AN ADDRESS ON OPHTHALMOLOGY IN ITS RELATION TO GENERAL MEDICINE.

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II.

As mentioned in the introductory remarks, the study of paralysis of the ocular motor nerves is of importance, not only for immediate utilitarian purposes, but for discipline. The indirect value of the facts with which ophthalmic surgeons have supplied us from their laborious research into these cases, for the elucidation of some very difficult problems in general medicine, is very great indeed. It is almost solely in this regard that I intend to speak of them. I begin with vertigo from paralysis of ocular muscles.

Although it is well known to ophthalmic surgeons that vertigo is one of the most important symptoms of paralysis of an ocular muscle, this is certainly not known widely in the profession. Graefe says that its non-recognition has often led to medical errors. It is common for the inexperienced, who do not know it, to overlook so simple a cause of this symptom and to treat the patient for some central affection. The case does not always diagnose itself. The patient who has paralysis or paresis of an ocular muscle or muscles, does not always come complaining of double vision, but sometimes giddiness and irregularity of gait, or confusion of sight. In cases of slight weakness of the external rectus, or when the superior oblique is the muscle paralysed, the palsy is by no means obtrusive; there is no ordinary strabismus; and in such a case the giddiness may be with fallacious ease attributed to some central lesion, to disease of the cerebellum for example; or it may even be put down to disease or temporary disorder of a part of the body so distant as the liver. Even when ocular palsy is obtrusive, as when the sixth nerve is completely paralysed, it may happen that the attendant vertigo is erroneously put down, not to that palsy itself, but to some supposed *additional* lesion of a nervous centre. This is attributing a symptom to a complex state of things, from overlooking a simple and obvious cause. I speak strongly here, because in former years I have erred from not knowing the real mode of causation of vertigo in ocular paralysis. In any case of vertigo, having excluded obvious possible causes, we should not be content with an ordinary examination of the movements of the eyeball, but should test the patient by coloured glasses and candle in a darkened room.

Apart from the obvious clinical value of the fact, that ocular paralysis causes vertigo, the cases help us to a demonstration of the physiology of vertigo, and again to a knowledge of the state of things in disorder of co-ordination. To study the nature of vertigo, we must take a very simple case.

The simplest ocular palsy we can study for this purpose, is that of the external rectus muscle; for this palsy is the simplest of all palsies; but one nerve is affected, and but one muscle. As that muscle moves the eyeball, the defects from its palsy are easily and also precisely studied. The movement which the muscle effects is, unlike that of some other ocular muscles, uncomplicated: it moves the eyeball directly outwards. For still further simplicity, we take a case in which this local palsy is the sole paralysis, and that it depends on lesion of the *trunk* of the sixth nerve, as it mostly does; we shall suppose it to be of the right side.

Let us remark, first of all, that the vertigo occurring with this, or with any other variety of ocular palsy, is *not* owing to diplopia, as it is very often supposed to be. My attention was first drawn some years ago to the true explanation by Brudenell Carter, and I have in very many ways profited by this knowledge. The vertigo is due to a wrong estimation of the position of external objects by the one eye, whose muscle is paralysed. This is the really important matter for our consideration. There is what is technically called "erroneous projection". This can be proved by a very simple experiment. In our supposed case of palsy of the right external rectus, the patient (his left eye being closed), when told to strike quickly an object held to the right of the middle line, strikes beyond the object. When the patient tries to turn the eye outwards, Helmholtz says: "The organ no longer obeys the will; it stops midway, and objects appear to displace

themselves to the right, while the eye and the retinal images produced there do not change position."

The patient judges of the position of the external object by the effort he makes to bring the eye to bear on it, not by any movement really accomplished. The patient judges, to use the language of Bain, by the "outgoing current"—*out-going*, but, be it observed, not *muscle-reaching*. The eye does not, or, to use Helmholtz's expression again, the retinal images do not, change position. The patient judges by the activity of the centre for the movement. Thus, to speak metaphorically, *the hand is misinformed* as to the position of the object, and, therefore, strikes beyond the object; or, we may say, if so incoherent a simile can be excused, "the hand makes a false step". It is not difficult to understand how a similar dis-harmony betwixt the ocular and wider locomotor movements can cause vertigo or actual reeling. During walking, there would be many real "false steps", from acting on the untrustworthy estimates of positions given by the centre for the impossible movement of the paralysed ocular muscle. Even in the simple experiment just mentioned, the patient will sometimes not only miss the object on his right, but will actually *stumble* to that side.

The facts of ocular vertigo have long seemed to me to give actual demonstration that vertigo is on its physical side a motor symptom. I find that Russell Reynolds had previously come to the same conclusion. My colleague Dr. Gowers has recently adopted this view. The facts mentioned show that vertigo is, physically, a disorder of co-ordination; that it is itself incipient or actual reeling. It is, I submit, inaccurate to say—in a case of disease of the cerebellum, for example—that vertigo *causes* disorder of co-ordination; it is itself, on the physical side, disorder of co-ordination. In this opinion I am glad to be able to say that Ferrier agrees with me. The only difference betwixt vertigo without outward manifestation and vertigo with the outward manifestation of a reel is one of degree; in one there are slight central changes, and in the other strong central changes, leading to peripheral effects. The difference is, I think, strictly analogous to the physiological difference betwixt speaking internally and externally; there is only a difference in strength of the central excitations or discharge.* The statement that vertigo is, on its physical side, a disorder of co-ordination, incipient or actual, seems strange only because the *sensation* of vertigo is often confounded with the *nervous process* occurring along with it. A sensation, that is, a state of consciousness, is often spoken of as being the same thing as a state of a sensory nerve; just as a horse-chestnut might be supposed to be the same thing as a chestnut-horse. The sensation of vertigo differs as much from the physical process occurring along with it as colour does from the physical process occurring along with it. Unfortunately, it is not the habit to analyse carefully betwixt abnormal states of consciousness and the physical conditions accompanying them. Thus pain is said to produce this or that physical effect. There is no more objection to the popular use of this expression than there is to the statement that the sun rises in the east. But the use of the word sensation, both for a state of consciousness and for a state of nervous centres, produces the same kind of confusion as the use of the expression that the sun rises in the east would do in an exposition of the movements of the solar system.

Considering the extreme simplicity of the facts with which ophthalmic surgeons have supplied us, it is surely somewhat perverse to study the more difficult cases of auditory vertigo, and the most difficult case of epileptic vertigo, before we have studied ocular vertigo. We can make a patient, who has complete paralysis of the third nerve, giddy and reeling, when we like. We have only to make him close his sound eye, and keep the paralysed lid upheld by a piece of sticking plaster, and let him walk; he tells us that he feels giddy, and we see him reel more or less. Starting here, we can next investigate, with more hope of success, auditory vertigo.

It is a very interesting fact, that Cyon has found that experimental irritation of the semicircular canals produces ocular deviation, each canal its own deviation. And Ferrier finds that irritation of what he calls the auditory centre, in the cerebrum of the monkey, produces movements of the eyes to the opposite side, with dilatation of the pupil. These facts are very significant, and enforce what has been said as to the desirability of making ocular vertigo the starting-point of our investigations into vertigo in general. As to epileptic vertigo, the demonstration that vertigo is a motor symptom is of vast importance; for it helps to show a real relationship betwixt epileptic vertigo and convulsion; it shows that epileptic vertigo is, not only clinically but physiologically, a minor degree of convulsion. Here, again, we must note the distinction between a mental state and a nervous state. There is no relationship whatever possible betwixt epileptic vertigo, considered, as it commonly is, as a defect or loss of consciousness, and convulsion;

* The peripheral effects are due to the development of movements of muscles, usually associated with the paralysed muscle (*vide infra*).

but there is one betwixt convulsion of the eyes, or discharge of the ocular motor centres (the physical side of epileptic vertigo), and general convulsion. Moreover, Hitzig's experiments, by passing galvanic currents through the head, show that there are all degrees, from a feeling of vertigo only (that is, with weak central discharge only) to vertigo with actual turning to one side, that is, with central discharges strong enough to produce peripheral effects.

Not only do we learn from ocular palsies the nature of vertigo, but we illustrate a very important principle in physiology, and in that part of physiology which corresponds to psychology. The facts of ocular vertigo, with other allied facts from other departments of medical practice, throw light on psychico-physical operations. Speaking generally, they help to show that ideas arise, not only during energising of sensory centres, which every body admits, but during energising of motor centres, which scarcely any body seems inclined to believe. We have seen that the patient judges of the positions of objects by central discharges, which do not actually displace his retinal images. Now, the extension of an object is really made up of relations of innumerable positions. To give a particular illustration, the facts of ocular vertigo are of inestimable value in supporting the doctrine, that the anatomical substratum of a word is a motor, an articulatory process.

The scepticism with which such a statement as the last is received is for the same reason that vertigo is considered to be not a motor but a sensory symptom. It depends on the confusion of sensations—states of consciousness—with states of sensory nerves—physical states. I do not say that a word is a motor process, but that the anatomical substratum of a word is. Such a conclusion has been reached by Bain and Fournié deductively; it has long since seemed to me that on this basis only is possible the explanation of all the symptoms in cases of aphasia: recently, by a totally different kind of evidence, Ferrier has come to an identically similar conclusion. Surely, *à priori*, there is no more difficulty in believing that a mental state can occur with an outgoing than that it can occur with an incoming nervous current. Why should the mere direction of the current make such a marvellous difference? There is no more reason, *à priori*, why the idea of a word should not arise in consciousness during a nervous discharge going to or towards the articulatory muscles, than there is that colour arises, as everybody admits, during a discharge of a centre receiving retinal impulses.

Were it only for its bearing on the interpretation of aphasia, ocular vertigo is well worthy of careful study. There are other facts supporting the doctrines advanced.

In some cases of palsy of the ocular muscles, the size of objects is altered, as in paralysis of the superior oblique and in cases of weakness of the internal rectus. There is a similar phenomenon in certain simple experiments on the eye. If, so to speak, we impress the retina with a scarlet object, we find that the after-image alters in size with accommodative efforts. Now, in this instance, the retinal area affected is unaltered; there is a motor change only. We can also alter the shape of objects to some extent; if we obtain an after-image of a circle and project this on to an inclined sheet of paper, the spectral circle becomes oval. This statement I owe to an American ophthalmic surgeon, whose name, I regret to say, has escaped me. Moreover, a sensation which is of entirely central origin is alterable in size by accommodation. A medical man, the subject of migraine, has, at my request, noticed the alteration in size which he can make in the visual spectre which precedes his headache by altering his accommodation. When he looks at a book at reading distance, the spectre is of about the size of a shilling; at a distance of eight or nine feet, it is about two feet in diameter. In some cases of epilepsy, and particularly in those in which loss of consciousness is very early in the paroxysm, there is a warning by alteration of size of objects; mostly they get bigger and nearer; one expressed it that the walls of the room seemed to come nearer. In the scientific investigation of the epileptic paroxysm, the warning is the most important thing. It is quite as important to study warnings of alteration of shape as warnings by projection of colours; they are exactly correlative motor and sensory symptoms. In all such cases, *post mortem* careful search of those spots which Ferrier declares to be respectively the special centre for movements of the eye and the visual centre should be made.

In all these cases, except possibly in the cases of epilepsy, the retinal area affected, or the number of central elements affected, corresponding to a given retinal area are unaltered; they show that estimation of size of objects depends on activity of a motor element only. There is more than this; these are facts showing that activity of motor centres causes alteration of the size of objects when actual movement is impossible or slight. Thus, in paralysis of the ciliary muscle, either by atropine or in disease, objects appear smaller. These facts, like those given when speaking of vertigo, show plainly, I think, the inseparable connection of motor activity with sensory activity in ideation.

They show that, whilst the colour the secondary or dynamical quality of an object is a sensory affair, its size and shape, its primary or statal quality is a motor affair. If so, then ophthalmological facts are of inestimable value not only as bearing on an important psychological problem, but as bearing on the most important of all questions whatever in psychology; they demonstrate that the estimation of the extension of objects is due to motor activity; and they show that activity of motor centres will suffice; and thus they lend support to the doctrine that, in remembering the shape of an object, there is slight activity of central motor centres as much as there is slight activity of sensory centres in remembering its colour. This general conclusion, reached before Hitzig's and Ferrier's researches, is in absolute harmony with them.

There is evidence quite independent of that already stated supporting this doctrine. Weir Mitchell has found that faradising the ends of nerves in stumps makes the patients feel "movements" of the lost parts which used to be supplied by the nerves, the amputated ends of which are irritated in the experiment.

My friend Dr. Sturge has supplied me with an illustration of ideal movements occurring during central discharge in a case of hemiplegia. There were ideal movements of parts which were really immovable. "A man suffering from left hemiplegia, with scarcely any recovery of power in the arms, marked rigidity of arm and hand, no affection of sensation. Frequently, when in bed, or at other times, the patient endeavours to exercise his paralysed muscles. In the case of the hand, he endeavours to open and close the fingers, being unable in reality to perform any movements with the hand, which is constantly flexed on the forearm, with the fingers firmly closed. In doing this, he has many times felt exactly as though the fingers were in reality opening and closing to their full extent; 'would take my affidavit that this is so'. This has so surprised him, that he has put the other hand down to see whether the fingers really were moving, but he has always found them flexed firmly in their usual position, and he has felt them remain in this position with his right hand during the very time that he has had the sensation in the hand itself that it was opening and closing. On one occasion, he called out to one of the other patients that he was able to move his paralysed hand, but this patient has told me that, when he went to look, he found the hand in its usual state, and devoid of all movement."

One reason for alluding to this case is, that I understand that Professor Bain has suggested that ideal movements would be found to occur in some cases of hemiplegia if inquired for; in this case, the facts stated by Dr. Sturge were volunteered by the patient; the movement was not the one often observed when a hemiplegic man yawns.

To resume the facts, we stated, when speaking of ocular vertigo, those of which we called deceptive as to size in paralytic states of the ocular muscles, and of alteration of size of after-image in accommodative efforts, of alteration of size of a migrainous spectral image, the facts of Weir Mitchell's experiments, and those of Dr. Sturge's observation, all point to one thing—to the truth of Bain's doctrine of the "out-going current"; and, as it appears to me, they give a demonstration that energising of motor as well as of sensory centres occurs during ideation. In particular, they supply powerful indirect evidence that the anatomical substratum of a word is a motor, an articulatory process. Weir Mitchell has independently reached the same conclusion as Bain, but his facts have been explained by the hypothesis that the ideal movements were simply owing to the patient's imagination. Supposing this so-called explanation were acceptable in a popular way, it might still be affirmed that the physical state during the imaginative state was an activity of motor centres. Physiologically, the imaginative explanation is of the same value as the explanation given of the inability of some aphasics to speak, viz., that they do not speak because they have lost the memory of words; these explanations are not physiological, they are attempts to explain physical conditions by psychical conditions; in reality, they explain nothing.

[To be continued.]

MARYLEBONE.—Dr. Whitmore states in his report for the five weeks ending December 2nd, that there were 531 births and 341 deaths registered, which were equivalent to an annual death-rate of 22.26 per 1,000 population. He also reported that, whilst small-pox was spreading in a similar manner to what it did in 1871, the revaccination of adults was almost entirely neglected. As regards primary vaccination, there was no reason to find fault, as the percentage is gradually increasing. There had been three deaths from small-pox, and two of these had occurred amongst unvaccinated persons. Dr. Whitmore gives analyses of water which are scarcely satisfactory, and tables of the sanitary work performed, which indicate a fair amount of work for the time of year.