# *The Relation between the Movements of the Eyes and the Movements of the Head*

BEING THE

FOURTH ROBERT BOYLE LECTURE

DELIVERED BEFORE THE

*OXFORD UNIVERSITY JUNIOR SCIENTIFIC CLUB*

*On Monday, May* 13, 1895

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LONDON: HENRY FROWDE, AMEN CORNER, E.C.

EDINBURGH: 12 FREDERICK STREET. GLASGOW: 205 BUCHANAN STREET

Oxford: 116 High Street

1895

Price One Shilling net

Oxford
HORACE HART, PRINTER TO THE UNIVERSITY

# On the Relation between the Movements of the Eyes and the Movements of the Head.

WE all know that it was a long time before mankind found out that the earth moves. For ages the apparent motion of the heavenly bodies was supposed to be their real motion, the earth being fixed. We, who know something of the truth in this matter, do not however, any more than our ancestors did, see or feel the earth move. We believe that it does so either because we have been told by some one who, we think, knows about such things, or because we have reasoned the matter out from data observed by ourselves or reported by credible observers. But in habitual thought and speech we go back to the old assumption which, for our practical, terrestrial purposes, answers well enough, and is perfectly in accordance with our sensations.

When we turn from the great Cosmos to the microcosm, when we compare the motion of our own body among the various fixed (terrestrially fixed) and moving bodies around us, with the motion of the earth among the stars, we find quite a different state of matters. It never occurs to us that our own body is at rest, and that the trees, houses, &c. move. When we really move we not only know, but feel and see that we are moving, and every one, learned or ignorant, old or young—if only he is sober—feels and sees that the solid earth is fixed, except on the rare occasion of an earthquake, and in the case of some illusions which we shall have to consider. I wish to discuss the cause of this *sensation* of the fixedness of the earth, and also incidentally of the exception implied in the words I have just used, 'if only he is sober.'

If we keep our head fixed and look at any really fixed scene—say, a room in which there is nothing moving—or a landscape, if we can find one without railway trains, ships, moving beasts or flying birds, we can allow our eyes to run over it in as uniform or as irregular a way as we please, and see that the scene remains fixed. We might have supposed that, as we move our eyes from right to left, the whole scene, like a moving panorama, should seem to move from left to right, but it does not do so. It remains visibly at rest, and we know, without any reasoning about it, that the changes of view were produced by the motion of our eyes.

We fancy that we can move our eyes uniformly, that by a continuous motion like that of a telescope we can move our eyes along the sky-line in the landscape or the cornice of the room, but we are wrong in this. However determinedly we try to do so, what actually happens is, that our eyes move like the seconds hand of a watch, a jerk and a little pause, another jerk and so on; only our eyes are not so regular, the jerks are sometimes of greater, sometimes of less, angular amount, and the pauses vary in duration, although, unless we make an effort, they are always short. During the jerks we practically do not see at all, so that we have before us not a moving panorama, but a series of fixed pictures of the same fixed things, which succeed one another rapidly. It is not difficult to understand how this gives rise to a sensation of the fixedness of the external scene. If, in the otherwise fixed scene, there is a really moving object, we see it move, because during the pauses, short as they are, the moving object has visibly changed its place, and in each of our fixed pictures the moving object is seen to move. If it moves too slowly for this, then we do not *see* it move, but only infer its motion from comparison of its position at different times. If we keep our eyes fixed on the moving object, and this is possible if it does not move too fast or too irregularly, then we see it fixed and the really fixed things moving, an illusion we have all observed when the pier seems to move and the steamer remain at rest.

That the eyes jerk in the way now stated can be made plain by means of a simple experiment. If we have in the field of view a bright object, such as an incandescent electric lamp, and after running our eyes over the scene before us, shut our eyes, we see secondary images of the bright object[1]. Now, if the eyes moved continuously from one position to another, we should see between the two secondary images of the bright object corresponding to these two positions, a bright band composed of an infinite number of images each infinitely near its two neighbours. But we see no such band, but a finite number of sharp individual images, each of which corresponds to the position of the eyes during a pause between jerks; unless the bright object is very

[1] The secondary images are better seen if we look at a white surface and wink rapidly.

bright, there is nothing in the secondary image to represent the positions of the eyes during the jerk. If for a bright object we take the sun, then we do see bands joining the sharp secondary images. These bands are fainter than the sharp images and die away sooner. They are the impressions made on the retina by the image of the sun passing rapidly across it during the jerk. But, if with the fixed bright object in the field we follow with our eyes a really moving thing, then on shutting the eyes we see a band of light because the image of the bright object passed not very rapidly across the retina.

This habit of jerking the eyes from one position of vision to another, as fast as the light, well-poised globes can be swung round by the quick-working, straight-fibred muscles which move them, may be an innate habit, or it may have been acquired by our looking at things and turning quickly from one object of interest to another; at all events, it is now the way in which alone we can move them, unless we fix them on a moving object.

So far I have supposed the head fixed and the eyes alone moving. Let us now attend to what happens when we move our head [1].

The movement of the head, unless it is very rapid, makes no difference at all in the phenomena just described.

If we call the line along which we look during the pause between two jerks a glance-line, we may describe the whole phenomenon by saying that the glance-lines are fixed relatively to fixed external objects, whether

[1] By 'moving the head' I mean moving the head either alone or along with the body or any part of it.

the head is rotated or not. This of course means that, during a pause, the eyes are rotated relatively to the head about the axis about which the head is really rotated, in the opposite sense and through the same angle as the head.

It might, for all that has been yet said, be supposed that this fixedness of the glance-lines, when the head is rotated, depends on the habit of looking at things; but that this is not the cause, or at all events not the only cause, is plain from the fact that the same relative movements of the eyes take place when we look at an objectless field of view, such as the clear cloudless sky, or, as was I believe first noticed by Dr. Breuer, when the eyes are shut. By placing the fingers lightly over the closed eyelids we can feel the motion of the prominent cornea. If, with eyes shut and fingers so placed on the eyelids, we turn the head or turn head and body round, we feel the eyes twitch. As the head turns round, the eyes retain for a little a fixed orientation in respect to external fixed things and then jerk so as to make up for lost time—again pause, and again jerk, and so on. So that while the head turns uniformly, the eyes, which must of course on the whole make one full turn while the head makes one full turn, do their rotation intermittently, being so to speak left behind by the head and then making up by a rapid jerk.

Another proof that these compensatory movements, as they may be called, of the eyeballs are not, or at least not wholly, caused by the effort of looking at things, is afforded by observing what happens when the head is rotated about a fore and aft axis, about an axis coinciding with a glance-line. If we keep our eyes fixed on a particular point and rotate the head about the line along which we look [1] we still see things fixed, the world does not seem to revolve about our fore and aft axis. Here also we can show by means of secondary images that we see a series of fixed pictures.

If, with a bright object in the field of vision, we fix our eyes and keep them fixed on a point, about 15° distant from the bright object (if we keep both eyes open, about as far from our eyes as the bright object is, so as to avoid double vision), and then rotate the head about a fore and aft axis through, say, 30° by inclining the head towards one shoulder, and shut the eyes after this performance, we see a number of sharp secondary images of the bright object arranged upon an arc of a circle, the radius of which is the angular distance of the bright object from the point fixed.

If I have rotated my head through about 30°, I see about five secondary images, so that what I call the *angle of rotatory nystagmus* is in my case about 6°. Here we have been looking all the time at the same point, and it is not easy to suppose that the very slight attention we pay to objects seen indirectly, or as we sometimes say 'with the tail of the eye,' could lead to a habit, so fixed that we cannot escape it, of moving the eyeballs in the way described.

I have said that the movement of the head, *unless it is very rapid*, does not affect the fixedness of the glance-lines. Translatory motion of our body may be so rapid, as in a railway train, that the eyes cannot twitch so fast as to keep the glance-lines fixed relatively to near fixed objects. The eyes do their best, they twitch but not

[1] If we take a sufficiently distant object as the thing to be looked at, we may neglect the want of coincidence of the two glance-lines belonging to the two eyes, and, moreover, all that is here described is seen as well, though not so conveniently with one eye shut.

enough, unless the train is moving slowly, and near objects seem to fly backwards. We succeed with fixed objects at a greater distance from us; we can see them fixed, and all objects between us and such visibly fixed objects are seen to move backwards, fixed things beyond them seem to move forward with us. Of course, if by keeping our attention on our carriage and its contents, our glance-lines become fixed in reference to these really moving things, they seem fixed, and the whole world outside of the carriage is seen to move in the direction opposite to that of our real motion. It is also obvious that rotation of the head, if it is more rapid than the quickest possible rotation of the eyeball in the head, must affect the position of a glance-line, for, in order that the glance-line remain fixed, the eyeball must rotate in reference to the head as fast in one sense as the head rotates in reference to external things in the other sense; but in the case supposed the eyeball cannot do so. We can try this experiment without having recourse to mechanical means of rotating our body and head, which of course we could do as fast as we please, and a great deal faster than would be either pleasant or safe. The most rapid rotation of our head which we can produce by the direct action of our muscles is what is known as wagging, that is a rotation about a vertical axis upon the joint between the first two vertebrae. In this way we can give the head an angular velocity considerably greater than the maximum angular velocity of the eyeball. When we do this as fast as we can, we see that external things do not appear steady. When we wag our head to the right we see the world wag to the left, and *vice versa*. But the external really fixed things do not appear to us to describe nearly so large an angle as the head really does; the eyes make an effort to compensate the rotation of the head, an effort only partially successful, the angle through which external things seem to move being the difference between the actual angular rate of movement of the head and the maximum possible angular rate of movement of the eyeball in its socket. This difference can best be observed, and indeed can be approximately measured, by observing a distant light on a dark night, while we wag the head. The point of light seems drawn out into a horizontal line of light, the apparent length of which is the angular difference in question. As we can wag our head much faster than we can nod it, the apparent length of the vertical line of light into which a bright point is drawn out when we look at it and nod as rapidly as we can, is much less than that of the horizontal line of light just spoken of, but I find that I can by nodding rotate my head about a right and left axis a little faster than I can rotate my eyes about the same axis, so that the luminous point does appear drawn out into a short vertical line.

Such violent movements of the head occur sometimes in our ordinary (not experimental) use of our eyes, but they are rare and isolated, so that the disturbance of the fixedness of the glance-lines which they cause does not really affect our sense of the fixedness of the world. The illusion of the moving pier and fixed steamer, which we have all also observed when there is a train alongside that in which we happen to be, and we see the moving train fixed and the fixed train moving, is instantly corrected by looking at the shore or the railway station. For a moment these also seem to move, but our glance-lines almost instantly become fixed in reference to these things which we know are fixed, and it is then difficult to recall the illusion. Another similar case is that of the moon and the clouds. We sometimes see the moon moving and the clouds fixed, sometimes the clouds moving and the moon fixed, as our glance-lines are fixed relatively to the clouds or to the moon, and a little practice enables us to change from the one sensation to the other at will.

What has been said seems to show that our immediate sense that the earth and what we call fixed objects on it are fixed is a consequence of the way in which we move our eyes, and, in particular, of the way in which, by a suitable movement of the eyeballs, we involuntarily and unconsciously compensate movements of the head voluntary or involuntary, conscious or unconscious[1].

That such an immediate sense of the fixedness of external fixed things is of great use to us in moving about among them is plainly shown when we observe the trouble which a drunken man, who has lost this sense, has in guiding himself.

I now turn to the question—what is the cause of this prompt and wonderfully accurate compensatory movement of the eyeballs?

There are three sources from which we can obtain information leading to an answer. (1) Experiments on ourselves, (2) anatomical observations and measurements, and (3) observations of the effects of injuries to the labyrinth of the internal ear.

I shall consider these in their order.

By experiments on ourselves I mean the study of the effect on the motion of the eyes and on our sense of the

[1] I need hardly repeat that by movements of the head I mean movements of the head whether accompanied or not by movements of the body.

fixedness of external things, of movements of our head (in this case, always along with the rest of our body) which we do not make, as a rule, for any other purpose.

I have already stated that if we shut our eyes, place our fingers on the eyelids and turn round about a vertical axis, we feel with our fingers the jerking motion of the eyeballs. If instead of turning once round, we turn round several times, still better if we seat ourselves on a turning table and get some one else to turn it and us round at a uniform rate, we find that the jerks become less and less frequent, and after two or three turns cease altogether. Another thing which we observe is, that although the turn-table is being turned round at a perfectly uniform rate we feel the rotation becoming slower and slower, and when the jerks of the eyeballs have quite ceased we feel ourselves at rest, and have no sensation of rotation. Let us for convenience call the sense in which the rotation is still going on positive. This uniform positive rotation has become to us imperceptible (as long as we keep our head in the same position in respect to the vertical) and is what we may call a new zero of rotation. If the rate of rotation is now increased, we feel this increase as a positive rotation; if it is diminished, we feel the diminution as a negative rotation—a rotation the other way about. What we really perceive then is *acceleration* of rotation, using the word acceleration in its technical sense. If the turn-table is stopped, this is a negative acceleration, and what we feel is that we are being turned round in the negative sense and at the same time we feel our eyeballs jerk. The sense of rotation and the jerking die away in this as in the former case.

If, while we are being turned round with uniform angular velocity, but after all sense of rotation and all jerking of the eyeballs have ceased, we open our eyes, we still feel ourselves quite at rest but we see all external objects turning round us: as has been well said by Professor Mach, the external world seems to turn round inside an outer unseen fixed world. It is in reference to this imaginary fixed world that our glance-lines are now fixed. If the rate of rotation is changed while the eyes are open, the sensation of rotation is exactly the same as if they were shut, we feel the acceleration—positive or negative—as a rotation in the one or in the other sense, and the jerks of the eyeballs take place as if the real external world were not there, and we were looking beyond it at the unseen fixed world outside of it, that imaginary world in reference to which our glance-lines are now fixed.

If while the experiment I have described is going on, we move so as to change the direction, in our head, of the axis of rotation, for instance, if, after uniform rotation about a vertical axis has gone on, with the head in its usual upright position, until the sense of rotation has ceased, we bow our head forwards so that the axis of rotation is now parallel to a line from the occiput to the chin, a very striking, and somewhat alarming, but most instructive sensation is experienced. What we feel is that we are being turned round with a rotation which is the resultant of two rotations of equal angular velocity—one the real rotation about what is now the vertical, the other the imaginary (but equally perceived) rotation in the opposite sense about the line in the head which was vertical. If the angular movement of the head is small, so that the angle between what is the vertical and what was the vertical is small, then the two component rotations nearly neutralize one another and the strange and alarming resultant is slight, but if the head is bent so that the old and new verticals are at right angles to one another, the real and the imaginary components are both felt in full, and the effect is very startling. If the rate of rotation is changed simultaneously with the change of position of the head, we have a resultant of two rotations of different angular velocity. The most easily observed case of this kind is when the rotation is stopped altogether at the moment of change of position of the head. Here the real component is zero, and we have only the imaginary one. This is the case of the well-known practical joke: a man is asked to plant the poker before him on the floor, place his forehead on the end of it, walk round it three times, and then rise and walk to the door.

The preliminary part of this experiment presents no difficulty; the victim plants the poker, puts his forehead on it, walks round it with the greatest ease and with no sense of anything unusual. But when he rises, the line in his head which was vertical is now horizontal, and he feels himself turned round about that horizontal line. The external world he also sees turning round this line, objects on the one side rising up and objects on the other side sinking down. In this visibly swaying world he has to guide his sensibly rotating body, and if his friends do not catch hold of him he is pretty sure to fall. All these experiments are most conveniently made on a smoothly working turn-table of such a size that one can comfortably lie down upon it. By the kindness of Messrs. Dove, lighthouse engineers, I had the use of a large turn-table made for the revolving lantern of a lighthouse. It could be turned round smoothly and uniformly, at the moderate speed that is most suitable for experiments of the kind in question. A few experiments with such an apparatus will convince any one that we have here to do with a perfectly definite sense, and not with any vague sensations caused by the inertia of the soft parts of the body.

This is one of the ways in which the phenomena have been explained by those who hesitate to believe that there can be a definite special sense only discovered within the last few years. That the origin of the sensation is not in the soft parts of the body generally, but in the head, is made perfectly plain by the fact that the position of the head and the changes of that position alone determine the sensations. We must therefore look in the head for the organ of this sense.

In close proximity to the cochlea, which is universally regarded as the organ of hearing, there is an organ of very striking, and we might say mysterious, form. It occurs in all vertebrates, and occurs in them fully developed except in the lowest forms of fish. It is contained in a bony (in cartilaginous fishes cartilaginous) cavity, which communicates in birds and mammals with the cochlea or lagena. This cavity may be divided into the vestibule and the three semicircular canals. The canals open at both ends into the vestibule, and each has *at one end* an enlargement called the ampulla [1]. Within this bony case is contained a membranous structure consisting of the utricle situated in the vestibule, and three membranous canals, each in one of the bony canals, each with an ampulla in the bony ampulla;

[1] In all animals the non-ampullary ends of the superior and the posterior canal have a common opening into the vestibule.

and each opening at both ends into the utricle. The vestibule contains, besides the utricle, the saccule, a membranous bag continuous with the cochlear duct, and has in the side next the tympanic cavity a hole in the bony wall filled in by a membrane and known as the *fenestra ovalis*. The saccule and the utricle have each a spot on the lower wall supplied with nerves which end in hair cells covered with the otolith or otoconia and known as the *macula acustica*.

The *maculae acusticae* are probably, as suggested by Mach and Breuer, organs fitted to perceive acceleration of translatory motion, and are not connected directly with the function of the semicircular canals. The *fenestra ovalis* belongs to the organ of hearing, which may thus be said to have a right of way through the vestibule. We need not therefore here consider any further these organs, but confine ourselves to the semicircular canals and the utricle in its relation to them. As already stated, each bony canal contains a membranous canal. The membranous canal is, except at the ampulla, much smaller in bore than the bony canal, so that the space outside the membranous canal filled with perilymph is much greater than the space inside filled with endolymph. The membranous ampulla much more nearly fills the bony ampulla, so that here the perilymph space is comparatively small. The membranous canal is pretty firmly attached (in some animals at all events) to the periosteum of the bony canal. Each canal is, in all animals I have examined, approximately in a plane, and it is important to consider the relations of these planes to one another and to the mesial plane of the head.

As I have brought part of the apparatus with me, I may shortly describe the method I used to measure the angles which these planes make with one another, and also an improved method of which I have not yet had time to make any very full trial.

The method illustrated by the human skull shown is fully described, with woodcuts from photographs, in Professor McKendrick's *Text Book of Physiology*, vol. ii. pp. 697-699, and therefore need not be reprinted here. The other method will I hope give more accurate measurements.

It consists in attaching the preparation—either a cast of the canals or, in the case of a bird, the dissected and cleaned bony canals—to one arm of a branched rod, and a lump of wax to the other. The rod is then fixed to the large apparatus described in the passage in Professor McKendrick's book referred to. The canals are successively made horizontal, and a small plate of glass fixed horizontally in each case—parallel therefore to each canal—to the lump of wax. We can also attach a glass plate parallel to the mesial plane. We can thus have, on a comparatively small piece of wax, glass plates parallel to all the planes the relations of which to one another are to be measured. The lump of wax is then removed from the rod and the angles between the planes of the glass plates measured by means of an ordinary reflexion goniometer.

The general results are:—

1. The canals do not lie rigorously in planes, but sufficiently nearly so to give closely accordant results.

2. The external canals are very nearly at right angles to the mesial plane, and therefore, from the bilateral symmetry, the two external canals are very nearly in one plane.

3. The superior and posterior canals of the same side make approximately equal angles with the mesial plane. In all cases which I have examined, the angle between the posterior canal and the mesial plane is somewhat larger than that between the superior canal and the mesial plane.

From the bilateral symmetry, therefore, the superior canal of the one side is nearly, but not quite, parallel to the posterior canal of the other side. In the discussion of the way in which the system of canals may be supposed to act, I shall for convenience assume that these canals are parallel, as the deviation from exact parallelism only complicates but does not at all vitiate the argument.

4. In man, and in a large number of other animals, the three canals are very nearly at right angles to one another. But in a good many of the animals I have looked at, the superior and posterior canals make with one another an angle considerably greater than a right angle.

Looking at the six canals as forming one system we see that we have three axes (in man and some other animals rectangular axes, in some other animals with two of the axes inclined), and that at right angles to each axis there are two canals, one in the one internal ear, the other in the other; these two canals having their ampullae at opposite ends, so that if rotation take place about the axis the ampulla in the one case precedes the canal, in the other follows it. The vertical axis, as we may call that at right angles to the two external (or horizontal) canals, is pretty nearly vertical in most animals, in the usual position of the head when the animal looks to the horizon; in man it is not exactly so, we must bow our head a little to make this axis vertical. If we suppose we are looking north, the other two axes are N.E. & S.W. and N.W. & S.E. respectively. In man they pass from the eye of one side to the mastoid process of the other side, and are nearly at right angles to one another. As already stated, in some animals they are inclined and are nearer the right and left than the fore and aft line in the head.

In order to see how such a system can work as a hydro-dynamical instrument, let us first consider one canal.

Here we have two watery liquids, the endolymph within the membranous canal, its ampulla and the utricle, the perilymph between these and the bony case. How will these behave when rotation takes place about an axis normal to the plane of the canal? The inertia of the liquids will tend to produce a flow through the canal in the sense opposite to that of the rotation.

Let the rotation take place so that the ampulla precedes the canal. Here the endolymph will tend to flow from the utricle into the ampulla, and thence through the canal to the utricle again. But, as Mach has pointed out, the canal has too small a bore to allow of any sensible flow through it, so that the effect of this rotation will be to increase the pressure within the membranous ampulla. But, and this is a point to which as far as I know no one has hitherto called attention, as there will also be a tendency of the perilymph to circulate, so in its circle there is also a narrow place, namely at the ampulla; for as the membranous ampulla nearly fills its bony case, there is not much room there for the perilymph to pass from the vestibule into the space surrounding the membranous canal. There will therefore be a diminution of pressure of perilymph at the ampullary end of the canal, so that the ampullary walls will be stretched by the increase of pressure within and the diminution of pressure without. Of course when the rotation is kept up uniformly for some time the pressure inside and outside of the membranous ampulla is soon equalized and the stretching or relaxation ceases. With the cessation of the stretching the sensation must also cease.

If now the rotation is stopped the perilymph and endolymph will tend to move on, and pressure will be produced inside the membranous ampulla of that canal, which during the rotation moved with the ampulla following the canal.

All this will of course be reversed when the rotation takes place with the ampulla following the canal, the pressure inside the membranous ampulla will be diminished, that without increased, and the walls will become flaccid.

In each membranous ampulla there is a so-called *crista acustica* where nerves terminate in hair cells, and it is not difficult to suppose that stretching of the ampullary walls will irritate these nerve-endings, while a relaxation of the ampullary walls will produce no irritation. If this be so, then we have three axes each with an organ sensitive to rotation about it in either sense and capable of discriminating between the two; and as every rotation of the head can be resolved into component rotations about these three axes, we have the means of perceiving the axis, and what we may call the intensity of the rotation, or perhaps more correctly the rotational acceleration.

This hydrokinetic theory of the function of the semicircular canals was propounded at very nearly the same time by Professor Mach of Prague, Dr. Breuer of Vienna, and myself. I give the names in the order of publication. The views expressed by us were not exactly the same, and the statement of the theory I have just given is any one of them with additions and corrections from the other two.

I have not thought it necessary to refer to the hydrostatical theory of Goltz, or indeed to give any details of the literature of the subject. A very full and accurate digest of almost everything that has been written on the functions of the several parts of the labyrinth of the ear has been published in Russian by Dr. Stanislaus von Stein, and translated into German by Dr. C. von Krzywicki.

The theory as I have just described it might perhaps have been developed, as I have here developed it, from a consideration of the structure and position of the canals. But as a matter of fact this was not the historical order. It was the experiments of Flourens that first directed attention to these organs as having something to do with the equilibrium of the body. In reference to these experiments and those made since by many able physiologists and skilled operators, I shall only say that the results seem to me to be consistent with the hydrokinetic theory. Certain of de Cyon's experiments, in which he increased the pressure in the canals by inserting in them small tangle plugs without producing any nystagmus or rotatory movements of the head, appear to contradict the theory. But increase of pressure in the bony canal can have no tendency to stretch the walls of the membranous ampulla, and therefore could not be expected, if the theory as I have stated it is correct, to produce a sensation of rotation; what is required is that the pressure inside the membranous ampulla should be greater than that outside of it.

The symptoms observed in cases of disease of the internal ear also appear to support the hydrokinetic theory.

But the position of the canals in close anatomical relation to the organ of hearing had impressed on the minds of physiologists so obstinate an opinion that they must be connected with the perception of sound in some way or other, that even now many will not admit that they are the peripheral organs of a sense of rotation.

A favourite theory was (and there are still some who hold it) that the semicircular canals give us information as to the direction in which sound comes to us. There are two ways in which we can show that this view is erroneous.

1st. By considering the physical conditions. The shortest sound wave which we can hear is so long compared with the dimension of the ear that every part of the ear must be at any instant in the same phase of the wave. We must assume that, as far as the effect of such sound waves is concerned, the liquid contents of the internal ear are incompressible. It is as absurd to speak of sound waves travelling round one of the canals, as to say that it is high water at one end of a dock and low water at the other, at the same time.

2nd. By experiments on the way in which we really do perceive the direction of sound. I shall describe two such experiments:—

(a) Let the observer close his eyes—for security it is best to bandage them—seat himself in a chair and keep his head steady. Now let an assistant produce a sharp short sound. In showing this experiment to Section D of the British Association at its meeting at Belfast in 1874, I used three coins in the way I show you now. The observer can tell with really astonishing accuracy whether the sound comes from the right or from the left, because he hears it louder in the nearer ear, but he is without any knowledge at all as to whether it comes from above or below, from the front or the back. He forms a judgment indeed on this point, but his judgment is usually wrong, often very ludicrously so.

The experiment is most striking when the click is produced in the mesial plane of his head, in which case he has not the binaural effect to help him. In this connexion I may say that I know no experiment which illustrates so well the marvellous delicacy of our sense of relative loudness of sound, a very small deviation from the mesial plane being quite certainly recognized.

We have then with one ear no means of ascertaining the direction of sound if we keep the head fixed. How then do we ascertain the direction of sound, for we all know that we can do so with very considerable accuracy. This leads me to the second experiment.

(b) Let the observer, still with eyes closed and bandaged, stand up and be at liberty to move his head. Let the assistant produce the clicking sound, not once only, but again and again at short intervals, always in the same place. The observer turns round until he faces the source of sound. He knows that he is facing it when he hears it equally loud in both ears, and hears it to the right when he turns a little to the left and to the left when he turns a little to the right; that is the criterion of whether the source is behind or before him. Having now got the azimuth, he seeks the altitude. Moving his head about a right and left axis he seeks the position in which he hears the sound best. He is now looking towards the source of the sound.

The concha of the external ear acts as a sort of screen, and it is remarkable how much difference there is in the qualities as well as in the loudness of most sounds with different altitudes.

Stand in front of a pipe from which water is rushing, not too near it, and move the head round a right and left axis, bow in fact to the pipe, and a striking difference in the quality and loudness of the sound will be observed in the different positions of the head.

It may be said birds have no concha, and yet they perceive as well as we do the direction of sound. But there is a method by which, without any use of the action of the concha, and by purely binaural observations, we can ascertain the direction of sound. By one observation, as already described, we can find a plane containing the line along which the sound reaches us. That plane is at right angles to the line joining our two ears. By moving the head we can shift the line joining our two ears, and by another similar observation obtain the plane at right angles to the new position of the line joining the two ears and containing the direction of sound. The direction of sound is the intersection of these two planes.

I do not think *we* use this method (although I have tried it and found it work), but we often see birds incline their heads when listening in such a way as to suggest that they use it[1].

[1] If the direction of sound is not at right angles to the line joining the two ears, one ear will be more or less in the acoustic shadow of the head. Con-

There is another objection which is often brought against the theory I have been explaining. It is said, 'Is it conceivable that there should be a special sense, common to man and all vertebrate animals, which has remained unknown till about twenty-two years ago? This is a sense invented not discovered by scientific men, otherwise we should all have known about its existence at least.'

This objection is not one to be met by contempt, it has a real basis, and as I believe this sense to be a real one, I feel bound to look for the cause of the incredulity.

A special sense is popularly understood to be a gateway of knowledge. Information as to external things comes to us in various ways, and each of these ways has from ancient time been recognized and named as a special sense. But this is not exactly the physiological way of looking at things. I may illustrate the difference by a sort of analogy. In a large business establishment the manager sits in his room upstairs. He has various ways of getting information. The post brings him letters; he looks at them: some he carefully considers and answers, others he looks at and puts into the waste-paper basket—but he has looked at them all. So we *see* things: many of the things we see we

sidering the dimensions of the head and the length of the waves of sound, it it is obvious that there must be much diffraction of sound, so that the shadow is by no means complete. Further, if the sound is not monotonic, we shall have in the shadow variation of quality of sound, just as with light which is not monochromatic we have variation of colour in the shadow of an opaque object of size comparable with the wave-length of light. This variation of quality explains why it is that we can determine the direction of sound better in the case of a noise than of a pure musical note. In the same way the concha of the external ear throws an acoustic shadow which varies with the direction of sound to the concha.

consider, take note of, others we pay no attention to, do not an hour later remember anything about them. But there are many messages which come to the business establishment and never reach the manager's room at all. They are attended to by clerks in the office. They are not futile; they are real messages, and serve their purpose, a purpose essential to the carrying on of the business. If these were not attended to downstairs the manager would very soon hear of it. So with us. There are what we may call sensory impressions which do not make their way to the conscious *Ego*, but are all the same properly attended to by what in us corresponds to the clerks. If our clerks neglect their work the conscious *Ego* very soon becomes aware that there is something wrong.

In the case of the sense of rotation ordinarily we pay no attention to its messages—the clerks at the sensory centres of the ampullary nerves and at the motor centres of the muscles of the eyeballs do all that is necessary. We perceive the result of their work in our visual sense of the fixedness of the outside world, and we do not trouble ourselves as to how the office work has been done.

But, and here I come to a matter I referred to early in this lecture, the office work is sometimes not well done, and the visual sense of the fixedness of the outside world is lost. If this is due to disease we send for the Doctor and ask him to find out what is wrong in the office, and, if he can, put it right. But there is a far more common cause of the loss of the visual sense of the fixedness of the outside world, one which it has not been left for two or three scientific men to discover in the last quarter of the nineteenth century. The most characteristic effect of alcohol is to make all reflex actions sluggish. Under the influence of a moderate dose of alcohol, what I have called the office work goes on all right, but not quite so fast as with no alcohol. The message arrives and the answer is sent, but not quite so promptly. The conscious *Ego* may not note anything wrong, but a quantity of alcohol far short of a dangerous poisonous dose may delay the transmission of the signal to the muscles of the eyeball so much as to affect quite perceptibly the compensation of the movements of the head. A perfectly sober man sees the world wag a little when he wags his head very vigourously—a point of light is perceptibly drawn out into a horizontal line of light—the office work fails a little under such extreme pressure. But a little alcohol makes the office-work fail more readily, and as the dose is increased it fails altogether, and the sense of the fixedness of the world is wholly lost. Even in such an extreme case of intoxication, short of paralysis, the drunken man may see the world steady, if only he can keep himself steady. I dare say we have all seen very drunken men walking quite straight, but with a preternatural fixedness of the head. If anything makes them move their head they totter and reel. They move the head a little; that happens to them in consequence of a small and slow rotation of the head, which happens to us when we wag our head violently, and they reel and stagger just as we should reel and stagger if we tried to walk, violently wagging our head all the time.

Just as there are blind men and deaf men, so there are men who have lost or never had the sense of rotation. Such persons are almost always deaf-mutes.

The close anatomical relation of the organ of hearing and the organ of the sense of rotation has this effect, that imperfect development or pathological injury of the one is usually associated with similar defect in the other. And experiments on deaf mutes have shown that a large proportion of them are defective in the sense of rotation. This is shown by the absence of the normal jerking of the eye-balls when they are rotated, and by a perceptible insecurity in their gait. They do not reel as drunken men do; just as blind men find their way about much better than we could do if our eyes were bandaged up, they have learned to get on fairly well with the help of experience and their other senses.

I am not sure whether in this account of the sense of rotation, of its organ, and of the use of it, I have carried all my hearers with me, and convinced you of the real existence and real practical use of this sense. I hope, however, I have made it clear that the subject is worthy of attention, and that we have here matter for the careful consideration of Physicists, Physiologists, and Psychologists.