instance, the sudden illumination of a star by the heat produced by collision of two dark bodies, variability produced by the revolution of a dark body, or by the passage of the light through nebulous light-absorbing masses.

Among modern inquirers Prof. Pickering has been more original in his suggestions. He has shown that the light-curves of some stars may be explained by supposing them to have axes of different lengths, with dark portions at the ends, symmetrically situated as regards the longer axis.

In the following discussion of the cause of variability suggested by the meteoritic hypothesis, I shall divide variability into regular and irregular, defining regularity by constantly recurring maxima and minima on the light-curves.

THE CAUSES OF VARIABILITY SUGGESTED BY THE METEORITIC HYPOTHESIS.

Regular Variability.—All regular variability in the light of cosmical bodies is caused by the revolution of one swarm or body round another (or their common centre of gravity).

In the case of the revolution of one swarm round another an elliptic orbit is assumed, and the light *at maximum* is produced by collisions among the meteorites at periastron.

In the case of the revolution of a swarm round a condensed body, the light *at maximum* is produced by the tidal action set up in the secondary swarm.

In the case of one condensed body revolving round another, the light *at minimum* is caused by an eclipse of one body by the other. This can only happen when the plane of revolution of the secondary body passes very nearly through the earth.

Irregular Variability.—All irregular variability in the light of cosmical bodies is caused (*a*) by the revolution of more than one swarm or body round another (or their common centre of gravity) ; or (*b*) by the interpenetration of meteoritic sheets or streams.

In the case of the revolution of more than one swarm round another in elliptic orbits, the irregular maxima are caused by differences of period and periastric conditions

So far as I know, the only previous explanation of variability on such a basis as the one above stated, which assigns the revolution of one mass round another as a cause of variability, is the one we owe to Newton, who suggested that such stellar variability as we are now considering was due to conflagrations brought about at the maximum by the appulse of comets ; and no doubt his idea would have been more thoroughly considered than it has been hitherto, if for a moment the true nature of the special class of bodies we are now dealing with had been *en évidence.* We know that some of them at their minimum put on a special appearance of their own in that haziness to which I have before referred as having been observed by Mr. Hind. My researches show that they are all nebulæ in a further stage of condensation, and such a disturbance as the one I have suggested would be certain to be competent to increase the luminous radiations of such a congeries to the extent indicated.

Some writers have objected to Newton's hypothesis on the ground that such a conflagration as he pictured could not occur periodically; but this objection I imagine chiefly depended upon the idea that the conflagration brought about by one impact of this kind would be quite sufficient to destroy one or both bodies, and thus put an end to any possibilities of rhythmically recurrent action. It was understood that the body conflagrated was solid like our earth. However valid this objeetion might be as urged against Newton's view, it cannot apply to mine, because in such a swarm as I have suggested, an increase of light to the extent required might easily be produced by the incandescence of a few hundred tons of meteorites.

I have already referred to the fact that the initial species of the stars we are now considering have spectra almost cometary, and this leads us naturally to the view that we may have among them in some cases swarms with double nuclei—incipient double stars, a smaller swarm revolving round the larger condensation, or rather both round their common centre of gravity. In such a condition of things as this, it is obvious that, as before stated, in the swarms having a mean condensation this action is the more likely to take place, for the reason that at first the meteorites are too sparse for many collisions to occur, and that, finally, the outliers of the major swarm are drawn within the orbit of the smaller one, so that it passes clear. The tables, which shall be given hereafter, show that this view is entirely consistent with the facts observed, for the greater number of instances of variability occur in the case of those stars in which, on other grounds, mean spacing seems probable.

I propose here to consider the suggested cause of variability somewhat in detail. I will begin with Groups I. and II.

In these groups the variability is produced by the revolution of one or more smaller swarms round a central swarm, the maximum luminosity occurring at periastron, when the revolving swarms are most involved in the central one.

According to the theory, the normal condition is that which exists at minimum, and in this respect it resembles that suggested by Newton—namely, that the increase of luminosity at maximum was caused by the appulse of comets. All other theories take the maximum as the normal condition, and the minimum as a reduction of the light by some cause—large proportion of spotted surface, or what not.

Anything which in the normal minimum condition of light-equilibrium will increase the amount of incandescent gas and vapour in the interspaces will bring about the appearance of the hydrogen lines and carbon flutings as bright ones. The thing above all things most capable of doing this in a most transcendental fashion is the invasion of one part of the swarm by another one moving with a high velocity. This is exactly what I postulate. The wonderful thing under these circumstances then would be that bright hydrogen and carbon should *not* become more luminous, not only in bright-line stars, but in those the spectrum of which consists of mixed flutings, bright carbon representing the radiation.

We may consider three cases of revolution. Taking that first in order which will give us the greatest light range, we find that this obviously will occur in those systems in which the orbits are most elliptic and the periastric distances least.

On the other hand, a mean ellipticity will give us a mean range.

In these two cases, to account for the greatest difference in luminosity at periastron passage, we have supposed the minor swarm to be only involved in the larger one during a part of its revolution, but we can easily conceive a condition of things in which the orbit is so nearly circular that it is almost entirely involved in a larger swarm. Under these conditions, collisions would occur in every part of the orbit, and they would only be more numerous at periastron in the more condensed central part of the swarm, and it is to this that I ascribe the origin of the phenomena in those objects—a small number—in which the variation of light is very far below the normal range, one or two magnitudes instead of six or seven.

Now it is at once obvious that we should get more variability in these early groups than in any of the more condensed ones, for the reason that in the latter we require the conditions either that the plane of revolution should pass through the earth, or that the light of the central star shall be relatively dim.

This point is best studied in relation to Group II.

The total number of stars included in Argelander's Catalogue, which deals generally with stars down to the ninth magnitude, but in which, however, are many stars between the ninth and tenth, is 324,118. The most complete catalogue of variables (without distinction) that we have has been compiled by Mr. Gore, and published in the Proceedings of the Royal Irish Academy (Series II., vol. iv., No. 2, July 1884, pp. 150-163). I find 191 known variables are given; of these 111 are in the northern hemisphere and 80 in the southern hemisphere.

In the catalogue of *suspected* variable stars given in No. 3 of the same volume (January 1885, pp. 271-310), I find 736 stars, of which 381 are in the northern and 355 in the southern hemisphere. Taking, then, those in the northern hemisphere, both known and suspected, we have the number 492. We have, then, as a rough estimate for the northern heavens one variable to 659 stars taken generally.

The number of objects of Group II. observed by Dunér, and recorded in his admirable memoir, is 297; of these 44 are variable. So that here we pass from 1 in 657 to 1 in 7. Of the great development of variability conditions in this group then there can be, therefore, no question.

Further, while by the hypothesis there is no limit to the increase of luminosity, the variability presented by these objects is remarkable for its great range. The light may be stated in most general terms to vary about six magnitudes—from the sixth to the twelfth. This, I think, is a fair average; sometimes a difference of eight magnitudes has been observed; the small number of cases with a smaller variation I shall refer to afterwards. A variation of six magnitudes means roughly that the variable at its maximum is somewhere about 250 times brighter than at its minimum; a variation of eight magnitudes means that it is 1600 times brighter at maximum than minimum.

These values alone would indicate a condition of things in which the minimum represents the constant condition, and the maximum, one imposed by some cause which produces an excess of light. These various conditions having been premised in considering these groups, I will first deal with the nebulæ.

That many of the nebulæ are variable is well known, though, so far as I am aware, there are no complete records of the spectroscopic result of the variability. But bearing in mind that in some of these bodies, such as the Dumb-bell Nebula, we have the olivine line almost by itself; and in others, which are usually brighter, we have the lines of hydrogen intensified, as in Orion; and in others, more condensed still, the flutings of carbon added, as in Andromeda; it does not seem unreasonable to suppose that any increase of temperature brought about by the increased number of collisions should increase the intensity of the lines of hydrogen or carbon in the spectrum of a nebula.

The observations already accumulated show conclusively that in the nebulæ—even those so far condensed as the one in Andromeda—the temperature is low; in other words, the meteorites are very far apart; regular variability, therefore, would for this reason be very difficult to detect. It is probable, therefore, that in all the cases previously recorded, we are not dealing with the results of rhythmic action, but the interpenetration of nebulous streams or sheets. When, however, we come to the stars—that is, the more condensed nebulæ—in Group I. and Group II., the temperature is higher, the condensation is greater, and the interaction of double or multiple nebulæ can be more easily traced. This fundamental difference of structure between these bodies and stars like the sun should be revealed in the phenomena of variability; that is to say, the variability of the uncondensed swarms should be different in *kind* as well as in degree from that observed in bodies like the sun or α Lyræ, taken as representing highly condensed types.

Since the stars with bright lines, as I have shown, belong to the former group, and since, therefore, they are very akin to nebulæ, we might, reasoning by analogy, suppose that any marked variability in their case also would be accompanied by the coming out of the bright hydrogen lines. This is really exactly what happens both in β Lyræ and in γ Cassiopeiæ. In β Lyræ the appearance of the lines of hydrogen has a period of between six and seven days, and in γ Cassiopeiæ they appear from time to time, although the period has not yet been determined.

Another star of Group I., η Argûs, is also remarkable from the fact that its light varies in the same sort of way. This star is in the southern hemisphere, and during the last twenty or thirty years a considerable discussion has been going on among astronomers as to whether the surrounding nebula is or is not changing its position with regard to the star in question, which has a bright-line spectrum like β Lyræ, and a period not of thirteen days, but of seventy years. The light varies from the sixth to the first magnitude.

Leaving Group I. and coming to Group II., there is one star, Mira Ceti, whose variations in light-intensity may be taken as characteristic. The history of the discovery of this star's variabiltty has already been given. What happens to it in just a little less than a year is this. First, it is of the second magnitude, and then in about eighty days it descends to the tenth, and, so far as observations with ordinary instruments go, it is invisible. In about another hundred days it again becomes visible as a star of the tenth magnitude. It then increases its light to the second magnitude, and begins the story over again. But sometimes at the maximum its brilliancy is not quite constant. That is to say, sometimes it goes nearer the first magnitude than the second. What happens to the light of the star below the tenth magnitude it is not easy to say. What one knows is that to some telescopes it remains invisible for about 140 days or something like that, and then it begins its cycle over again.

I owe to the kindness of Mr. Knott the opportunity of studying several light-curves of "stars" of this group, and they seem to entirely justify the explanation which I have put forward. It is necessary, however, that the curves should be somewhat carefully considered, because in some cases the period of the minimum is extremely small, as if the secondary body scarcely left the atmosphere of the primary one but was always at work. But when we come to examine the shape of the curves more carefully, what we find is that the rise to maximum is extremely rapid: in the case of U Geminorum, for instance, there is a rise of five magnitudes in a day and a half; whereas the fall to minimum is relatively slow. The possible explanation of this is that the rise of the curve gives us the first sudden luminosity due to the collisions of the swarms, while the descent indicates to us the gradual toning down of the disturbance. If it be considered fair to make the descending curve from the maximum exactly symmetrical with the ascending one on the assumption that the immediate effect produced is absolutely instantaneous, then we find in all cases that I have so far studied that the star would continue for a considerable time at its minimum.

Broadly speaking, then, we may say that the variables in this group are *close doubles;* the invisibility of the companion being due to its nearness to the primary or to its faintness.

We now pass from Groups I. and II. to III., IV., and V. These contain the hottest, and therefore brightest bodies in the heavens. They are, moreover, more condensed than those we have considered. On this ground, their normal light cannot be *increased* to any very great extent by any constantly recurring action, but it may be *reduced* by eclipses caused by the revolution of still further condensed secondary swarms. The nearer the primary, and therefore the smaller the period of the secondary body, the more likely is the eclipse to occur regularly. There are several Group IV. stars of this class, notably Algol, to the first observations of which we have already referred.

This body, which is always visible in our latitudes, well illustrates this class of variable. If we take the beginning of a cycle, it is a star of the second magnitude; suddenly in three hours it goes down to the fourth, and then it comes up in another three hours to the second, and goes on again for very nearly three days: and then it goes down again, comes up again, and goes on again for another three days, and so on.

There is another star very like this—a star which is in 81° N. declination, No. 25 in Argelander's Catalogue. The difference between Algol and this is that the rise and fall are a little more rapid. Its light is feeble for about the same time as the other one, but at the bottom the curve is flat, by which I mean that, instead of going suddenly down and coming suddenly up again, it stops at its least luminosity for some little time.

Prof. Pickering[1] has demonstrated by photographs of the spectra of Algol that Goodricke's explanation of its periodical variability is correct, the companion having no light of its own. In the case of the star D. 81° N. 25 there must be luminosity from the star which eclipses the other. And a very beautiful justification of this view has recently been noted, because, although there is no change in the spectrum of Algol, there is a considerable change in the spectrum of the star, the bottom curve of which is flat, showing that probably the companion has an absorbing action of some kind on the light of the central star passing through it or its surroundings. The light practically changes very much as our sunlight would change if it had to pass through the atmosphere of another sun somewhat like itself coming between us.

In Group VI. we again have a new condition. In these stars the light is relatively faint, and the variation is doubtless due to swarms of meteorites moving round a dim or nearly dark body, the maximum occurring at periastron when the tidal action in the swarm is greatest; hence the addition of the light of what we with our solar conditions should term a large comet would make a great difference in the total radiation.

J. NORMAN LOCKYER.

[1] Proc. Amer. Acad. Sci., vol. xvi. p. 17

SOME POINTS IN THE PHYSICS OF GOLF.

IT is not an easy matter to determine the initial speed of a golf-ball:—but this is so only because the direct processes which have given us so much information about the flight of military projectiles are here practically inapplicable. No doubt, a ballistic pendulum, or a Bashforth chronograph, might after long and tantalizing experiment give us the desired information. If they did, they would give it much more accurately than we are otherwise likely to obtain it. But the circumstances of a "drive" at cricket or golf are so uncertain, even with the best of players, that it would be waste of time, and wanton vexation of spirit, to employ these instruments of precision. Yet the questions involved are of a very interesting kind, not only from the purely physical point of view but also in consequence of the recent immense development of these national games; so that there is considerable inducement to attempt at least a rough solution of some of them.

The following investigation, because based mainly on mere eye-observations usually of a rather uncertain and difficult kind, is offered only as a rude attempt at a first approximation; and I am quite prepared to find myself obliged to modify the results, when new and more accurate information is forthcoming.

My main reason for bringing it forward in such a condition is to enlist if possible (at this, the proper season) a few keen and accurate observers, who may occasionally find themselves in a position to obtain data of real value. Thus I shall devote what might otherwise be considered an excessive amount of attention to the nature of the real *desiderata*, and to the quality and the sources of the more common errors of the estimates which have been kindly furnished to me. Such as they were, however, they enabled me to state to the Royal Society of Edinburgh, on July 20, conclusions as to initial speed, and coefficient of resistance, nearly agreeing with those given below.

The influence of even a moderate wind on the flight of a golf-ball is so very considerable that, in the first part of my paper, I shall consider the flight of a golf-ball in a dead calm only, and when it has been driven fair and true without any spin. In a former article (NATURE, Sept. 22, 1887) I have discussed the effects and the causes of spin. Also I shall confine myself to the "carry," as the subsequent motion depends so much upon purely accidental circumstances. By far the most valuable data connected with the subject are those which can be obtained in calm weather alone, and which bear on the form, dimensions, and duration of the first part of the course of the ball. It is mainly due to the excessive rarity of *perfectly* calm days that our knowledge of the data is so slight.

Under these restrictions, it is somewhat curious to find that the extreme carry of a golf-ball is not very different from that of a cricket-ball. Both may be spoken of as somewhere about 200 yards. But the circumstances of propulsion are in general very different:—for, unless it is specially teed on a slope, or driven with a spoon (in which case its initial speed is necessarily reduced), a golf-ball goes off at a very moderate inclination to the horizon:—while the sensational drives at cricket usually have the unquestionable advantage of a much higher trajectory.

Theoretically, the proper position of an observer who wishes to secure at once all the required data should be some miles to one side of the plane of flight, so that he should see the trajectory, as it were, orthogonally projected on a dark background of cloud. The small size of the ball, even if there were not other insuperable difficulties, makes observations in this way impossible. Hence each distinctive feature of the trajectory must be separately studied; and this implies either a staff of observers, or, what is much less easy to obtain, a player who can make a number of successive drives almost exactly "similar and equal" to one another. I am convinced that many of the great incongruities which I have found among the data furnished to me, even by skilled observers, are due mainly to the fact that the measures of different characteristic features had been made on drives essentially different in character from one another.

Another fertile source of error lies in the too common assumption that, because a gentle breeze only is felt by the players, who may possibly be in the lee of a sand-hill, there is nothing beyond a similar breeze at heights of 60 to 100 feet; whereas, at that elevation there may be a pretty strong wind. Unless attention is most carefully paid to this, the estimates of the position of the highest point of the trajectory are sure to be erroneous.

The desiderata which are of real importance; and which must, if possible, be obtained from one and the same drive:—the air being practically motionless:—are

1. The initial inclination to the horizon.
2. The range (on a horizontal plane) of the carry.
3. The maximum height attained.
4. The horizontal distance of this maximum from the tee, expressed (say) as a fraction of the range (2).
5. The time of flight.

To these we may add, though it is of less importance, and also much more difficult to estimate with any approach to accuracy,

6. The final inclination to the horizon.

These data are not independent:—in fact theory (such as it is) shows several relations among them. But, as no one of them, except the second and fifth, admits of accurate determination, it is desirable to measure as many of them as possible; so that they may act as checks on one another.

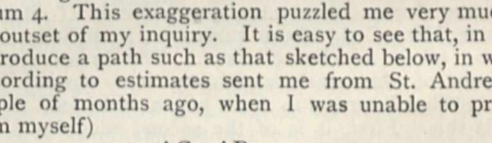
We may also add, what I have recently been endeavouring to obtain:—

7. The horizontal distance passed over in the first second.

This, if properly ascertained, would be one of the most directly useful of the whole set of attainable data.

My experience has been that observers always overestimate the values of the quantities 1, and 6, above:—though they state their ratio fairly well as about 1 : 3. The time of flight, 5, also is usually given too great. But the greatest over-estimate occurs in the case of datum 4. This exaggeration puzzled me very much at the outset of my inquiry. It is easy to see that, in order to produce a path such as that sketched below, in which, (according to estimates sent me from St. Andrews a couple of months ago, when I was unable to procure them myself)

$$AC : AB :: 3 : 4;$$



(where D is the highest point of the trajectory) the initial speed and the resistance must *both* be very great. For clearness, the vertical scale is much exaggerated.

Thus I was led to make some experiments with the view of finding an approximation to the utmost admissible initial speed. This I tried to obtain by measuring the speed of the club at impact, and multiplying by 1·6. A hollow india-rubber shell, of the size of a golf-ball, was teed in front of a horizontal axle on which were fixed, six inches apart, two large pasteboard disks with broad borders of very thin white calico. The ball was teed on a level with the axle, midway between the planes of the disks, and three inches beyond their extreme edges. A stout wire, dipped in black paint, projected from the nose of the club. A drive was then made, in a direction parallel to the axis; first, with the disks at rest; second, when they were revolving about nine times per second. From the result of the first experiment, the correction for the second, due to the fact that the club did not move exactly parallel to the axis, was roughly determined. The results obtained varied within wide limits; *i.e.* from 140 to 700 feet per second for the speed of the club-head at impact. But the majority of the experiments gave from 200 to 300 feet per second. The golfer whose services I enlisted for these experiments, though a very good player, confessed that the novelty of the circumstances had prevented his doing himself justice:—the revolution of the disks, in particular, tending to prevent him from "keeping his eye on the ball." There can be little doubt that the main cause of discrepance among the results was the fact that the correction had to be found when the disks were at rest, and to be applied to data obtained when they were moving. At the time, I formed from these experiments the conclusion that the initial speed of the *ball* must be somewhat over 400 feet per second. I have since been led to believe that this is an under-estimate. I hope when I return to my Laboratory, to carry out this class of experiments with more satisfactory results; by repeating, under favourable conditions, an electrical process which recently failed from the employment of inadequate apparatus.

So long as the speed of a spherical projectile is less than that of sound, it appears that the resistance of the air is at least approximately as the square of the speed. (It is on this account that the effect of even a light head, or following, wind is so considerable. For it is the *relative* speed that determines the resistance, and even a small change in a quantity makes an important change in its square.) Our knowledge of this question is as yet very imperfect; but we cannot fall into any egregious error by making our calculations on the assumption that this law is correct. To apply it, however, we require a numerical datum, *e.g.* the resistance (in terms, say, of the weight of the golf-ball) for unit speed.

Robins, more than a century ago, gave as the result of experiments a statement equivalent to the following:—The terminal speed of an iron sphere in ordinary air is that which it would acquire by falling, *in vacuo*, through a space of $300d$ yards, where d is the diameter in inches.

From this it is easy to calculate that the resistance-acceleration of a golf-ball should be about

$$-\frac{v^2}{400};$$

where v is the speed in feet per second, and the denominator is 400 feet.

In the recent edition of *The Bashforth Chronograph*[1] we find that, for an iron shot whose diameter is d inches, and mass w pounds, the acceleration due to the resistance of the air at speed v (expressed in feet per second) is

$$-\frac{118\cdot3\, d^2}{w} \cdot \frac{v^2}{1000^2}.$$

It is clear that this expression holds for spheres of any material. For the whole resistance depends only on the size and speed, while the acceleration due to it is inversely as the mass. Now for an average golf-ball $d = 1\cdot75$ nearly; and $w = 0\cdot101$, because the specific gravity of gutta-percha is nearly the same as that of water. Hence we may express the acceleration by

$$-\frac{v^2}{280}$$

very nearly:—the denominator being in feet.

I have decided to employ Bashforth's result as probably the more accurate:—my own independent estimate, above alluded to, having given 300 in place of 280. It indicates resistance some 43 per cent. greater than that deduced from the older reckoning of Robins. In the formulæ below we will write a for Bashforth's 280 feet.

For a golf-ball not under the influence of gravity the equation of motion would therefore be

$$\ddot{x} = -\frac{\dot{x}^2}{a};$$

which gives, if V be the speed when $t = 0$,

$$\frac{1}{\dot{x}} - \frac{1}{V} = \frac{t}{a},$$

or

$$\dot{x} = v = \frac{V}{1 + \frac{Vt}{a}}.$$

From this we have

$$x = a \log\left(1 + \frac{Vt}{a}\right),$$

and

$$v = V\epsilon^{-\frac{x}{a}}.$$

Thus in general, as $\epsilon^{-0\cdot7} = \frac{1}{2}$ nearly, the speed, whatever it be, is reduced to half when the ball has moved through 196 feet, or about 65 yards. The time of passage is 280/V.

In treatises on *Dynamics of a Particle* (Tait and Steele, for instance) it is shown that, for the assumed law of resistance, the approximate equation of a flat trajectory is

$$y = \left(\tan\alpha + \frac{ga}{2V_0^2}\right)x - \frac{ga^2}{4V_0^2}\left(\epsilon^{\frac{2x}{a}} - 1\right).$$

In obtaining this result it has been assumed that dx/ds may be treated as being practically unity. This gives a fair approximation to the form of the path of a golf-ball up to, and a little beyond, its highest point; but can scarcely be relied on for the last 30 yards or so of the path, where the inclination to the horizon becomes considerable. But the error will not be a very serious one. If we reject this approximate equation we are forced to use the intrinsic equation of the path, which can be integrated exactly. But, though its use can be made comparatively *simple* by employing a graphic method, it is always very *tedious*, and therefore only to be resorted to in the last extremity; and when we are in possession of data far more exact than any yet obtained. The same may be said, so far as data are concerned, of the elaborate Tables calculated by Bashforth. If we had *accurate* information as to the speed at the highest point of the trajectory, these would give us all that could be desired.

In the above formula V_0 represents the horizontal component of the initial speed:—or, practically, with the limitation introduced, the initial speed itself. α is the angle of projection, and has been carefully determined as on the average about $13^\circ\cdot5$. Its tangent is 0·24. Mr. Hodge, to whose valuable assistance I owe this as well as many of my other data, found it absolutely necessary to use a clinometer, as the eye-estimates of the angle of projection are almost always greatly exaggerated. The only other datum required to complete the equation is an approximate value of V. Two methods of finding it were tried, as follows:—

From a number of (necessarily very rough) observations, made by holding to the ear a watch ticking 4 times per second, it seems that in the first second a well-struck ball goes on an average somewhere about 100 yards.

Hence the initial speed must be about

$$280(\epsilon^{15/14} - 1) = 537 \text{ feet per second.}$$

An error of 1 p.c. in this measurement entails 1·6 p.c. error in the result.

The average time of flight seems to be about 4·5 seconds

[1] Cambridge University Press, 1890. For this reference, and for some much needed explanations, I am indebted to Prof. Greenhill.

for a very good drive. As the length of the path is somewhere about 600 feet, the initial speed must be about 468. This, also, is an exceedingly rough estimate, as the effect of gravity has been omitted. The percentage error here is the same as that of the observed time, but has the opposite sign.

Taking them together, these two estimates appear to indicate an initial speed of about 500. Let us for a moment assume this to be the true value, and see how it will agree with the other facts of the case.

Introducing the assumed data, we have for the typical trajectory

$$y = 0{\cdot}258x - 2{\cdot}524(\epsilon^{x/140} - 1).$$

The value of x for the maximum of y is given by

$$0{\cdot}258 - \frac{2{\cdot}524}{140}\epsilon^{x/140} = 0;$$

so that $x_0 = 372$, and $y_0 = 62$, at the highest point of the trajectory. These values, especially that of y_0, agree very well indeed with those independently observed; so that we have a first hint that our assumptions cannot be much in error.

The range (so far as this approximation goes) is to be found by putting $y = 0$ in the general equation. This leads to

$$14{\cdot}31 = \frac{140}{x}(\epsilon^{\frac{x}{140}} - 1).$$

By the aid of a table of values of the function $(\epsilon^z - 1)/z$, which I constructed for the purpose of this inquiry, I find easily

$$\bar{x} = 140 \times 4{\cdot}08 \text{ nearly} = 571.$$

This, again, is a tolerable approximation to the observed range; and, as above stated, we could not expect more. Now nothing in golf is more striking than the well-known fact that, once a player is able to drive a fairly long ball, he secures comparatively little increase in his range by even a great additional exertion. Assuming that the additional effort is well and truly applied (and this is usually, as most men too well know, a *very* large assumption indeed) its only effect must be to increase the initial speed. Let us see how an increase of initial speed to 600 feet per second will increase the range, other things being the same. Performing the calculations as before, the rough equation for the range becomes

$$20{\cdot}165 = \frac{140}{x}(\epsilon^{\frac{x}{140}} - 1);$$

and x is found to be $140 \times 4{\cdot}51$, nearly, $= 631$ feet, or only about 20 yards more. Yet the initial energy of the ball was 44 per cent. greater. So far as this point is concerned, our result is in good accord with experience. On the other hand, if we assume the initial speed to be 400 feet per second only, we find

$$\bar{x} = 3{\cdot}55 \times 140, \text{ nearly}, = 497.$$

This represents a fair, but not an exceptionally good, drive. It thus appears that our assumption, of an initial speed of about 500, meets adequately the requirements of the data for a really fine drive, so far as yet tested.

The ranges for initial speeds of 100, 200, ... , 600 feet per second are, in order, 112, 277, 400, 497, 571, 631. (Had there been no resistance, the ranges would have been as the square numbers, 1, 4, 9, ... , 36.) From these data it would appear that the great majority of golfers give the ball an initial speed of some 200 to 250 feet per second, only:—very frequently not so much, even off the tee:—and that to obtain a carry of double amount, the ball must have nearly quadruple energy.

We may now apply the test supplied by the datum (4). We have, for initial speed 500,

$$x_0 = 372, \bar{x} = 571,$$

so that, in the figure above,

$$AC : AB :: 372 : 571.$$

The ratio is rather *less* than 2 : 3; whereas according to observation, it ought to have been greater; though, of course, always less than 3 : 4. But I do not attach much importance to this discrepancy, as the estimate made of the highest point of the path is at best a rude one, and depends very much upon the position of the observer. For instance, it is almost impossible for him to make even a guess at its true position if it should happen to be situated nearly above his head.

I have calculated a number of trajectories for larger values of a, and with V correspondingly reduced, so as to keep the carry the same. But all seem to give too great a value for the maximum height attained; and to place that maximum too near the middle of the carry; to suit the long, raking, drives which have furnished my data. The estimated value, 500 feet per second, of the initial speed in "tall" drives like these, may appear a little startling at first. But anyone who knows how to *cut* a tough ragweed with a thin cane, instead of merely bruising it, as ninety-nine men in a hundred would certainly do at the first attempt, will recognize the sort of *nip* which a really skilled golfer gives at the instant of striking the ball.

It is curious to reflect that it is the resistance of the air, alone, which makes it possible for the legislature to tolerate the game of golf. For the normal drive which was studied above would, but for the resistance of the air, have a carry of 1250 yards (more than two-thirds of a mile) and the ball would fall at that distance with its full initial speed of 500 feet per second! The golfer might deal death to victims whom he could not warn with the most Stentorian "Fore." He could carry, at St. Andrews, from the first tee to the "Ginger Beer" hole! This illustrates, though in a very homely and feeble way, the service which the atmosphere is perpetually rendering us by converting into heat the tremendous energy of the innumerable fragments of comets and meteorites which assail the earth from every side with planetary speeds.

When there is a steady wind, even when it blows in the plane of flight, the mathematical problem is much more difficult:—and this difficulty is not sensibly less when an approximate solution only is sought. For the speed of the wind depends on the height above the earth; and, even if we take the simplest law for this dependence, neither of the equations can be treated separately.

It is easy, however, to see the general nature of the effect. In driving against the wind, the resistance (which of course depends on the *relative* velocity) is greater than in still air:—but its direction is no longer in the line of flight, except at the highest point of the path. It acts in a direction less inclined to the horizon than is the path, and therefore its effect on the horizontal component of the velocity, as compared with that on the vertical component, is greater than in still air. With a following wind, unless it be going faster than the ball, the opposite effects are produced. The general result is to affect the carry considerably, and the vertical motion but slightly. The time of flight is probably a little shortened by a following wind, while it is lengthened by a head wind. The belief, prevalent among golfers, that a ball rises higher against a head wind, and lower with a following wind, than it would do in a calm, is due directly to the effect of perspective:—the highest point of the path being shifted nearer to, or further from, the player. The true effects on the greatest height reached are usually too small to be detected by a casual observer.

The diameter of a cricket-ball is nearly 3 inches, and its weight 5·5 oz. The value of a for its motion is therefore 327 feet. Partly on this account, but more on account of its lower speed, a cricket-ball has its path much less affected by resistance than is that of a golf-ball. If we take its maximum initial speed as 130, the initial resistance is only about 1·6 times its weight; while for a golf-ball it rises to about 28-fold its weight. Their momenta are nearly equal, being about 45 and 50 respectively. But their kinetic energy is very different in the two cases, being 90, and 390, foot-pounds respectively. This, again, is in full accord with every-day experience. In the simple vernacular of the cricketer, a well-struck golf-ball would be characterized, at least for the first fifty yards or so of its course, as a "hot" one indeed!

The article may fitly close with a few remarks on another very prevalent fallacy:—viz. the belief that a golfer continues to guide his ball with the club long after it has left the tee. How any player who has ever "jerked" a ball (and who has not?) could maintain such an opinion is an inscrutable mystery. But it is a physical fact, established by actual measurement, that when a block of wood weighing over 5 pounds is let fall on a golf-ball (lying on a stone floor) from a height of 4 feet, the whole duration of the impact is less than 1/250 of a second. When it falls from a greater height the duration of impact is less. But if the elastic force which made the block rebound had been employed to move the golf-ball itself, whose weight is only 1/10 of a pound, (or 1/50 of that of the block) the operation would have occupied only 1/50 of the time; say the 1/12,500 of a second. In the case before us we are dealing with much greater speeds, and therefore with still smaller intervals of time. It is with veritable *instants* like these that we are concerned when driving a golf-ball. The ball has, in fact, left the club behind, before it has been moved through more than a fraction of its diameter.

Another way in which this important point can be made plain to anyone is as follows:—When two bodies impinge, the whole time of the mutual compression is greater than that which would be required to pass over the space of linear compression with the relative speed, but less than twice as great. And the time of recoil is greater than that of approach in the ratio $1 : e$:—where e is the "coefficient of restitution" which, with hard wood and gutta-percha, is about 0·6 when the relative speed is very great. Hence the whole time of impact between the club and the ball is that in which the club, moving at 300 feet per second, would pass through about four times the linear space by which the side of the ball is flattened.

P. G. TAIT.

THE WORKING EFFICIENCY OF SECONDARY CELLS.

UNDER this title a paper was contributed, at the recent meeting of the Institution of Electrical Engineers at Edinburgh, by Prof. Ayrton and Messrs. Lamb, Smith, and Woods, which contains some considerable additions to our knowledge of the subject of secondary cells. The cells on which the tests were made were of the 1888 E.P.S. type, and were charged and discharged at the maximum working currents, these being kept constant in value by hand and automatic regulation. In the most important series of tests the limits of volts employed was 2·4 volts for charge and 1·8 volt for discharge: it was found that a lower limit than this led to detrimental actions in the cells, with loss of active material.

The advantages of a constant current are that it is a nearer approximation to practical working conditions, and that the calculations are much simplified: in fact, the ampere efficiency is got by simply multiplying together the ratio of the charge and discharge currents and the ratio of the times occupied in charging and discharging. The true (or watt) efficiency was found by plotting time readings of the P.D., and taking the ratio of the areas of the curves thus drawn: this, multiplied by the ampere-efficiency, is the required true efficiency.

The first important point brought out in the paper is the importance of the resuscitating power possessed by accumulators. In an early set of tests, made on well-charged cells, the authors found a quantity efficiency of over 100 per cent. with correspondingly abnormal watt efficiency, and this, although the tests occupied five days, from which they conclude that, "if accumulators be well charged up before being tested, five days' continuous alternate charging and discharging with the maximum currents allowed by the manufacturers fails to give the normal working efficiency."

Since these results were so unsatisfactory, some method of avoiding drawing on a previous store had to be adopted. Some experimenters secured this condition by running down a cell, and then leaving it short-circuited for some time. In the present series of experiments the required condition was fulfilled as follows: the cells were continuously charged and discharged with regularity until the successive charges occupied exactly the same time, and successive discharges did also. When the cells arrive at such a "steady state," it can evidently be taken that no drawing on a previous store is taking place. It was, then, under these conditions that the experiments were made.

As such a long series of experiments would entail much labour in keeping the current constant, an automatic regulator was devised to effect this, together with further automatic devices for breaking circuit when the P.D. reached any predetermined value, and for telling the time when such break occurred. The authors state that these apparatuses worked to within ½ per cent. of the supposed limits. Throughout the investigation D'Arsonval instruments were adopted, and by suitably suspending the movable coil, the calibration curve was absolutely a straight line. In these further tests the same instrument was used for measuring volts and amperes, the requisite alteration of circuit being made by a rocking commutator. The volts were read frequently, and curves of P.D. plotted. With this apparatus and measuring instruments, the curves given below for steady state of

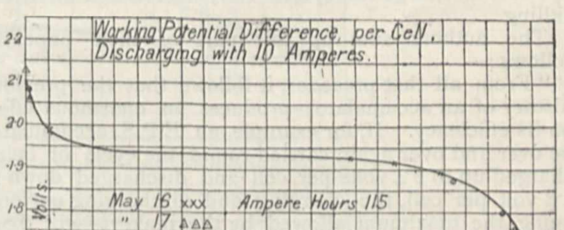


FIG. 1.

charge and discharge between limits of 2·4 and 1·6 volts per cell were obtained.

From these curves efficiencies of 98·3 per cent. for current, and 86·5 per cent. for energy, were obtained.

It was then found that 1·6 volt was far too low a limit to take, as scaling of the plates took place, and so (as mentioned above) limits of 1·8 and 2·4 volts were adopted. After several charges and discharges, the cells arrived at a "steady state" again, the successive times being as follows:—

Discharges. h. m.		Charges. h. m.
10 10	... ...	11 38
10 10	... ...	11 37
10 11	... ...	11 37

"showing to what an absolutely definite state cells arrive after a definite cycle of charge and discharge between fixed limits has been repeated continuously, without interruption, for some weeks." These results give an ampere efficiency of 97·2 per cent. and an energy efficiency of 87·4. These they adopt as the true steady values for this type of cell, and this shows a working storage capacity of 21,380 foot-pounds per pound of plate.

The next point brought out is the effect of rest on a charged cell; the cells were fully charged, and allowed to rest in that state, being well insulated from everything. In every case the first discharge and charge show a marked falling off in capacity and efficiency, the latter being reduced to 58 per cent. in one case cited. A point of some theoretical interest is brought out in connection with the curves obtained in this part of the investigation: a normal discharge curve *falls* rapidly at first, then remains constant, and falls again, as shown in Fig. 1;

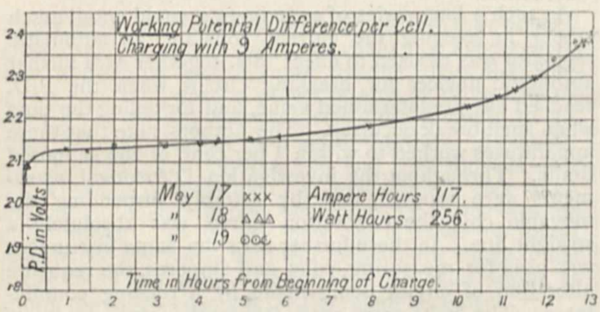


FIG. 2.

the first discharge curve, *after rest*, *rises* at first instead of falling.

The authors sum up this part of their paper as follows:—

"From all that precedes, it follows that the previous history of an accumulator produces an enormous effect on its efficiency. If, for example, an E.P.S. accumulator be over and over again carried round the cycle of being charged up to 2·4 volts per cell and discharged down to 1·8 volt per cell, the charging and discharging currents being the maximum allowed by the makers—viz. 0·026 ampere per square inch in charging, and 0·029 ampere per square inch in discharging—the '*working efficiency*' thus obtained may be 97 per cent. for the ampere-hours and 87 per cent. for the watt-hours. If, on the contrary, the cell be constantly charged up before being tested, then for the first few charges and discharges between the above limits, and with the same current-density in charging and discharging, even the *energy* efficiency may be as high as 93 per cent.; whereas, if the accumulator has been left for some weeks, then, although it was left charged, the energy efficiency for the first few charges and discharges will be as low as 70 per cent.

"While, on the one hand, our tests show that continued rests of a charged accumulator appear to be far more serious for the accumulator than we had previously imagined, the working efficiency appears to be higher than has hitherto been supposed, since we believe that about 84 per cent. efficiency in the watt-hours is all that the advocates of accumulators have claimed for them."

The next section deals with some points connected with the chemical action, and it is shown that the actual amount of SO_3 liberated on charge per ampere-hour, as calculated from the change of specific gravity, agrees well with the ordinary simple formulæ. We understand a further paper on this point may probably be forthcoming later on, which will deal with the chemical changes going on in the plugs at various points in the charge and discharge. As this involves the partial destruction of a cell, and a lengthy series of analyses, it was not found possible to put it in the present paper.

The next point brought out is of considerable interest: during several charges and discharges, the difference of temperature between the working cell and a neighbouring idle cell was observed frequently, and it was noticed that the cell cooled during discharge, in spite of the heat generated by the resistance. This was simultaneously observed by Prof. Duncan in America. The general shape of the temperature curves is given below.

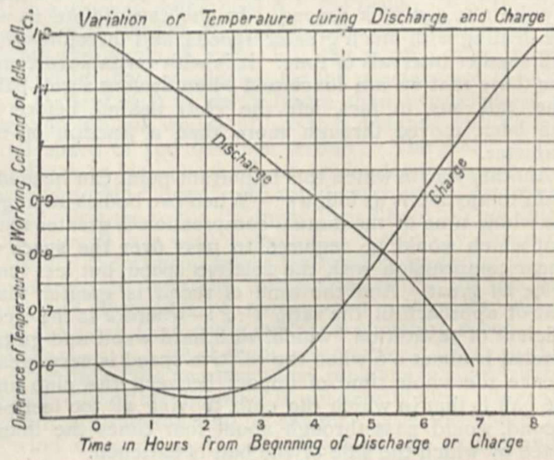


FIG. 3.

From the mean excess of temperature of the cell the authors deduce the somewhat startling fact that 17 per cent. of the energy put into the cell is wasted by radiation and convection. As they found that but 13 per cent. is really lost, it follows that the rest of the energy must be given by some sort of primary battery action, so that they consider an accumulator is partly a reversible and partly an irreversible battery. In this way the gradual deterioration is accounted for. Possibly this may partly account for the short life of small accumulators.

The concluding section deals with the question of the resistance of cells when brought to a steady state. The method adopted is that of introducing an opposing E.M.F. in the voltmeter circuit when time readings of the E.M.F. are being taken on breaking circuit. From these the E.M.F. at breaking circuit is found by producing backwards to zero, and the P.D. being also measured, together with the current before breaking, we can get the resistance at the moment of break. The method is delicate, and seems to have yielded good results; but lack of time has prevented this section being dealt with with other than one set of current values.

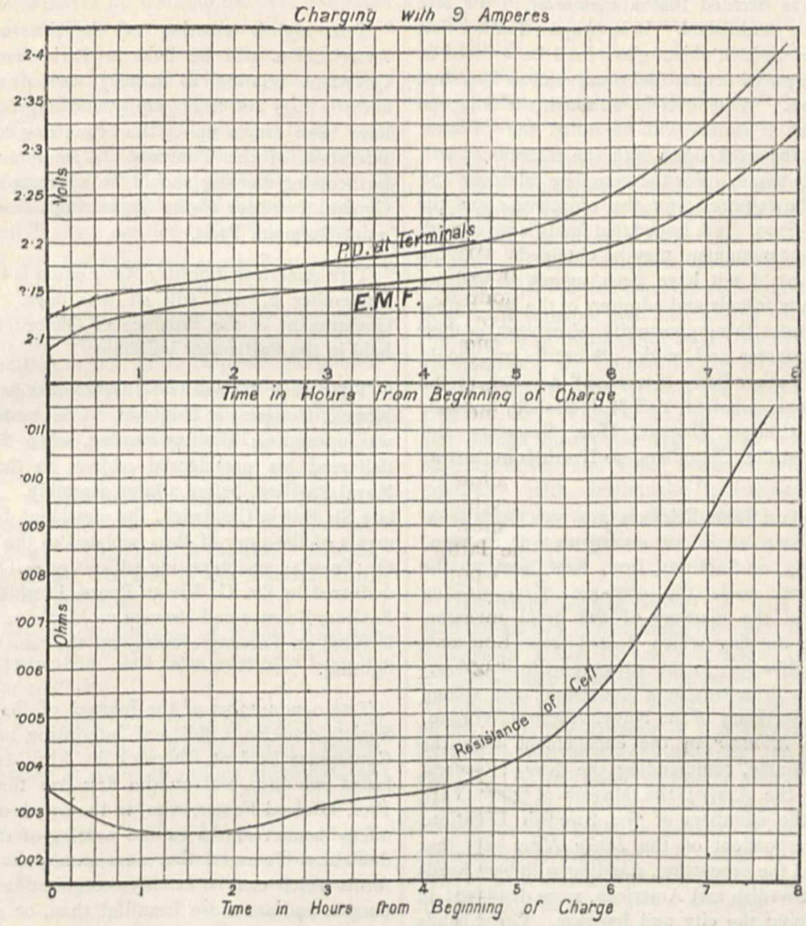


FIG. 4.

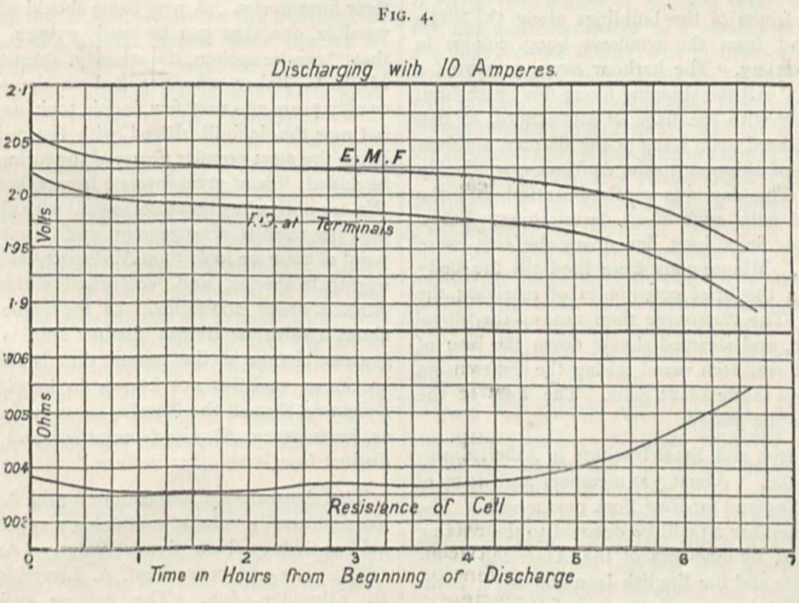


FIG. 5.

We print the curves given in the paper for resistance on charge and discharge.

The paper seems, on the whole, a useful and suggestive contribution to the current knowledge on the subject.

NOTES.

AT a meeting lately held at Stonyhurst College, as we have already recorded, it was decided that a memorial of the late Father Perry should be established. It is proposed either that a new telescope with a 15-inch object-glass shall be erected at Stonyhurst, or that the present equatorial stand shall be furnished with a 15-inch objective. Whichever be adopted, the telescope and the house in which it stands will be called the "Father Perry Memorial," and the work done with the instrument will be published under this name. For the complete telescope and house, £2700 would be required; for the objective alone the sum needed would be £700. It is hoped that funds large enough for the more magnificent monument may be obtained. A more appropriate memorial could not have been suggested, and we have no doubt that many friends and admirers of the late Father Perry will be glad to take this opportunity of expressing their appreciation of his character and work. Subscriptions should be sent either to the "Father Perry Memorial" account, at the London Joint Stock Bank, Limited, Pall Mall Branch, London, S.W., or to Arthur Chilton Thomas, Hon. Secretary and Treasurer, *pro tem.*, Marldon Chambers, 30 North John Street, Liverpool.

THE remains of Captain John Ericsson are now being conveyed across the Atlantic to their last resting-place in Sweden. The transfer of the body, on Saturday, from New York to the war-ship *Baltimore*, was made the occasion of a striking ceremony in honour of the memory of the great inventor. The coffin, wrapped in the flag which floated from Ericsson's famous naval ram, the *Monitor*, in the struggle with the *Merrimac*, was escorted down Broadway by a procession; and among the mourners were the Secretary of the Navy, Admiral Wordon, who commanded the *Monitor* in the engagement with the *Merrimac*; Admiral Braine, commanding the Navy; General Howard, commanding the Army; the Mayors of New York and Brooklyn; and the members of the Swedish Legation. The New York correspondent of the *Daily News* says that dense crowds witnessed the procession, standing with bare heads as it passed. Flags, Swedish and American, were displayed in great profusion throughout the city and harbour. Great bands of streamers festooned fronts of the buildings along the city's main thoroughfare, and from the windows hung colours in endless profusion and variety. The harbour never presented a more charming picture. All the shipping along the water front were dressed for the day with the flags of all nations, at half mast. The body was placed upon a tug at the Battery, and was taken down a long line of shipping to the *Baltimore*, which lay waiting to receive it. The day was rarely beautiful, and the great harbour swarmed with craft of all descriptions. Below the *Baltimore*, stretching in a long line down the bay, were ranged other war-ships. Minute guns were fired till the body reached the ship, when the brief ceremonies of receiving the body were completed. The *Baltimore* then ran up the Royal naval ensign of Sweden, and steamed slowly down the line of battle-ships towards the sea, each vessel raising the same ensign as she passed and firing a salute of 21 guns. The forts at the Narrows also saluted as she passed.

THE meeting of the Iron and Steel Institute in America promises to be very successful. About 300 members and many of their friends will leave England for New York next month. The week beginning on September 29 will be devoted to the reading of papers and discussions by members of the American Institute of Mining Engineers and the English Iron and Steel Institute; and, again, on October 9 and the two following days, these two bodies will co-operate at an international meeting at Pittsburg. Afterwards, excursions will take place to the iron ore and copper regions of Lake Superior and to the new iron-making district of Alabama. The American Reception Committee will provide sleeping and luncheon cars to take their visitors over 3000 miles of the United States.

THE eighth meeting of the International Congress of Americanists will be held in Paris from October 14 to 18. Questions relating to history and geography, archæology, anthropology and ethnography, and linguistics and palæography, have been drawn up by the organizing committee for the consideration of the Congress. Communications regarding the forthcoming meeting should be addressed to M. Désiré Pector, General Secretary of the Organizing Committee, 184 Boulevard Saint-Germain, Paris.

THE American Forestry Association is to meet at Quebec on September 2, and will sit four days. By invitation of the Government of the Province of Quebec, the meetings will be held in the Parliament buildings.

THIS week the Sanitary Institute has been holding its twelfth Annual Congress at Brighton. The business of the Congress was opened on Monday evening, when Sir Thomas Crawford delivered his presidential address in the music-room of the Royal Pavilion, before a large assembly. After a tribute to the late Sir Edwin Chadwick, the president dealt with "some fragments of the story of laws violated to the prejudice of health." On Tuesday an interesting address on "The Living Earth" was delivered by Dr. G. Vivian Poore, President of the Section for Sanitary Science and Preventive Medicine. Mr. W. H. Preece lectured on Tuesday evening on the sanitary aspects of electric lighting.

THE new number of the Journal of the Royal Horticultural Society contains a full and interesting report of the Daffodil Conference held at Chiswick in April last. The Conference lasted two days, and on the first day the chair was taken by Prof. Michael Foster, who, in the course of his opening address, offered some remarks on the naming of different forms of the daffodil. He urged that new names should be given only to forms which can be readily recognized as distinct by ordinary people, and are more beautiful than, or differ in beauty from, their forerunners. A new name should also, he thought, be, if possible, one that can be easily written, and easily read, and that "can be spoken, if not easily, at least without great effort." Mr. J. G. Baker, who presided on the second day, said that twenty years ago very few people took any interest in daffodils, but now the daffodil shared with the primrose the honour of being the most popular flower of the spring-time. "The genus," he added, "is of great interest from a botanical point of view. We are obliged as botanists to deal with all plants on one uniform plan as regards arrangement and nomenclature. From that point of view we look upon *Narcissus pseudo-narcissus* as a single aggregate species, and, comprised within this, there are in gardens about 200 forms. In the whole genus we have only about twelve or sixteen distinct species in this sense. The greatest change at the present time is the raising of forms from species or varieties not known to hybridize before, and it is wonderful that all the Narcissi cross so freely, many of them—as, for instance, *N. pseudo-narcissus* and *N. poeticus*—being so distinct from each other in form."

THE Australasian Association for the Advancement of Science has published a volume containing a report of its first meeting, held at Sydney, New South Wales, in August and September 1888. The editors are Prof. A. Liversidge, F.R.S., and Prof. R. Etheridge, Jun. The volume includes many valuable addresses and papers, and is well illustrated.

THE death is announced of Dr. Felix Liebrecht, an early and highly successful student of folk-lore and comparative mythology.

He was born at Silesia, in 1812, and after studying at Breslau, Munich, and Berlin, became a professor at Liége, where he remained during the greater part of his working life. He translated the "Pentamerone" of Basile from the Italian in 1846, and in the following year issued a version of the romance of Barlaam and Josaphat from the Greek of Johannes Damascenus. In 1851 he translated Dunlop's "History of Fiction." A curious treatise by Gervase of Tilbury attracted his notice as being a sort of encyclopædia of mediæval folk-lore, and he brought out an edition of it in 1856 enriched with many valuable notes. A selection of his contributions to periodical literature on his special subject appeared in 1879, entitled "Zur Volkskunde."

PROF. FLOWER and Mr. Lydekker are engaged in preparing for publication a work entitled "An Introduction to the Study of Mammals, Recent and Extinct." It is based mainly upon the articles contributed by the first-named author and Mr. G. E. Dobson to the ninth edition of the "Encyclopædia Britannica," but much new matter will be added, and the whole brought up to date. The publishers are Messrs. Black, of Edinburgh, and the work is expected to appear before the end of the year.

MESSRS. D. C. HEATH AND CO., Boston, announce the publication of a new number in the series of "Guides for Science Teaching," issued under the auspices of the Bostonian Society of Natural History. The book is entitled "Insecta," and is written by Prof. Hyatt, Curator of the Natural History Society. It is extensively illustrated.

MR. J. B. MARCOU'S "Bibliography of North American Palæontology in the year 1886" has been reprinted from the Smithsonian Report for 1886–87.

THE City and Guilds of London Institute has issued its programme of technological examinations for the session 1890–91. In a special paper attention is called to various alterations and additions.

THE University Correspondence College has published, in its Tutorial Series, a Directory containing all necessary information as to London Intermediate Science and Preliminary Scientific Examinations.

DURING the cruise of the U.S.S. *Thetis* in the Behring Sea and Arctic Ocean, in 1889, several officers were directed to prepare reports on subjects connected with the waters and regions visited by the vessel. One of the reports drawn up in accordance with instructions related to the Eskimos of north-western Alaska, and was written by Mr. John W. Kelly, who had spent three winters among the north-western Eskimos. This report has now been published by the United States Bureau of Education, and all students of ethnography will find in it much that cannot fail to interest them. It is accompanied by English-Eskimo and Eskimo-English vocabularies, prepared by Ensign Roger Wells, chiefly from information furnished by Mr. John W. Kelly. These vocabularies are primarily intended for teachers in Alaska, but it is expected by the Bureau of Education that they will also be of service to officers of the navy and of the revenue marine service, to all Goverment officials in Alaska, to committees of Congress visiting the country, and to many others who for any reason may desire to study the Eskimo language.

THE U.S. Bureau of Education has issued a Bulletin, by Prof. C. F. Smith, of the Vanderbilt University, on "Honorary Degrees as Conferred in American Colleges." The author protests vigorously against the lavish way in which various American institutions raise incompetent persons to the rank of "doctor."

WE have received from the Santiago Observatory (Chile) two volumes comprising the meteorological observations for the years 1882–87. This Observatory, which is furnished with the best instruments, has published observations in the present form since 1873, although they had been taken and partially published for many years previously. The observations for three hours daily are given in a tabular form, and the means for each day are laid down in curves. Full particulars as to the instruments and methods of observation are given in an earlier volume, and the series presents most valuable materials for the study of the climate in those distant parts.

THE Italian Meteorological Office has published its "Annals" for 1886, consisting of four folio volumes. The first volume contains the Report of the Director of this extensive organization, and shows that there were in that year 123 observatories and stations at which complete observations were made, and 630 stations recording temperature and rainfall. This part also contains some valuable memoirs, among which may be mentioned: the climate of Massowah, by P. Tacchini; the comparison of anemometers, and evaporation, by Dr. Ragona; the temperature of snow at different depths, and of the air above it, by Dr. Chistoni, &c. Vols. ii. and iii. contain the ten-day, monthly, and annual means for the various stations, and the results of evaporation and cloud observations. Vol. iv. deals more especially with earthquake phenomena, and contains investigations on several shocks which have occurred in Italy, together with memoirs on the various seismographs used in different countries.

THE Archæological Survey of India perseveres with its unostentatious task of reclaiming from ruin and oblivion the countless inscriptions which lie scattered about India, offering a clue to many questions of ancient history and philology. These despised or neglected records are found in all sorts of likely and unlikely places. The *Englishman* of Calcutta refers to one which has lately been recovered from obscurity, and which is just a thousand years old. It was found incised on a stone slab partly fixed in the wall of a house and used as a seat, in the bazaar of Pahoa or Pihewa, in the Umballa district. Considerable difficulty was experienced in inducing the owner of the house to allow the stone to be removed, but the treasure was eventually acquired, and now lies in the Lahore Museum. The inscription consists of twenty-one stanzas of Sanskrit verse, and is an account of the building and endowment of a temple of Vishnu, together with a eulogy of the family who performed the meritorious deed. Regarding one of the brothers we are told that "when suppliants with rapture looked on his lotus face their mental anxiety completely vanished in an instant; and the crowd of hostile trumpeting elephants always shook before him in battle ready to disperse." This may be taken as a characteristic Oriental rendering of the sentiment of the familiar Scotch song, "His step is first in peaceful hall, His sword in battle keen." For extravagance of laudation, however, a higher place must be given to an inscription found near Jubbulpore, in which it is said of a certain king, that although the tread of his armies roused the apprehension of the three worlds—heaven, earth, and hell—yet there was no dust raised, as the road was flooded by the tears of the captive women who followed in his train.

THE *Photographic News* concludes an interesting article on the photographing of clouds with the following suggestions, which are offered for the benefit of those who have not had much experience in making cloud negatives:—If the sun is to be included in the picture, films or ground-glass backed plates should be used. Any lens which will take a good landscape can be used, and its smallest stop should be employed. As a rule, the exposure will be about one second on a slow plate, but in the case of red sunrises and sunsets, this may often be increased to as much as eight or even ten seconds unless isochromatic plates are available. The development must be very carefully watched, and not carried too far.

COUNTLESS swarms of rats periodically make their appearance in the bush country of the South Island, New Zealand. They invariably come in the spring, and apparently periods of about four years intervene between their visits. In a paper published in the new volume of the Transactions and Proceedings of the New Zealand Institute, Mr. Joseph Rutland brings together some interesting notes on the bush rat (*Mus maorium*). In size and general appearance it differs much from the common brown rat. The average weight of full-grown specimens is about 2 ounces. The fur on the upper portions of the body is dark brown, inclining to black; on the lower portions white or greyish-white. The head is shorter, the snout less sharp, and the countenance less fierce than in the brown species. On the open ground bush rats move comparatively slowly, evidently finding much difficulty in surmounting clods and other impediments; hence they are easily taken and destroyed. In running they do not arch the back as much as the brown rat. This awkwardness on the ground is at once exchanged for extreme activity when they climb trees. These they ascend with the nimbleness of flies, running out to the very extremities of the branches with amazing quickness; hence, when pursued, they invariably make for trees if any are within reach. The instinct which impels them to seek safety by leaving the ground is evidently strong. A rat, on being disturbed by a plough, ran for a while before the moving implement, and then up the horse-reins, which were dragging along the ground. Another peculiarity of these animals is that when suddenly startled or pursued they cry out with fear, thus betraying their whereabouts, an indiscretion of which the common rat is never guilty.

IN a paper recently read before the Vienna Academy, Herren Elster and Geitel give the results of a year and a half's observations of atmospheric electricity on the north side of Wolfenbüttel (bordering an extensive meadow). They used a stand carrying a petroleum flame and connected by insulated wire with an electroscope. A marked difference was found in the phenomena of spring, summer, and autumn, on the one hand, and winter on the other. In the former the daily variation of the fall of potential showed a distinct maximum between 8 and 9 a.m., as Exner found at St. Gilgen, and a distinct minimum between 5 and 6 p.m., whereas Exner found a maximum about 6. In winter there is great irregularity; but a weak minimum occurs about 11 a.m., and a more decided maximum about 7 p.m. It appears to the authors that other factors than humidity, with which Exner seeks to explain the variations, are concerned in the case. When the temperature goes below zero, cold mist being then generally present, there is often rather a sharp rise in the values, the aqueous vapour having then less action. Rainfall in a neighbouring region lowers the fall of potential both in winter and summer, and a disturbance of the normal course will announce a coming change in places still unclouded. Snow, it seems, rather raises the values. It has been shown by Linss that the course of the fall of potential is inversely as the coefficient of dispersion of the air for electricity; which, again, depends not only on the dust and aqueous vapour present, but also, according to Arrhenius's theory, on a sort of electrolytic or dissociative action of the sun's rays on the atmosphere (thus it has been shown that electricity escapes from a conductor under the influence of ultra-violet rays). The authors find their results support this latter view. They consider that the electric processes during formation of precipitates are the chief cause of the disturbance of the normal condition.

THE additions to the Zoological Society's Gardens during the past week include a Rhesus Monkey (*Macacus rhesus*) from India, presented by Miss White; a Common Fox (*Canis vulpes*), British Isles, presented by Mr. H. Fane Gladwin; a —— Fox (*Canis* ——) from Buenos Ayres, presented by Mr. J. R. Bell; a Common Otter (*Lutra vulgaris*), British Isles, presented by Mr. W. Corbet; a Punjab Wild Sheep (*Ovis cycloceros*) from India, presented by Dr. W. King; two European Scops Owls (*Scops gui*) from Austria, presented by Mr. Edward R. Divett; six Prussian Carp (*Carassius vulgaris*), British fresh waters, presented by Mr. G. S. Godden; three Pochards (*Fuligula ferina*), Europe, purchased; a Yak (*Poëphagus grunniens*), a Yellow-footed Rock Kangaroo (*Petrogale xanthopus*), and three Cambayan Turtle-doves (*Turtur senegalensis*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

OBJECTS FOR THE SPECTROSCOPE.

Sidereal Time at Greenwich at 10 p.m. on August 28 = 20h. 28m. 16s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G.C. 4572	—	—	20 17 28	+19 45
(2) P Cygni	Var.	Yellow.	20 13 44	+37 41
(3) D.M. + 17° 4370 ...	7	Reddish-yellow.	20 33 4	+17 53
(4) ε Delphini	4	Yellowish-white.	20 28 0	+10 56
(5) α Delphini	3·5	White.	20 34 30	+15 31
(6) 228 Schj.	7	Reddish-yellow.	19 28 1	−16 34
(7) X Ophiuchi	Var.	Red.	18 33 7	+ 8 44

Remarks.

(1) This is a fine though small planetary nebula, with four minute stars in close proximity. Lord Rosse described it as "a beautiful little spiral." In 1866 Dr. Huggins observed the spectrum of the nebula, and recorded:—"The spectrum of this nebula consisted of one bright nebulous line of the same refrangibility as the brightest of the lines of nitrogen. No other line was certainly seen." It is evident that this observation has an important bearing on the character of the chief line of the nebula spectrum, and it would be well if some other observer would take the trouble to reobserve it. It should be noted also whether the nebulosity is limited to one side of the line, or is equally visible on both sides.

(2) In 1600, this appeared as a bright star, but it has since been comparatively faint. It is especially interesting from the fact that its spectrum contains bright lines, chief amongst these being the lines of hydrogen and D_3. The Henry Draper Memorial photograph, however, shows in addition a very bright line near λ 447, which Mr. Lockyer suggests, from its association with D_3, is Lorenzoni's *f* of the chromosphere spectrum. It will be remembered that this line occurs also in the spectrum of the Orion nebula. Hence, the lines in the visible part of the spectrum which are common to P Cygni and the Orion nebula are hydrogen (F and G), D_3, and 447 (*f*); another bright line in the violet is also common to the photographs of the two spectra. This similarity is evidently in favour of the view that stars with bright line spectra are similar in constitution to nebulæ, though they are probably more condensed. It will be an interesting inquiry to see if P Cygni has anything more in common with nebulæ in the visible spectrum.

(3) According to the records of the spectrum of this star, it is one of the finest of Group II. Dunér calls it superb, all the bands 2–9 being extremely wide and dark. It affords a good opportunity for further observations of the peculiarities of this class of spectrum.

(4) Gothard states the spectrum of this star as ? II.*a*, whilst Vogel writes it I.*a* (Group IV.). My own observations confirm Vogel's statement, the spectrum being almost like that of α Lyræ.

(5) A star of Group IV.

(6) This is a star of Group VI., showing the secondary bands 2, 3, 4, and 5 (all in the red and yellow) in addition to the bands of carbon. The star is not so red as most of the members of the group, and this is no doubt due to the absorption of red light by the secondary bands. Other details should be looked for.

(7) This Group II. star will reach a maximum about September 5, and the appearance of bright lines, as in other variables of the same type, may be expected. The period is about 300 days, and the magnitude changes from 6·8 to 9. A. FOWLER.

OBSERVATIONS OF SATURN AT THE DISAPPEARANCE OF THE RING.—In a memoir "Sur la variabilité des anneaux de Saturne," published in the *Bulletin Astronomique* (vol. ii. p. 28), M. E. L. Trouvelot touches on some interesting phenomena that he observed in 1877–78, before, during, and after the passage of the sun and earth across the plane of Saturn's rings. On May 18, 1877, M. Trouvelot remarked that the illuminated surface of the ring appeared notably less luminous than the planet; further observations confirmed this, and left no doubt that its relative light diminished up to the passage of the sun across its plane. It was also observed that the colour of the light of the ring appeared yellowish and slightly orange when compared with that of the planet, whereas observations made between 1872–76 indicated that the planet was of a yellowish colour when compared with the ring. The two sets of observations are thus diametrically opposed to each other; and it appears that, when the height of the sun above the plane of the ring is reduced to 4° 30′, the surface of the latter gradually diminishes in light with the approach of the sun to the plane, and afterwards the opposite surface increases in light intensity until the angular distance of the sun from the plane of the ring is again 4° 30′. The cause of this diminution and increase is not well known. It may be due to the change in the angle of incidence of the sun's rays, and, therefore, in the amount of light reflected or to the absorption of the sun's rays by the atmosphere belonging to the rings, or to many other causes.

From October 6, 1877, when the sun was 1° 49′ north of the plane of the rings, to February 6, 1878, when the sun crossed the plane, the illuminated surface gradually decreased in width until it appeared as a thin line difficult to recognize, because of its extreme tenuity. It was observed that the decrease in the width of the illuminated ring appeared to be produced by a shadow slowly obscuring it, and M. Trouvelot attributes the shadow to the existence of a zone elevated above the general level of the ring and slightly inclined towards the planet. To produce the observed phenomenon, a protuberant zone on the ring B, and 6000 kilometres from its outer edge, would have to have an elevation of about 400 kilometres above the plane of the rings: that is, if the north and south surfaces are symmetrical, the thickness of the zone would be 800 kilometres. In consequence, however, of the position of the zone on the ring B, and 25,600 kilometres from the edge of A, the better half of it is generally invisible, hence in practice the thickness may be said to be 400 kilometres, or nearly 249 miles.

Prof. A. Hall has a short note on "The Thickness of Saturn's Ring," in the *Astronomical Journal*, No. 222, and develops the equation by means of which it may be determined. He also notes that Dusejour gives a value equivalent to 958 English miles in his "Traité Analytique," t. ii. p. 127 (Paris, 1789), as the result of a discussion of the disappearances and reappearances of the ring observed before 1789. Herschel, by comparing the thickness of the ring with the apparent diameters of the satellites, found the value 856 miles (Phil. Trans., vol. lxxx. pp. 6 and 7, 1790).

Schroeter found the value of 539 English miles from measurements of the width of the trace of the ring on the ball of the planet ("Kronographische Fragmente," pp. 157 and 211, Göttingen, 1808).

W. C. and G. P. Bond by comparing the amount of light received from the surface of the ring a short time before its disappearance with the light received from *the edge of the ring* found the value <43 miles.

With respect to this latter value M. Trouvelot remarks: "Mais Bond, qui ignorait que le système des anneaux de Saturne n'est pas plan, et que c'est à une assez grand distance de son bord extérieur qu'il atteint son maximum d'épaisseur, ne pouvait arriver qu'à une évaluation erronée et trop petite de cette épaisseur."

Several other points are touched upon in M. Trouvelot's memoir, viz. that Cassini's division appeared more visible on the eastern side of the planet than on the western, when the elevation of the sun above the plane of the ring was between +0° 45′ and +0° 27′; and that the edge of Saturn, like that of Jupiter, was notably more luminous than other parts of the globe. The difference in outline between the preceding and following parts of the ring, the deformation of the limb of the planet at different dates, and many observations possible during the disappearance of the ring are also considered.

The memoir concludes with some remarks and suggestions on the observations that should be made during 1891–92. The next disappearance of the ring is on September 22, 1891, and it will reappear on October 30 of the same year. Again, in May 1892, Saturn will be well situated for observations on the structure of the rings causing the shadow noticed in 1877–78. It is to be hoped therefore that the increased powers now at our disposal will enable many of the questions raised by M. Trouvelot to be definitely settled.

OBJECTS HAVING PECULIAR SPECTRA.—In *Astronomische Nachrichten*, No. 2986, Prof. E. C. Pickering, Director of Harvard College Observatory, notes that an examination of photographs taken during March and April 1890, by Mr. S. J. Bailey, near Closica, Peru, has led to the discovery of some interesting spectra.

A photograph of the spectrum of R Carinæ (R.A. 9h. 29·7m., Decl. −62° 21′, 1900) taken on April 4, 1890, shows that the G and *h* lines due to hydrogen are bright, as in Mira Ceti and other variables of long period.

Two photographs taken on March 19 and 20, 1890, of the star, Cordova General Catalogue, No. 15,177, magnitude 8½ (R.A. 11h. 0·8m., Decl. −65° 1′, 1900), show that it has a spectrum consisting mainly of bright lines, and similar to that of Wolf and Rayet's three stars in Cygnus.

The nebula, General Catalogue, No. 2581 (R.A. 11h. 45·1m., Decl. −56° 29′, 1900) has the same spectrum as General Catalogue 4628. Both these objects show bright lines in the ultra-violet portion of their spectra, which have not been discovered in any other planetary nebulæ.

D.M. +30° 3699, magnitude 9·3 (R.A. 19h. 31·9m., Decl. +30° 19′, 1900), is seen to have bright lines in its spectrum on photographs taken at Cambridge with the 8-inch Draper telescope on June 18, 23, and 25, 1890. The spectrum of this star differs from that of other bright-line spectra of which photographs have been obtained.

ON THE CAPTURE OF YOUNG (IMMATURE) FISHES, AND WHAT CONSTITUTES AN IMMATURE FISH.

SINCE steam-trawling became prominent, frequent complaints of the constant and great destruction of very young fishes by this mode of fishing have been made; indeed, besides the injury to the so-called eggs of food-fishes—then said to be deposited on the bottom—no subject attracted more attention in the Royal Commission of 1883–84—presided over by Lord Dalhousie. Recently the subject has again been urged before the National Sea Fisheries Protection Association—especially by the fish-merchants of London (on the alleged grounds of the diminution in size of the valuable food-fishes)—and, with the assistance of the Board of Trade, an International Conference, to discuss remedial measures "to be taken for the preservation and development of the fisheries in the extra-territorial waters of Europe," was convened in the Fishmongers' Hall. It would be a misapprehension, however, to suppose that those who attended the Conference confined their attention to extra-territorial waters, since the inshore ground (within the three-mile limit) is really more important, *e.g.* in regard to the preservation of certain flat fishes, than the offshore. Thus, as formerly shown, the plaice for the most part passes its early life in the shallow sandy bays of the inshore, and as it attains a length of about fifteen inches it in most cases frequents the deeper water offshore, where it chiefly spawns, the pelagic ova and larvæ being carried shorewards to repeat the process. In the same way multitudes of small turbot, brill, and soles pass their early existence not far from low-water mark on sandy beaches; ling in the barred condition amongst rocks at extreme low-water; and cod, coalfish, pollack, and whiting, near the same regions. Remedial measures therefore, applied, for instance, to the plaice in extra-territorial waters, could only affect the adult or nearly adult fishes, and mainly in regard to the spawning individuals, a point no doubt of vital importance, but which nevertheless does not touch the question before us, viz. the young or immature fishes.

In most modes of fishing as at present followed, young or immature fishes are captured. Thus, in line-fishing a considerable number are hooked throughout the year, and in certain parts of the east coast many in September and October. When we consider the large number of men engaged in line-fishing, and the almost constant nature of such captures, we cannot conscientiously overlook it. The drain on the young cod, haddock, whiting, ling, and other fishes is a steady one, and though many are replaced in the sea by the fishermen it is doubtful if they will survive. The smaller of the first three, indeed, are generally dead when brought on board. The use of the hook, on the other hand, for the capture of flat fishes—more particularly plaice in sandy bays—contrasts favourably with the work of sailing trawlers on the same ground, since a larger size of fish on the whole is secured; though scarcely a single fish thus obtained is mature. It is probable that the smaller mouth of the pleuronectids prevents the younger forms from so readily taking the hook, the size of which, moreover, would appear to be related to that of the young fishes captured. The liners themselves as a rule apply the remedy, since they leave the ground frequented by small fishes, *e.g.* haddocks, and seek more mature forms. They appear to be aware that these young fishes haunt the same area a considerable time. This practice cannot be too much commended.

Beam-trawling and otter-trawling, again, are credited with the capture of many young (immature) fishes. In the case of the beam-trawl, now so extensively used, if the meshes of the net be small and work carried on inshore, or where multitudes of young fishes are, large numbers especially of flat fishes are taken. In ordinary steam-trawling for profit, as observed off the east coast in 1884, however, the actual captures of small (immature) fishes were not as a rule serious. For the most part they consisted of common and long rough dabs, neither of which when adult is a large fish, though both, besides other uses, form an important item in the diet of the more valuable fishes. Off Girdleness (Aberdeenshire), however, a considerable number of young cod were captured in autumn, yet every one of these was used as food and was saleable. In the open offshore water very few young plaice are procured, almost all being of considerable size; but in inshore water, *e.g.* in such bays as St. Andrews, vast numbers of small plaice may be captured with a naturalist's trawl (*i.e.* one with a small mesh), and considerable numbers with the ordinary trawl of either sailing or steam trawler, one of the latter in 1884 having about sixty boxes as its catch. Though the very young plaice are abundant at the tidal margin, yet no graduated lines, indicating an increase in size as we proceed outwards, seem to occur, very small forms being found in the outer lines of St. Andrews Bay as well as those approaching low-water mark.[1]

In steam-trawling for profit, the condition of the captured young fishes depends on the length of time the trawl has been down, the state of the sea, and the condition of the bottom. Thus, if the trawl has been at work for five hours the younger fishes are often dead, and, if not, would probably die if replaced in the water, whereas when the trawl of a sailing boat has been down only an hour the majority would probably live if returned to the sea. If the sea be rough, the pitching of the vessel in hauling causes the bag of the net and its load of fishes to strike the side of the ship, and thus the snouts of the fishes are broken and many killed. In the same way soft muddy ground is fatal to the fishes in the trawl, just as in a less degree, the soft sand of the beach proves destructive to trout swept down by a spate.

Shrimp-trawling is another method of fishing proportionally more destructive to young fishes than perhaps any other. As carried on, for instance, in the estuary of the Thames by sailing boats near Canvey Island and towards Tilbury Fort, multitudes of small soles, dabs, plaice, bib, whiting, and other forms, *e.g.* unctuous suckers and *Cotti*, are retained by the small-meshed net, and before the sifting of the shrimps is concluded the majority have succumbed. Nor are hand-nets and the larger ones drawn by horses less destructive. All cause a frequent and great drain on the young fishes, especially in some places on such valuable forms as soles, turbot, and brill, while the food procured for the public is small in comparison with the loss of fish-food. There should be no insuperable obstacle to the immediate substitution of these methods by others less wasteful to fish-life. The French shrimp-trap, for instance, as recommended by Prof. Giard and M. Roussin, is a step in the right direction.

The use of the "sweep-net" on sandy shores for procuring sand-eels is followed by the capture of numerous young cod, green cod, gurnards, whiting, trout, turbot, brill, dabs, plaice, flounders, and other forms. The net has wings of 4-inch mesh, and a centre of strong netted curtain-gauze, so that small fishes are secured in hundreds. The net is worked by two men, one in a boat, the other on shore, and is especially destructive in estuaries. The little fishes thus captured escape the trawls of both sailing and steam-vessels.

The salmon-stake nets, on sandy beaches, secure many small turbot and brill from $5\frac{1}{2}$ inches upwards.

The stow or bag-net for sprats, as used by yawls in estuaries of rivers, is a small-meshed net of great length, fixed to the side of the vessel by the upper beam, and into which immense numbers of young herrings and sprats, and sometimes many sparlings, are swept by the current, besides various round food-fishes, flat fishes, and unsaleable forms, such as *Cotti*, Montagu's suckers, and pipe-fishes, not to allude to an occasional salmon. The captured fishes are now and then used for manure, and much valuable food is thus lost to the community.

The small-meshed sprat-nets (pole-nets) of the Forth are also responsible for great captures of small herrings and sprats for manure, as well as for the destruction of young round fishes, such as cod and whiting. The capture of whitebait in the Thames is another instance of the wholesale destruction of very young fishes.

From the foregoing brief sketch it will be apparent that no special kind of fishing is responsible for the capture of small (immature) fishes, and that legislative measures, to be effectual, must, more or less, cover all. The question, therefore, is beset with difficulties. The prohibition of the landing and sale of such fishes would, of course, shut them out of the market, but it would not prevent their being captured; and while they might be returned to the water as soon as practicable, the mortality, as already indicated, would be considerable. It is difficult to see how, by any modification of apparatus, these small fishes would be enabled to escape capture by liners, trawlers, shrimpers, seine, and other net-fishermen. As recommended to the Trawling Commission of 1884, the mesh of the trawl-net might be enlarged. Thus, for 9 feet at the cod-end, it might have a 2-inch mesh; then, for 12 feet, $2\frac{1}{2}$-inch mesh; next, 3 and $3\frac{1}{2}$-inch mesh; and, finally, a 5-inch mesh towards the beam. The enlargement of the mesh will not altogether prevent the loss of young fishes, but it will diminish it. Moreover, a limit to the time the trawl is down might be considered. The pressure of the larger on the smaller fishes when the bag of the net is hoisted by the derrick, and the swinging of the heavily-laden bag on the side of the ship in rough weather, however, are elements of disaster apparently beyond control at present. If the bag of the net with its fishes could be lifted horizontally into a raft or other apparatus level with the water, much injury to the contents, both young and adult, would be avoided; but the practical difficulties are great. In the other modes of fishing in which young fishes are captured in great numbers, and where restrictive measures are inapplicable, the obstacles would seem to be best met by the modification of apparatus and by the trained intelligence of the master-fisherman.

The question as to what constitutes an immature fish has not hitherto, perhaps, received that strict attention which it merits. In the trawling investigations of 1884 the term "immature" was not used in the strictly scientific sense—that is, in connection with the reproductive organs, though these were examined in all the species. The term, indeed, was purposely employed as nearly synonymous with unsaleable. Recently, Dr. Fulton, the energetic Scientific Secretary to the Fishery Board for Scotland, has had a large number of fishes examined—chiefly by Mr. T. Scott, on board the *Garland*—their sizes and the condition of the reproductive organs carefully noted, and the results, as elaborated by him, are given in a paper about to be issued by the Fishery Board in their Blue-book for 1890.[1] The paper is one of very great interest, and there can be little doubt that the term "immature" ought to be restricted to fishes that have never spawned; and it may thus happen that such may be saleable, *e.g.* in the case of the plaice, brill, turbot, cod, and others. On the other hand, mature food-fishes may be unsaleable from their small size, as in the case of the flounder, dab, and long rough dab, though, as already mentioned, these are important as the food of some of our most valuable fishes. As given by Dr. Fulton the smallest ripe food-fishes procured in the *Garland's* trawl were as follows:—Plaice 12 inches, lemon-dab 8 inches, dab 6 inches, long rough dab 6 inches, flounder 7 inches, craig-fluke (witch) 14 inches, turbot 18 inches, brill 16 inches, sail-fluke 9 inches, haddock 10 inches, whiting 8 inches, cod 20 inches, gurnard 8 inches, and catfish

[1] This appears to differ from the results of the *Garland's* recent work.

[1] I have to thank Dr. Fulton for an early proof, issued, by the sanction of the Secretary for Scotland and the Fishery Board, in connection with the International Conference.

(*Anarrhichas*) 20 inches (?). In most cases these small specimens were males, as in the even more remarkable case of the salmon —in which the milt of a parr a few inches long can be utilized for the successful fertilization of the ova of an adult female salmon. There would therefore be grounds for saying that fishes of a less size than the foregoing are immature. From these observations it will be seen that the minimum size of 12 inches for turbot and brill—adopted by the representatives of the sea-fishing industry of the United Kingdom in June of this year—does not err on the side of excess. Further, since the mature males are often so much smaller than the females, it is apparent that the same restrictive size would not be practicable, though the numbers of the mature females are of greater importance for the welfare of the fisheries than those of the males.

While, therefore, many difficulties beset legislative measures for the preservation of the young fishes, there need be no halt in the efforts of the fishery authorities in investigating the deep-sea fishing grounds far from shore; and this should be carried out as far as practicable and as frequently as possible every month of the year. A comparison of the surface, mid-water, and bottom fauna there with that already known to exist in such bays as St. Andrews could not fail to give valuable and interesting data. Besides, the gaps in the life-histories of the post-larval and young stages of many fishes would thus be more or less bridged over. Finally, there can be little doubt of the expediency of at once providing suitable open-air tanks, *e.g.* at St. Andrews, in communication with the tidal water for the study of the post-larval and young stages of food-fishes, especially with regard to their rate of growth. It has yet to be proved also whether it would be best to place the larvæ of valuable fishes, such as turbot, brill, and soles, in the sea, or to keep them till the post-larval or young stages are reached.

W. C. McINTOSH.

ESTABLISHMENT OF SCIENCE SCHOLARSHIPS.

WE have already called attention to the science scholarships which are being established by the Commissioners for the Exhibition of 1851. The official statement on the subject is as follows:—

In their seventh report to the Crown, presented in July 1889, the Commissioners for the Exhibition of 1851 announced their intention of appropriating an annual sum of not less than £5000 a year to the establishment of scholarships, to enable the most promising students in provincial colleges of science to complete their studies either in those colleges or in the larger institutions of the metropolis, care being taken that these scholarships should be a supplement to, and not in competition with, scholarships already existing through either public action or private endowment.

The decision to restrict the scheme of scholarships to provincial colleges was due to the feeling of the Commissioners that the provinces, which took so large a part in supporting the Great Exhibition of 1851, had a just claim to receive as direct a benefit from the funds derived from that Exhibition as is afforded to the institutions on the Commissioners' Estate at South Kensington, which, although unquestionably of national importance, confer a more immediate advantage on the metropolis.

To assist them in preparing a scheme for the distribution and regulation of the scholarships' the Commissioners obtained the services of a committee of eminent men of science—namely, Prof. Garnett, Prof. Huxley, Prof. Norman Lockyer, Sir Henry Roscoe, and Sir William Thomson. To these were added two Commissioners, Mr. Mundella and Sir Lyon Playfair, the latter of whom acted as chairman of the committee.

On the 18th of June last this committee presented the accompanying report on the scope and objects of the scholarships, and it has been adopted by the Commissioners.

The committee then considered the manner in which the scholarships should be distributed. On this point they were bound by the restriction of the present scheme to students in provincial institutions, in which term, however, they suggested that colonial Universities might be comprised. They thought it unnecessary to include in the scheme the Universities of Oxford, Cambridge, and Dublin, in view of the large endowments of those bodies. The committee decided upon the allotment of an annual series of seventeen scholarships in the manner shown by the accompanying list, and the institutions named in the list will be invited to nominate scholars, subject to the conditions laid down in the report of the committee, and provided that they possess scholars worthy of the purposes explained in it.

The present allotment is to be considered experimental and temporary, and the selection now made of institutions to which nominations are offered will be subject to modification in the future, having regard not only to the manner in which the nominations are exercised, but also to the claims of other Universities and colleges which may from time to time be brought under the consideration of the Commissioners.

Provincial and Colonial Universities and Colleges.

Scholarships Annually.		
	1	University of Edinburgh.
	1	University of Glasgow.
Alternately .	1	University of St. Andrews (including University College, Dundee). University of Aberdeen.
	1	Mason College of Science, Birmingham.
	1	Bristol University College.
	1	Durham College of Science, Newcastle.
	1	Yorkshire College of Science, Leeds.
	1	Liverpool University College.
	1	Owens College, Manchester.
	1	Nottingham University College.
	1	Firth College, Sheffield.
Alternately .	1	Aberystwith (University College of Wales). Bangor (University College of North Wales). Cardiff (University College of South Wales).
In each year	2	Belfast, Queen's College. Cork, Queen's College. Galway, Queen's College. Dublin, Royal College of Science.
		Canada:—
Alternately .	1	M'Gill College, Montreal. University of Toronto.
		Australia:—
In each year	2	University of Sydney. University of Melbourne. University of Adelaide. University of New Zealand.
	17	

The following is the first report of the committee for considering the regulation and distribution of the science scholarships:—

The committee have had their attention drawn to the fact that there is a large number of scholarships in the country; that they are increasing at a rapid rate; and, if the Commissioners act on the same lines as those already occupied, it is possible that education will gain little by their action, as the endowment of the Commissioners may interfere with the establishment of new scholarships by private liberality.

Hence it is desirable that the scholarships with which this committee have to deal should be of a higher order than most of those now existing; in fact, their functions should begin where the ordinary educational curriculum ends. This system has been adopted with excellent effects by the French *École pratique des hautes études.*

The committee propose:—(1) That the scholarships shall be of £150 a year in value, and shall be tenable for two years, but in rare instances may be extended to three years by special resolution of the Commissioners. The continuation, each year after the first, shall depend upon the work done in the previous year being satisfactory to the scientific committee which it is suggested shall be appointed by the Commissioners.

(2) That the scholarships shall be limited to those branches of science (such as physics, mechanics, and chemistry) the extension of which is specially important for our national industries.

(3) That the Commissioners shall from time to time select a certain number of provincial and colonial colleges in which special attention is given to scientific education, and give to each

the power of nominating a student of not less than three years' standing to a scholarship, on the condition that he indicates high promise of capacity for advancing science or its applications.

(4) That the Commissioners shall appoint a committee of advice, who will consider and report upon the reasons for which the nominations are made by the respective colleges, and the Commissioners will appoint to the scholarships upon the report of their committee.

(5) That the scholarships when awarded shall be tenable in any University either at home or abroad, or in some other institution to be approved of by the Commissioners. The holder of a scholarship must give an undertaking that he will wholly devote himself to the object of the scholarship, and that he will not hold any position of emolument during its continuance.

LYON PLAYFAIR, Chairman.
WM. GARNETT.
T. H. HUXLEY.
J. NORMAN LOCKYER.
A. J. MUNDELLA.
HENRY E. ROSCOE.
WILLIAM THOMSON.

SCIENTIFIC SERIALS.

American Journal of Science, August 1890.—On the cheapest form of light, from studies at the Allegheny Observatory, by S. P. Langley and F. W. Very. The authors have made a long and interesting series of observations, by means of the bolometer and spectroscope, on the light radiated by the firefly (*Pyrophorus noctilucus*, Linn.) found in Cuba and elsewhere. It has been previously shown that in all industrial methods of producing light, like the candle, lamp, or gas, more than 99 per cent. of the energy is, as far as illumination goes, wasted; and in sources of higher temperature, like the incandescent light and electric arc, even under the most favourable conditions, an enormous waste is involved. The study of the radiation of the fire-fly demonstrates that it is possible to produce light without heat other than in the light itself; that this is actually effected by nature's processes; and that these are "cheaper," that is, more economical in energy, than any industrial method now known. From the observations and facts given there seems no reason why the light should not one day be produced in the laboratory, and used for industrial purposes.—Contributions to mineralogy, No. 48, by F. A. Genth. Analyses are given of the following minerals: tetradymite, pyrite, quartz (pseudomorphous after stibnite), gold in chromiferous clay from Los Cerillos, New Mexico, zircon, scapolite, garnet, titaniferous garnet, allanite, and lettsomite from Arizona and Utah.—A curious occurrence of vivianite, by Wm. L. Dudley.—Classification of the glacial sediments of Maine, by George H. Stone.—The direct determination of bromine in mixtures of alkaline bromides and iodides, by F. A. Gooch and J. R. Ensign. The method described is as follows: the neutral solution containing the bromide and iodine is diluted to 600 c.c. or 700 c.c., and about 1 c.c. or 1·5 c.c. of strong sulphuric acid, or from 2 c.c. to 3 c.c. of the acid mixed with an equal volume of water, are added; a sufficient amount of sodium or potassium nitrite is then introduced, and the liquid is boiled until the colour has disappeared and the escaping steam no longer gives to red litmus-paper the characteristic colour of iodine. The residual liquid is then treated with excess of silver nitrate, and the precipitated bromide filtered off, dried, and weighed.—Some Lower Silurian graptolites from Northern Maine, by W. W. Dodge.—Siderite-basins of the Hudson River epoch, by James P. Kimball. Some interesting facts bearing on the structural geology of the Taconic area extending to the Hudson River, and on the geology of the whole Taconic region, are brought together and discussed.—On a new variety of zinc sulphide from Cherokee County, Kansas, by James D. Robertson.—Two new meteoric irons, by F. P. Venable. An analysis of a meteorite from Rockingham County, N.C., gave the result: Fe, 87·01; P, 0·04; SiO_2, 0·53; Cl, 0·39; Ni, 11·69; Co, 0·79 = 100·45. Another meteoric iron from Henry County, Vancouver, gave: Fe, 90·54; Cl, 0·35; SiO_2, 0·04; P, 0·13; Co, 0·94; Ni, 7·70 = 99·70.—Notice of some extinct Testudinata, by O. C. Marsh.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, August 18.—M. Duchartre in the chair.—Contribution to the theory of the experiments of M. Hertz, by M. H. Poincaré. After pointing out an error in the calculations of M. Hertz, an attempt is made, starting with Maxwell's fundamental hypotheses, to develop a more rigorous formula for the rate of propagation of the wave in air.—International meteorological tables, presented by M. Mascart. These are of the form finally decided upon by the International Committee at Zurich, 1888.—Order of appearance of the first vessels in the flowers of some *Tragopogon* and *Scorzonera*, by M. A. Trécul.—Experimental tuberculosis, on a mode of treatment and of vaccination, by MM. J. Grancher and H. Martin. The paper describes the result of some experiments on the inoculation of rabbits. The process described affords a more or less complete protection from tuberculosis to the rabbits inoculated.—On a portable electrical safety-lamp, for use in mines, by M. G. Trouvé. The smallest lamp described, supplied with six Planté accumulators (weight 420 grammes), runs for five hours.—Note on a theorem concerning life annuities, by M. A. Quiquet.—Experiments on transversal magnetization by magnets, by M. C. Decharme.—On an electric lighting-apparatus, for examining the strata in borings, by M. G. Trouvé.—Allyl-cyano-succinic ether; new synthesis by means of cyano-succinic ether, by M. L. Barthe. In this synthesis sodium-cyano-succinic ether is treated in alcoholic solution with allyl iodide. The new ether forms a colourless oil, and distils under 35 mm. pressure at 207°–210°.—Methyl cyano-succinate, and methyl cyano-tri-carballylate, by M. L. Barthe. Both these ethers are produced when sodium-methyl cyanacetate is treated with methyl chloracetate.—Researches on butter and margarine, by M. C. Viollette. This paper contains results of ten complete analyses. By the method given an adulteration of pure butter with ten per cent. of margarine can be detected.—Researches on the optical analysis of butters, by the same. The differences between the values of the refractive indices for pure butter and for margarine are sufficient to serve as the basis of an analytical method.—On a characteristic reaction of cocaine, by M. F. da Silva.

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