the life-blood of their College to-day, the source of their vitality, without whom they would have really little cause for existence.

Mr. Keeble, Natural Science Scholar of the College, made a short and graceful reply.

At the conclusion of dinner a move was made to the Combination Room, where friendly and animated intercourse was kept up for some time, and it was late before the last of those engaged in the celebration separated for the night.

Breakfast was provided the following morning from eight to ten for those resident in College overnight, and by midday the guests had departed, leaving the courts once more to solitude, and to their hosts a keen feeling of satisfaction at the honour done to the memory of William Harvey and to the College by the recent presence of so representative and distinguished a gathering of visitors.

SOME POINTS IN THE PHYSICS OF GOLF.[1]

III.

IN Part II of this paper (NATURE, Sept. 24, 1891) the following statements were made:—

"The only way . . . of reconciling the results of calculation with the observed data is to assume that, for some reason, the effects of gravity are at least partially counteracted. This, in still air, can only be a rotation due to undercutting."

"And, as a practical deduction from these principles, it would appear that, to secure the longest possible carry, the ball should be struck so as to take on considerable spin———."

As these statements, and some of their consequences, have been strenuously denied, I must once more show at least the nature of the evidence for them.

It depends, in one of its most telling forms, upon the contrast between the length of time a well-driven ball remains in the air (as if in defiance of gravity) and the comparatively paltry distance traversed. Every one who thinks at all on the subject must see that, *without some species of support*, the ball could not pursue for six seconds and a half a course of a mere 180 yards, nowhere more than 100 feet above the ground.

In fact, if we assume the initial slope of the path to be 1 in 4, as determined for the average of fine drives by Mr. Hodge with his clinometer (NATURE, Aug. 28, 1890) the carry of a non-rotating ball will be approximately (in feet)

$$AgT^2,$$

where g is the acceleration due to gravity, T the time of flight in seconds, and A a numerical quantity depending on the resistance. The value of A varies continuously between the limits, 2 for no resistance, and 1 for infinitely great resistance. [It is assumed that the resistance is as the square of the speed.]

This formula gives, with the average observed value of T ($6^s \cdot 5$, see Part II.) carries varying from about 900 down to 450 *yards!* The initial speed required varies from 416 foot-seconds upwards. The longest actually measured carry on record, when there was no wind, is only 250 yards. Unfortunately, in that case T was not observed, but analogy shows that it was probably much more than 7^s. Even if we take it as 7^s only, the "carry" ought to have been, by the formula (which is based on the absence of rotation), 522 yards at the very least!

I have purposely, in this example, kept to the case of an initial slope of 1 in 4; because those (and they are many, some of them excellent golfers) who altogether reject the notion that undercutting lengthens the carry, would of course in consistency refuse to believe that a long ball may sometimes start horizontally. But, to those who allow *this* statement, the fact that the action of gravity is occasionally largely interfered with, or even counteracted, is obvious without any numerical calculations. In fact, from my present point of view, initial slope is of little importance:—except, of course, in avoiding hazards. The want of it is easily made up for by a slightly increased rate of spin.

Another way of looking at the matter is to assume, from Mr. Hodge's data, 180 yards as a really fine carry, and thence to calculate by the formula the requisite time of flight. It varies from $4^s \cdot 1$ to $2^s \cdot 9$ according as the resistance, and therefore the necessary initial speed, are gradually increased; the former from *nil* to infinity, the latter from 132 foot-seconds upwards. Thus the observed time exceeds that which is really required when there is no spin, by 60 per cent. at the very least!

The necessity for underspin being thus demonstrated, we have next to consider how its effect is to be introduced in our equations. On this question I expressed a somewhat too despondent opinion in the previous part of this paper. A rather perilous mode of argument (which I have since been able to make much more conclusive) first suggested to me that the deflecting force, which is perpendicular at once to the line of flight and to the axis of rotation, must be at least approximately proportional to the speed and the angular velocity conjointly. But I tried (with some success) to verify this assumption by various experimental processes. These, as will be seen, led also to a numerical estimate of the magnitude of the deflecting force. [And I was greatly encouraged in this work by the opinion of Sir G. G. Stokes, who wrote:—"I think your suggestion of the law of resistance a reasonable one, and likely to be approximately true." This is quite as much as I could have hoped for.]

First: by the well-known phenomena called heeling, toeing, and slicing, which are due to the ball's rotation about a vertical axis. I have often seen a well-sliced ball, after steadily skewing to the right through a carry of 150 yards or even less, finally move at right angles to its initial direction, and retain very considerable spin when it reached the ground. Neglecting the effects of gravity, the equations of the path should be, in such a case,

$$\ddot{s} = -\frac{\dot{s}^2}{a},$$

$$\frac{\dot{s}^2}{\rho} = k\dot{s}\omega;$$

expressing the accelerations in the tangent, and along the radius of curvature, respectively. If we introduce the inclination, ϕ, of the tangent to a fixed line in the plane of the path, the second equation becomes

$$\dot{\phi} = k\omega,$$

showing that the time-rate of change of direction is proportional to the speed of rotation. The first equation gives, of course,

$$\dot{s} = V\epsilon^{-\frac{s}{a}},$$

where V is the initial speed.

The space-rate of change of direction, *i.e.* the curvature of the path, is thus

$$\frac{d\phi}{ds} = \frac{k\omega}{V}\epsilon^{\frac{s}{a}},$$

increasing in the same proportion as that in which the speed of translation diminishes; and, if we regard ω as practically unaltered during the short time of flight, the intrinsic equation of the path is

$$\phi = \frac{ka\omega}{V}(\epsilon^{\frac{s}{a}} - 1).$$

A rough tracing from this equation is easily seen to reproduce distinctly all the characteristics of the motion

[1] Part of the substance of a paper on the Path of a Rotating Spherical Projectile, read to the Royal Society of Edinburgh on June 5.

of a sliced, or heeled, ball. And, by introducing an acceleration in the plane of the path, constant in magnitude and direction, the path might be made to intersect itself repeatedly.

By the statement made above as to the whole change of direction in the course of a well-sliced ball, and with 5^s as the time of flight (for it, like the carry, is notably reduced by slicing) we have

$$\frac{\pi}{2} = 5k\omega.$$

Thus it is clear that we may easily produce rotation enough in a golf-ball to make the value of $k\omega$ as great as 0·3 or even 0·4. And this can, of course, be greatly increased when desired. This datum will be utilised later. The fact (noticed above) that the time of flight, and the carry, are both reduced by slicing, gives another illustration of the necessity for underspin when the time of flight is to be long, and the carry far.

Secondly: by a laboratory experiment which, I have only recently learned, is due in principle to Robins. (*An Account of Experiments relating to the Resistance of the Air.* R.S. 1747.) I suspended a wooden shell, turned very thin, by a fine iron wire rigidly fixed in it, the other end of the wire being similarly attached to the lower end of a vertical spindle which could be made to rotate at any desired rate by means of multiplying gear. Thin as was the wire, it was but slightly twisted in any of the experiments, so small was the moment of inertia of the wooden shell. The wire acted as a universal flexure joint; and, by lengthening or shortening it I could make the ball's mean speed, in small pendulum-oscillations, vary within considerably wide limits. I verified this result by substituting for the shell a leaden pellet of equal mass but of far smaller radius, as I feared that some part of the result might be due to stiffness of the wire, produced by torsion. But with the pellet the rotation of the orbit was exceedingly slow. Thus ω, and the average value of $\dot{s}$, could have any assigned values; and from the elliptic form and the rate of *rotation* of the orbit of the ball, the transverse force was found to be proportional to either of them while the other was kept constant. An exceedingly interesting class-illustration can be given by making the ball revolve as a *conical* pendulum, and while it is doing so giving it spin alternately with, and opposite to, the direction of revolution. The effects on the dimensions of the orbit and on the periodic time are beautifully shown. This form of experiment could be easily applied to considerable speeds, both of the translation and of rotation, if the use of a proper ball could be secured. But it cannot be made strictly comparable with the case of a golf-ball; as the speed of translation can never much exceed that for which the resistance is as its first power only. [Robins' suspension was bifilar, and the rotation he gave depended more on the twisting of the two strings together than on the torsion of either. In this mode of arrangement it is difficult to measure the rate of spinning of the bob, and almost impossible to vary it at pleasure.]

We must next say a few words as to the manner in which the spin, thus *proved* to have so much influence on the length of the carry, is usually given. I pointed out, in the earliest article I wrote on the subject, "The Unwritten Chapter on Golf" (*Scotsman*, Aug. 31, or NATURE, Sept. 22, 1887), that spin is necessarily produced when *the direction of motion of the club-head*, as it strikes the ball, *is not precisely perpendicular to the face.* Now, even when the head is not purposely laid a little back in addressing the ball, (many of the longest drivers do this without asking Why) it must always become so in the act of striking if the player stand ever so little behind the ball:—especially if, as Mr. Hutchinson so strongly urges upon him, he makes the path of the head at striking as nearly straight as possible. Mr. Hutchinson gives a highly specious, but altogether fanciful, reason for this advice. We now see why the suggestion is a really valuable one. A "grassed" club, and especially a spoon, gives this result more directly. As soon as I recognised this, I saw that it furnished an explanation of a fact which had long puzzled me:—viz. that one of my friends used invariably to call for his *short* spoon when he had to carry a bunker, so distant that it appeared impossible of negotiation by anything but a play-club. And, if the ball be hit ever so little under the level of its centre, with the upper edge of the face, very rapid underspin may be produced. This was probably at least one of the objects aimed at (however unwittingly) by the best club makers of last generation, for they made the faces of drivers exceptionally narrow. Some time ago I proposed, with the same object in view, to bevel the face by deeply rasping off both its upper and lower edges:—thus in addition saving the necessity for the "bone."

I have neither leisure nor inclination to attempt (for the present at least) more than a first approximation to the form of the path under the conditions just pointed out. Anything further would involve a laborious process of quadratures, mechanical or numerical, only to be justified by the command of really accurate data as to the values of a and V. I shall therefore at once assume that neither gravity nor the spin affects the translatory speed of the ball. (If the spin have such an effect, it will be taken account of sufficiently by a slight change in the constant of resistance; and the effect of gravity on a low trajectory is mainly to produce curvature which, in this case, is to a great extent counteracted by the spin. It is easy to see that the effects of this ignoration of gravity, in the tangential equation of motion, are to make the path rise a little too slowly at first, then too fast; to make it rise too high, and descend at too small a slope.) Hence we may keep the first equation of motion above, and write the second as

$$\dot{\phi} = k - \frac{g}{\dot{s}}$$

where ϕ is reckoned positive in the ascending part of the path; and k is written for $k\omega$, its dimensions being those of angular velocity. With the help of the value of $\dot{s}$, above, this becomes

$$\frac{d\phi}{ds} = \frac{k}{V}\epsilon^{\frac{s}{a}} - \frac{g}{V^2}\epsilon^{\frac{2s}{a}}.$$

In x, y coördinates, x horizontal, this is nearly

$$\frac{d^2y}{dx^2} = \frac{k}{V}\epsilon^{\frac{x}{a}} - \frac{g}{V^2}\epsilon^{\frac{2x}{a}}.$$

Thus the x coördinate of the point of contrary flexure is found from

$$\epsilon^{\frac{x}{a}} = \frac{kV}{g},$$

so that there must be such a point, *i.e.* the path is concave upwards at starting, if kV be ever so little greater than g.

Again

$$\frac{dy}{dx} = e + \frac{ka}{V}(\epsilon^{\frac{x}{a}} - 1) - \frac{ga}{2V^2}(\epsilon^{\frac{2x}{a}} - 1)$$

where e is the initial slope. The x coördinate of the vertex is found by putting

$$\frac{dy}{dx} = 0.$$

Finally, the approximate equation of the path is

$$y = ex + \frac{ka^2}{V}\left(\epsilon^{\frac{x}{a}} - 1 - \frac{x}{a}\right) - \frac{ga^2}{4V^2}\left(\epsilon^{\frac{2x}{a}} - 1 - \frac{2x}{a}\right).$$

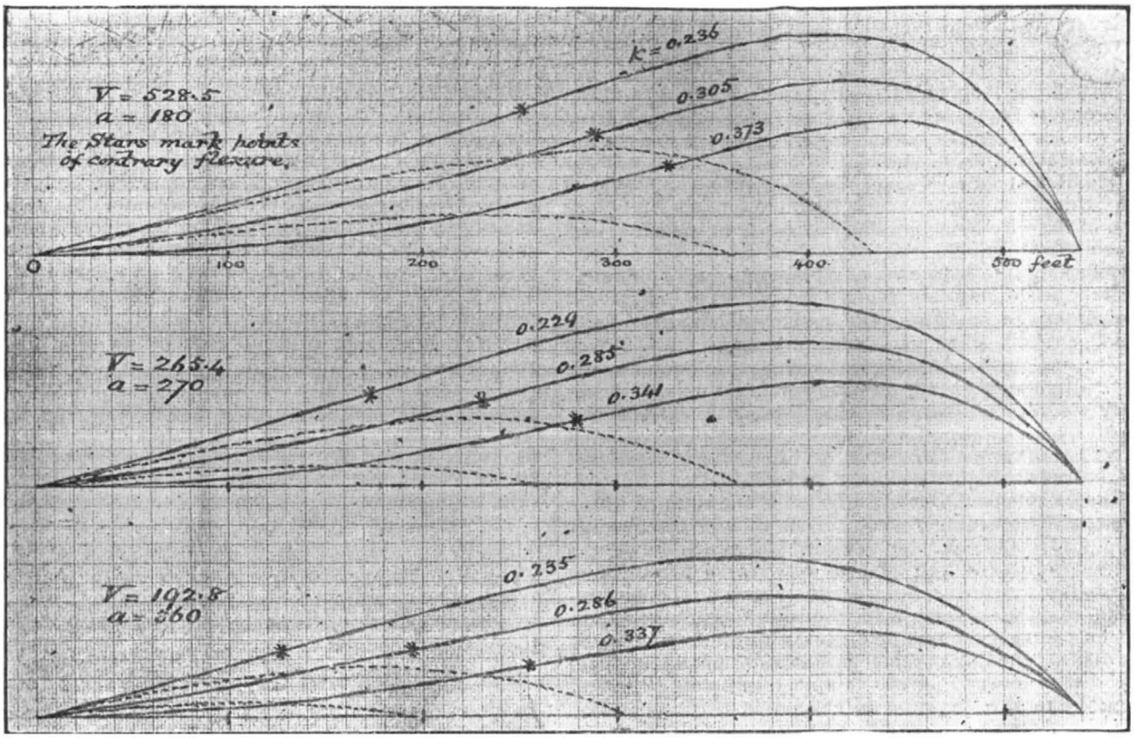
To deal expeditiously with these equations I formed a table of values of the various factors in brackets, by the help of Glaisher's data of natural antilogarithms (*Camb. Phil. Trans.* xiii, 243). Next, I utilized in the equation

$$Vt = a(\epsilon^{\frac{x}{a}} - 1)$$

the well ascertained data $6^s{\cdot}5$ for the time of flight, and 540 feet for the carry, thus obtaining a general expression for V in terms of a. Then, in consequence of the want of accurate data, I chose three values of a, one considerably less than, the second nearly equal to, and the third considerably greater than, that which results from Bashforth's experiments with iron spheres. Thus I found the following values:—

a	V
180	528
270	265·4
360	193·

fault. I regret this for the additional reason that I should have liked to add an illustration of an extremely exaggerated path in which e is (say) zero, and k unity at the least. Under conditions of this kind there might be kinks in the path! For a similar reason I cannot attempt to work out the effect of wind with any attempt at precision, at least in the case when the drive is against the wind and the upward concavity of the path becomes in consequence much more prominent. It is easy in every case to *form* the more exact equations, but the labour of treating them even to a rough approximation would be considerable.



Next, with each pair of these numbers, and with the successive values $\frac{1}{4}$, $\frac{1}{8}$, and 0 for e, I found k from the condition that $y = 0$ for $x = 540$. These values of k are of course greater as e is less, and also as a is less. But all are found to lie between the limits derived, above, from the data for a sliced ball. All the constants being thus found, the curves were easily traced by a few points:—and the position of the maximum ordinate was found as above. For contrast, I have put in (dotted) the paths of drives corresponding in all respects with the others, except the absence of rotation. Poorly as these show, they are probably unduly favoured at the expense of the others, as I have taken a the same for each of the group; though it is probably reduced by the spin, so that rotation increases the direct resistance. The comparison of these with those in which rotation has a share shows that, though strength and agility are undoubtedly of importance in long-driving, even a store of these qualities equalling in amount that of a full-sized tiger is comparatively inefficient as against the skill which imparts a sound undercut. For here, as elsewhere, the race is not to the swift, nor the battle to the strong. Craft beats Kraft all the world over! La Puissance! ce n'est pas frapper fort, mais frapper juste!

From the very nature of the process I used in approximating, none of these curves can be quite trustworthy, those giving the greater elevations being most at

I am engaged at present in endeavours to find something like a proper value of a, or of V, above; so as to have reasonable confidence in my data before I engage in what promises to be a heavy task. Of course, if I can obtain a satisfactory value of one of them, that of the other would follow. But independent determinations of both would enable me to subject the theory to the most complete test imaginable. I am inclined to think that the value of a (280 feet), which I calculated from Bashforth's data, is too large (*i.e.* it makes the resistance too small) for a golf-ball:—and thus that the true path is intermediate in form between those of the first and of the second series in the cut. For the initial speeds required, even with $a = 270$, to give a carry of 540 feet *without spin*, are 462 and 653 foot-seconds for slopes of 1 in 4 and 1 in 8 respectively:—the corresponding times of flight being only $3^s{\cdot}7$ and $2^s{\cdot}6$.

P. G. TAIT.

NOTES.

WE are glad to record that the Council of the Imperial University of Kasan has elected Prof. J. J. Sylvester honorary member of the University.

THE Albert Medal of the Society of Arts for the present year has been awarded to Sir John Bennet Lawes and a like medal