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.

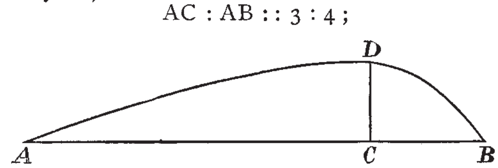
We may also add, what I have recently been endeavouring to obtain:—

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 over-estimate 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 300*d* 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

$$-\frac{118 \cdot 3\, d^2}{w} \cdot \frac{v^2}{1000^2}.$$

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 $d = 1 \cdot 75$ nearly; and $w = 0 \cdot 101$, 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 $\epsilon^{-0 \cdot 7} = \frac{1}{2}$ 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

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

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°·5. 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 $\bar{x}$ 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

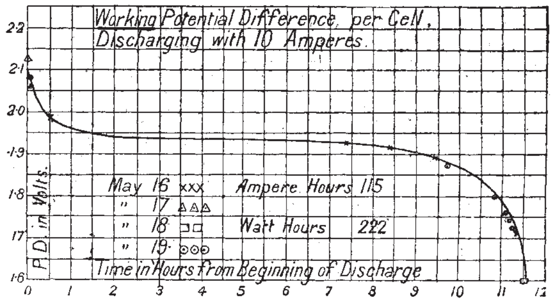


FIG. 1.

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.