the volume [a']. Then the wave-lengths, λ, of the lines in the spectrum of A, which belong to a, are to the wave-lengths, λ', of the lines in the spectrum of C, which belong to a', as [a] is to [a']. If there be no condensation the lines are the same as to their position, since the volume remains constant, though their relative intensities may vary greatly; the compounds of hydrogen with chlorine, bromine, and iodine may be cited as examples. Assuming this principle, the spectra of hydrogen and water vapour offer some very interesting relationships. Thus, the wave-lengths of the second spectrum of hydrogen, which seems to belong to a molecule, H′, of a more complicated structure, when divided by 2 give the wave-lengths of the lines of water vapour, the volume of the free molecule H′ being double that which hydrogen occupies in water vapour. The wave-lengths of the elementary spectrum of hydrogen can be arranged into two groups, *a* and *b*, which give the lines of the water vapour spectrum when they are respectively multiplied by $\frac{19}{30}$ and by $\frac{4}{5}$. From this Prof. Grünwald concludes that hydrogen is composed of the combination of four volumes of the element *a* with one of the element *b*. The first element, *a*, should be the lightest of all the gases, and much lighter than hydrogen; and since it should therefore probably enter largely into the constitution of the corona, Prof. Grünwald gives it the name of "coronium." The D_3 or "helium" line is found in the spectrum of the second element, *b*; and the Professor therefore gives *b* the title "helium." The correspondences between the wave-lengths calculated by Prof. Grünwald for the elements *a* and *b* and those of lines actually observed in the spectrum of the sun are certainly striking. Following out the same method, the Professor finds the chemical formula of oxygen as follows—

$$O = H'O' = H'[b_4O''_5] = H'[b_4(b_4c_5)_5].$$

The line of the corona, 1474 K, should belong to the element "coronium," and would correspond—$5316 \times \frac{2}{3} = 3544$—to a line, as yet unknown, of the elementary spectrum of hydrogen, with wave-length 3544. Prof. Grünwald had hoped that the late eclipse would have afforded an opportunity of searching for this line. It is clear that the dissociation of hydrogen in the sun is a necessary consequence of this theory, since its two constituent elements will thus both be in the free state in the solar atmosphere.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 *SEPTEMBER* 25—*OCTOBER* 1.

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

At Greenwich on September 25

Sun rises, 5h. 52m.; souths, 11h. 51m. 42·7s.; sets, 17h. 51m.; decl. on meridian, 0° 50′ S.: Sidereal Time at Sunset, 18h. 8m.

Moon (one day after First Quarter) rises, 14h. 54m.; souths, 19h. 14m.; sets, 23h. 36m.; decl. on meridian, 19° 26′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Decl. on meridian. ° ′
Mercury	6 57	12 34	18 11	5 6 S.
Venus	5 43	11 16	16 49	6 1 S.
Mars	1 38	9 9	16 40	16 30 N.
Jupiter	9 7	14 3	18 59	12 56 S.
Saturn	0 20	8 10	16 0	19 30 N.

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

Sept.	Star.	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image. ° °
25	*f* Sagittarii	5	23 27	0 30*	125 330
26	B.A.C. 7053	5½	17 35	18 55	73 270
26	*o* Capricorni	5½	17 36	18 55	73 269
27	*υ* Capricorni	5½	0 1	0 48	93 7
28	42 Aquarii	6	22 14	23 1	65 1

* Occurs on the following morning.

Meteor-Showers.

	R.A.	Decl.	
Near δ Aurigæ	78°	57° N.	Swift.
From Lynx	105	51 N.	Very swift.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·3	81 16 N.	Sept. 28,	5 34	*m*
R Ceti	2 20·3	0 41 S.	,, 28,		*M*
Algol	3 0·8	40 31 N.	Oct. 1,	4 1	*m*
λ Tauri	3 54·4	12 10 N.	Sept. 26,	22 36	*m*
			,, 30,	21 28	*m*
R Boötis	14 32·2	27 14 N.	,, 28,		*M*
δ Libræ	14 54·9	8 4 S.	,, 26,	3 13	*m*
U Coronæ	15 13·6	32 4 N.	,, 29,	21 59	*m*
R Scorpii	16 10·9	22 40 S.	,, 28,		*M*
U Ophiuchi	17 10·8	1 20 N.	,, 26,	4 37	*m*
		and at intervals of		20 8	
X Sagittarii	17 40·5	27 47 S.	Sept. 28,	23 0	*m*
			Oct. 1,	20 0	*M*
W Sagittarii	17 57·8	29 35 S.	,, 1,	19 0	*m*
β Lyræ	18 45·9	33 14 N.	Sept. 25,	4 0	m_2
R Lyræ	18 51·9	43 48 N.	Oct. 1,		*m*
S Vulpeculæ	19 43·8	27 0 N.	Sept. 30,		*M*
η Aquilæ	19 46·7	0 43 N.	,, 26,	3 0	*m*
S Sagittæ	19 50·9	16 20 N.	,, 25,	3 0	*m*
			,, 28,	3 0	*M*
R Vulpeculæ	20 59·4	23 22 N.	,, 30,		*m*
δ Cephei	22 25·0	57 50 N.	,, 28,	5 0	*M*
			Oct. 1,	23 0	*m*

M signifies maximum; *m* minimum; m_2 secondary minimum.

THE UNWRITTEN CHAPTER ON GOLF.[1]

THERE are two ways of dealing with a difficulty—the metaphysical and the scientific way. The first is very simple and expeditious—it consists merely in giving the Unknown a name whereby it may be classified and categorized. Thenceforward the Unknown is regarded as having become part of knowledge. The scientific man goes further, and endeavours to find what lies concealed under the name. If it were possible for a metaphysician to be a golfer, he might perhaps occasionally notice that his ball, instead of moving forward in a vertical plane (like the generality of projectiles, such as brickbats and cricket-balls), skewed away gradually to the right. If he did notice it, his methods would naturally lead him to content himself with his caddie's remark—"Ye heeled that yin," or, "Ye jist slicet it" (we here suppose the metaphysician to be right-handed, as the sequel will show). But a scientific man is not to be put off with such flimsy verbiage as this. He *must* know more. What is "heeling," what is "slicing," and why would either operation (if it could be thoroughly carried out) send a ball as if to cover-point, thence to long slip, and finally behind back-stop? These, as Falstaff said, are "questions to be asked."

As the most excellent set of teeth, if but one incisor be wanting, gives pain rather than pleasure to the beholder; so is it with the works of the magnificent Clark, the sardonic Hutchinson, and the abstruse Simpson. These profess to treat of golf in theory as well as in practice. But in each a chapter is wanting, that which ought to deal with "slicing," "heeling," "toeing," "topping," &c., not as metaphysical abstractions enshrined in homely though unpleasant words, but as orderly (or disorderly) events due to physical causes and capable of receiving a physical explanation. Mayhap, with the aid of scissors and paste, some keen votary of the glorious game will employ this humble newspaper column to stop, however imperfectly and temporarily, the glaring gap which yawns in the work of every one of its exponents! If so, this scrap will not have been written in vain. It may even, in the dim future, lead some athletic pundit to elaborate *The Unwritten Chapter*.

Every one has heard of the uncertain flight of the projectile from Brown Bess, or from the old smooth-bore 32-pounders, and of the introduction of rifling to insure steadiness. Now, all that rifling secures is that the ball shall rotate about an axis nearly in its line of flight, instead of rotating (as the old smooth-bore projectiles did) about an axis whose direction is determined by one or more of a number of trivial circumstances whose effects cannot be calculated, barely even foreseen. Thus it appears that every deviation of a spherical projectile from its line of flight (excluding, of course, that due to gravity) is produced by rotation about an axis perpendicular to the line of flight.

[1] From *The Scotsman*, August 31, 1887.

This question was very skilfully treated by Magnus in 1852. He showed by experiment that, when a rotating sphere is exposed to a current of air whose direction is perpendicular to the axis of rotation, the side of the sphere which is advancing to meet the current is subject to greater pressure than is that which is moving in the direction of the current. This difference of pressures tends to make the sphere move in a direction perpendicular at once to the current and to the axis of rotation—the direction, in fact, in which the part of the sphere facing the current is being displaced. But it is a matter of no consequence whether the current of air comes against the sphere, or the sphere moves in the opposite direction (and with the same speed) through still air. Hence Magnus's experimental result amounts to this :—*If a spherical ball be rotating, and at the same time advancing in still air, it will deviate from a straight path in the same direction as that in which its front side is being carried by the rotation.*

The physical explanation of the difference of pressures in question requires analysis which would be altogether out of place in an article like this. But, even without it, we feel ourselves to be on perfectly safe ground when we recollect that Magnus's result was obtained by direct experiment, and therefore expresses a physical truth.

Bearing in mind the statement italicized above, let us now consider the anomalous behaviour of a golf ball. The key of the position is "slicing." He who understands this will, without much further trouble, master the rest of the difficulties above referred to. Slicing is effected by the player's drawing the club towards his body while it is in the act of striking the ball. The ball is thus treated almost precisely as is a whipping-top—*i.e.* it is not merely driven forwards, but is made to spin about a nearly vertical axis. The side of the ball to which the club was applied was drawn in towards the player. Hence, as the ball advances, its front is moving towards the player's right, and the deviation takes place to that side accordingly.

A "topped" ball "dooks" (*i.e.* plunges, as it were, headlong downwards). We can see at once that it should be so, in accordance with the general statement. For, in topping, the upper part of the ball is made to move forward faster than does the centre, consequently the front of the ball descends, in virtue of the rotation, and the ball itself skews in that direction.

When a ball is "under-cut" it gets the opposite spin to the last, and, in consequence, it tends to deviate upwards instead of downwards. The upward tendency often makes the path of a ball (for a part of its course) concave upwards in spite of the effects of gravity. This is usually regarded as a very strange phenomenon, even by men to whom "dooking" seems natural enough. As will be seen later, a "jerked" ball must, from the way in which the face of the club is moving at impact, have this spin, and consequently must skew upwards.

Since a "heeled" ball deviates to the right as a "sliced" ball does, it must be rotating in a similar manner. But a "toed" ball deviates to the left, and must, therefore, have the opposite spin. The way in which the spin is produced in these cases is not so easy to explain as it was in the case of topping. We may begin, however, by saying that the terms "heeling" and "toeing" are entirely misleading, if they be taken to imply necessarily the hitting of the ball with the heel or the toe of the club as the case may be. For, as will soon appear, a ball may be heeled off the toe of a club, or toed off the heel, at pleasure! And when a man holds his club properly, so that in the act of striking the ball *the club-head is moving in a direction exactly perpendicular to the face*, there will be neither heeling nor toeing whatever part of the face strikes the ball, provided it be struck by the face proper, and not by an edge. It will not be driven so far by the heel, or by the toe, as by the proper centre of percussion; but there will be no spin, and therefore no skewing.

The true explanation, therefore, of heeling and toeing is to be found in the fact that the club-head, when it strikes the ball, is not moving perpendicularly to the face; or, what comes practically to the same thing, the face of the club is not perpendicular to the direction in which the club is moving (*i.e.* it is to be presumed the direction which it is desired that the ball should take). In this case we may regard the motion of the head as resolved into two parts—one perpendicular to the face, the other parallel to it. The former gives translation only to the ball. The latter gives it not only translation, but rotation also. When the toe of the club is too much thrown back—*i.e.* when the heel is too much forward—the motion parallel to the face is from toe to heel, exactly as in "slicing." "Heeling" and "slicing" are thus practically the same thing, so far at least as the ball is concerned. But, so far as the player is concerned, they are quite different; and (what is of far more importance) the modes of cure are entirely dissimilar. To cure slicing, cease to pull in your arms; to cure heeling, place your club beside the ball as in addressing, and note the lie of the head. If that be incorrect, put it right; if it be correct, the fault lies in "gripping" (instead of holding loosely) with your right hand. Many a man's play has been spoiled for the day by his having applied (too often by his caddie's advice) the cure for "heeling" when the disease was "slicing," or *vice versâ*.

When the toe of the club is turned inwards, the face is pushed tangentially outwards behind the ball, so that the spin and its consequences are exactly the reverse of those just described.

From what has been said above, it is obvious that the flight of a ball, if it be nearly spherical and have its centre of gravity at its centre, depends solely upon the impulse originally given to it. [If the centre of gravity be not in the centre of the ball, it is only by mere chance (in teeing) that the ball escapes having a rapid rotation given to it, even by the most accurate of drivers. Should it fortunately escape initial rotation, still its flight cannot be regular. A simple and exceedingly expeditious test of this defect consists in placing the ball on mercury in a small vessel. If, in that position, it oscillates rapidly about the vertical, it should be at once rejected as absolutely worthless.] This is a point on which opinions of the wildest extravagance are often expressed. Some balls, it is said, "will not fly," &c. How if they were fired from a blunderbuss? Nobody seems to have made the trial in the only reasonable way—viz. by using a cross-bow or a catapult to give the initial speed. With such an instrument two homogeneous spherical balls of equal size and weight, whatever their other peculiarities, would be despatched under exactly the same conditions, and their behaviour could be *compared*—it would not require to be *contrasted*.

But he is correct (in meaning, though not in his English) who says that some balls "won't drive." It is easy to recognize a good ball by trial, but difficult to define one, at least without periphrasis. A good ball is one which acquires, under given conditions of good driving, as great an initial speed as possible, coupled with the minimum of rotation.

So far as we are aware, all direct scientific experiments on elastic resilience have been made at low speeds, and consequently with but slight distortion of the impinging bodies. But the circumstances of a "drive" in golf are of a totally different character; so that the results of the drive must be themselves regarded as the only data of the requisite kind which we possess. In this matter very valuable data (not for golf alone) might easily be obtained by measuring the height to which a ball rebounds when fired from a powerful catapult against a wooden or stone floor; recording on each occasion the extent to which the springs of the weapon were extended, and the appended weight which would produce the same extension. Some keen golfer may thus find thoroughly useful as well as congenial occupation, when his happy hunting-grounds are inches deep in snow. P. G. T.

SCIENTIFIC SERIALS.

Bulletin de l'Académie Royale de Belgique, June.—On the problematic satellite of Venus, by Paul Stroobant. After a complete survey of the various appearances of this object between the years 1645 and 1768, the author discusses the different conjectures advanced by astronomers to explain the phenomenon. The theory of a true satellite is rejected on the ground that no orbit could be made to correspond with all the recorded observations, while the elements calculated by Lambert from some of them would make the planet ten times larger than its actual size. In the same way are disposed of the other suggestions that it might be the reflection of Venus on certain frozen particles in the atmosphere, or an inter-Mercurial planet, or a planet with a revolution slightly differing from that of Venus, or an asteroid, and the like. Several reasons are then advanced in support of the view that the pretended satellite is to be referred to certain small fixed stars near which Venus was passing when the various observations were taken. This explanation is specially obvious in one instance, where the movement attributed to the supposed satellite is precisely the proper motion, but in the opposite direction, of Venus at that moment in relation to the fixed stars.—On a specimen of crystalline iron-glance formed on some old iron