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GEORGE A. BERRY

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ON SO-CALLED PARESIS OF DIVERGENCE

ALTHOUGH I have seen a few similar cases, I shall, in order to be as brief as possible, base my remarks on the subject of this communication on the following case :—

James Ancrum, aged fourty-four, a previously healthy man who neither smokes nor drinks, suddenly developed a diplopia on the morning of the 16th September last. He is employed on night duty at the Edinburgh Gasworks, and principally in regulating pipes, metres, etc. His family history is good and unimportant.

He was first seen by me at the Royal Infirmary three days after the occurrence of the diplopia. He stated that in the interval he constantly saw double. In the mornings, according to his wife's statement, he had quite a marked convergent squint. This passed off in the course of the day—after rest. Being always at work at night, he slept during part of the day.

On examination: there was no apparent interference with the associated lateral or vertical movements of the eyes. On distant fixation there was manifest convergence.

In addition to this history of a marked manifest squint, which existed in the morning and afterwards diminished to such an extent as to be unnoticeable, the further examination brought out several points of interest.

The convergence on distant fixation, tested with a candle flame and red glass in front of one eye, equalled about three metre angles (*i.e.* convergence to a point lying about one-third of a metre or thirteen inches from the eyes). Any abnormal convergence was not particularly apparent on mere inspection. The diplopia was corrected by a prism of 22°.

At the reading distance there was much less excess of convergence manifest. Indeed, there did not seem to be constantly any diplopia at all. This could always be elicited, however, with the flame and red glass. Yet there existed a high degree of latent convergence. Either eye, if occluded, at once deviated inwards in a most marked manner.

Tested for objects at the distance of six to eight feet, the double images were found to be considerably closer together on fixation to either side than on looking straight forwards. The latter separation was also somewhat less when the eyes were turned upwards, and somewhat greater when they were turned downwards.

The power of fusion was good. The diplopia could be corrected by the same prism at different distances, provided the object fixed was not too rapidly moved either towards or away from the patient. This was even found to be the case for the fixation of a candle flame with a red glass in front of one eye.

The treatment consisted in complete rest with bromide of sodium. After four days all spontaneous diplopia had disappeared. There was still some latent convergence, though very much less than formerly. Diplopia could also be elicited with a red glass, and this was corrected by a prism of 12°. After a week only a trace of latent convergence could be made out by means of a vertically

refracting prism. The patient then returned to work, and there has since been no recurrence of the symptoms.

The case was evidently not one of the sudden manifestation, owing to abolished or restricted fusion power of a previously existing latent convergence. This cause, though in my experience far from frequent, undoubtedly accounts for the sudden appearance of some concomitant squints. I have only seen this, however, in young people. The good fusion existing throughout, and the eventual loss of latent convergence, clearly pointed to some other cause. The character of the diplopia again showed that no paresis of any of the ocular muscles existed. The diagnosis was therefore restricted to either *convergence spasm* or *divergence paresis*. This was in fact an example of an affection which has recently received some attention and been generally described as *paresis of divergence*.

In some respects it might seem immaterial whether such cases are regarded as spasm of convergence or as paresis of divergence. The effect on convergence might well be considered to be the same in either case. Yet it is not only of interest, but surely of some clinical importance, to attempt to make a distinction between a state of irritation leading to the stimulation of some nerve centre, so that its regulation is withdrawn from normal control, and a curtailment of power by which, notwithstanding the existence of a normal impulse, the effect falls short of what takes place under conditions of health.

A curious feature in these cases is the diminution in the distance apart of the double images on lateral fixation. Evidently if both external recti were paretic, the images, instead of being nearer together on looking to either side, would be further apart. The fact that the reverse is the case has been considered as diagnostic of paresis of diver-

gence. Certainly, on the assumption that there is any weakness in the innervation of the external recti, a weakness in their lateral associated contraction must from this symptom be excluded. This fact seems, however, to be the only ground for assuming that a weakness in their divergence innervation must therefore exist.

Before offering an explanation for this relation of the double images on lateral fixation, it is necessary first to consider a point which is mainly mechanical. We must inquire

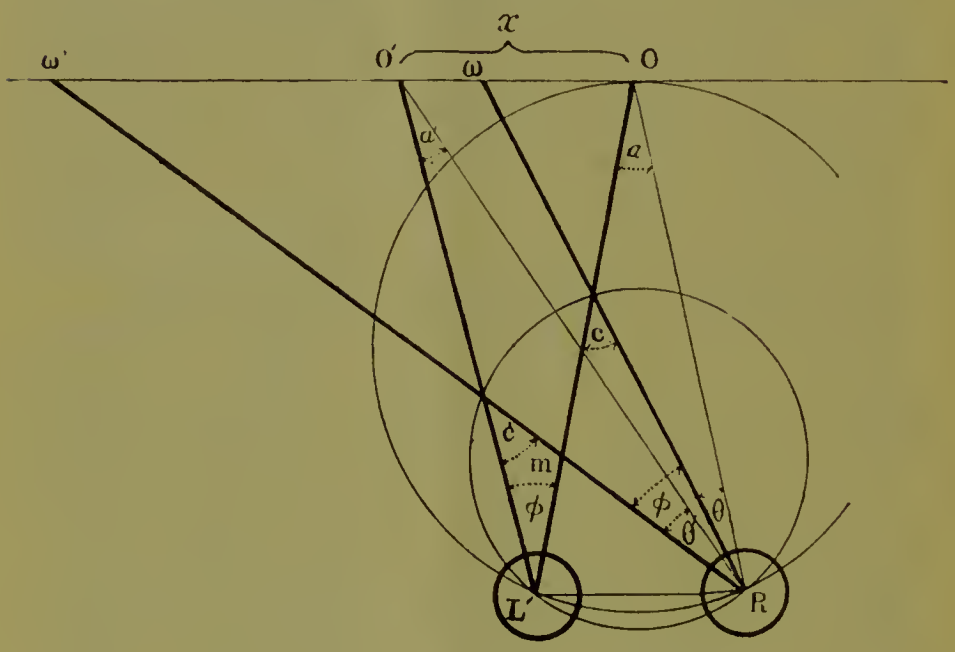

FIG. 1.

what must be the double image relations on the assumption that the same degree of over-convergence is maintained as the eyes move from side to side. Does the same approximation of the images on lateral fixation as characterises the affection with which we are now dealing, result as a consequence merely of concomitance? It is easy to show that this is not so. Let *O* (figs. 1 and 2) be an object in the middle line. Let us suppose that in attempting to fix *O* it is impossible to do so binocularly owing to the tendency to

over-convergence. One eye alone is then used for fixation, while the other deviates in a degree corresponding to the over-convergence. Suppose the left to be the fixing eye. The visual axis of the other then cuts the line of fixation at *c*. If the object be moved to *O′* both eyes must move laterally through the same angle ϕ, on the assumption that the same degree of convergence is maintained between their axes. The left being still the fixing eye, their axes would now meet at *c′*.

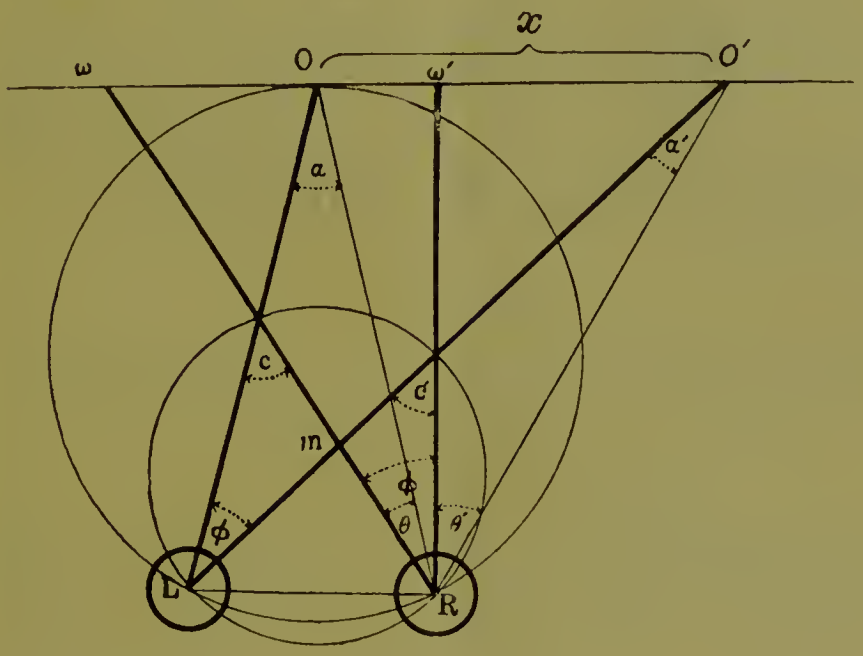

FIG. 2.

In the triangles *Rmc* and *Lmc′* the angles ϕ and *m* are equal, therefore the angles *c* and *c′* are also equal. In the triangles *LRc* and *LRc′*, the angles *c* and *c′* being equal, it follows that *L*, *R*, *c* and *c′* are points on a circle—the circle of equal convergence. Now prolong *Rc* and *Rc′* till they meet the line or circle along which the fixation object is moved at ω and ω'. Then join *OR* and *O′R*. We have then two angles θ and θ' which are related to each other in the same manner as the angular excen-

tricities of the misdirected right eye's retinal images, for fixation in the middle line and laterally, respectively. If, therefore, the angles θ and θ' are unequal, the images of O and O' will be at different distances from the fovea of the right eye and be projected accordingly upon the fixation plane or circle, the one further than the other from the true image of the left eye.

The figures enable us to demonstrate geometrically the relation $\theta : \theta'$ in a general way.

The angle a' is less than a, because O' lies outside the circle of which LR is a chord and which passes through O. In the triangles RcO and $Rc'O'$ the external angles c and c' are equal to the two internal and opposite angles respectively, $a+\theta$ and $a'+\theta'$. We have then:

$$c - a = \theta$$
$$\text{and} \quad c' - a' = \theta'$$

but as $c=c'$ and a is greater than a', it follows that:

$$c' - a' \text{ is greater than } c - a$$
$$\textit{i.e.} \quad \theta' \text{ is greater than } \theta.$$

The false image in the right eye must therefore be more excentric on fixation to the side than when the object looked at lies in the middle line in front of the eyes. The excentricity, too, will be greater for objects moved along a plane at right angles to the face than for those moved in a circle, as on the perimeter. Only for objects moved along a circle passing through O and the centres of rotation of the two eyes is the excentricity the same in both cases.

A constant degree of convergence should therefore sometimes be associated with a greater and not a less degree of separation of the double images to the side.

It may easily be shown that this difference between θ' and θ is greater the smaller the angle of convergence at c is (*i.e.* the greater the circle of equal convergence) and the nearer the plane along which the fixation object is moved is situated to the circle of equal convergence.

Indeed, only under such circumstances as considerably increase the difference could the greater separation of the double images on lateral fixation be appreciable. For a test made at the distance of ten or twelve feet and more, and any marked degree of over-convergence, the separation would be practically the same for lateral as for central fixation.

A geometrical proof of this kind, although it enables one to demonstrate the greater excentricity of the deviating eye's image on lateral fixation, does not afford data for the calculation of the increase of excentricity under different conditions, and, therefore, of how far the extra excentricity is likely to be appreciable. I therefore append a more complete analytical proof for the case where the object of fixation is moved along a horizontal line in a plane parallel to the vertical plane through the centres of rotation of the two eyes. With the same lettering as in the figures, and putting D for the perpendicular distance from the eyes of the plane along which the object of fixation is moved, d for half the intra-ocular distance, and x for the variable distance OO' we get—

$$\alpha = 2\tan^{-1}\frac{d}{D}$$

$$\text{and } \alpha' = \tan^{-1}\frac{x+d}{D} - \tan^{-1}\frac{x-d}{D}$$

$$= \tan^{-1}\frac{2dD}{D^2-d^2+x^2}$$

$$\text{but } \theta = c-\alpha$$

$$= c - 2\tan^{-1}\frac{d}{D} \quad . \quad . \quad . \quad . \quad . \quad 1.)$$

$$\text{and } \theta' = c-\alpha'$$

$$= c - \tan^{-1}\frac{2dD}{D^2-d^2+x^2} \quad . \quad . \quad 2.)$$

From 2.) it is evident that when x increases (*i.e.* when the fixation object is carried further and further from the middle line) θ' also increases. For $x=0$ (coincidence of O and O') $\theta'=\theta$, as $\tan^{-1}\frac{2dD}{D^2-d^2}=2\tan^{-1}\frac{d}{D}$.

1.) and 2.) also show that the proportion $\frac{\theta'}{\theta}$ is greater the smaller c is. The less marked, therefore, the over-convergence for fixation in the middle line, the more appreciable should be the increased separation of double images on lateral fixation.

Whilst there can be no doubt, therefore, that the maintenance of a constant degree of convergence would cause a greater separation of the double images on lateral fixation, we must next determine whether the amount of this difference is sufficiently great to be appreciable under circumstances which ordinarily present themselves, and under conditions in which the test is usually made. To get at this we have to calculate the distances along the horizontal line cut by lines from the eye, forming angles θ and θ' with RO and RO', but on the opposite side from those shown on the figures, as these new lines represent the directions of projection.

I have already referred to the conditions which determine whether the difference on medial or lateral fixation is likely to be appreciable or not. I add here one example. For fixation on a plane about twenty inches distant and convergence towards thirteen or fourteen inches, the double images in the middle line would be rather over an inch apart, and for fixation 30° to the side about $1\frac{3}{4}$ inches. This difference would no doubt be quite appreciable.

Under all circumstances, however, retention of a permanent degree of convergence would at least not cause the images on lateral fixation to appear nearer. If

there were any appreciable difference they would, on the contrary, appear further apart.

When, therefore, the images are closer on lateral fixation, it shows either that the right eye image is projected on to a plane nearer to the eyes when a lateral object is fixed than when fixing a central object, or that *convergence has decreased.* The former assumption is one which there does not seem to be any reason for entertaining. We must therefore infer that in spasmodic convergence or so-called paretic divergence, the tendency to over-convergence is not so firmly established, that it is incapable of being lessened under conditions in which equally strong convergent movements are not habitually called for.

It is interesting to observe that although on the assumption of a constant degree of convergence the excentricity of the image in the deviating eye, of an object fixed by the other eye, may increase, but never diminishes, on lateral fixation, this is not the case if convergence, while always excessive so far as the requirements of binocular fixation are concerned, is altered, on lateral fixation, in a manner which may be looked upon as habitual. Let us assume, *e.g.* that as the eyes move to either side, the degree of convergence diminishes to the extent necessary to maintain the fixation of objects in a plane near to and parallel with the eyes (*e.g.* the page of a book). The relations of the excentricity of the deviating eye's images, when the other fixes an object centrally or laterally placed on a plane parallel to this one, but at a greater distance from the eyes, may then be deduced from the accompanying figures (figs. 3 and 4). Here, although there is some difference according to which of the two eyes fixes to either side, we have

$\theta = A - \alpha$ and $\theta' = A' - \alpha'$. For anything beyond ten feet distance of fixation, at any rate, α and α' are so small that they may be neglected. Practically, therefore, θ then $= A$ and $\theta' = A'$. And as A is always, on looking to the side of the fixing eye, and also, except for small lateral displacements on looking to the other side, greater than A', θ is generally greater than θ'. With any ordinary degree of over-convergence the diminution of θ' for distant

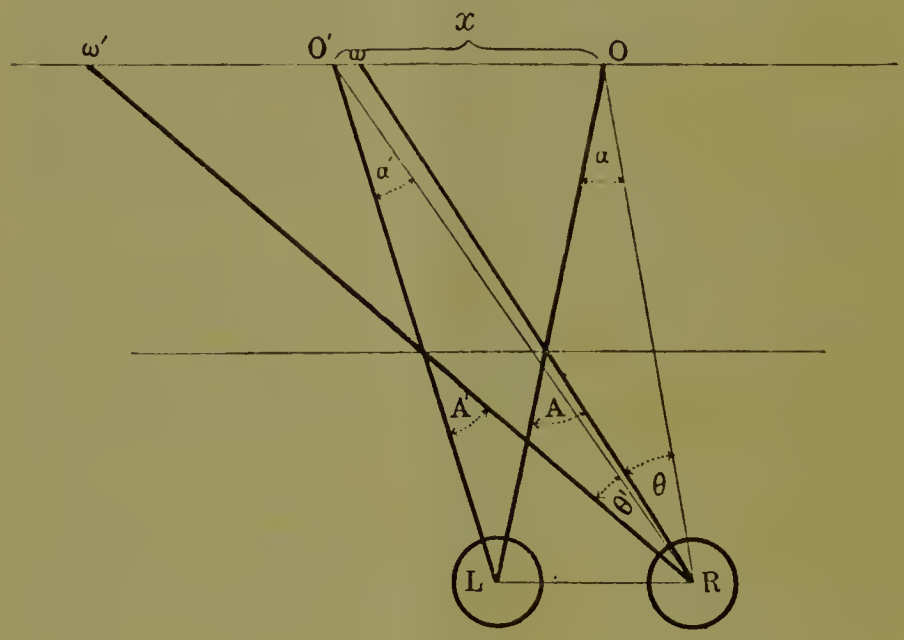

FIG. 3.

fixation (ten feet or more) would be readily appreciable. On fixation, 30° to either side, the double images would appear distinctly closer together than on median fixation.

With the same notation as before, and putting D' for the vertical distance of the nearer plane from the line joining the rotation centres of the two eyes, we get as the general value of θ' for varying values of x:—

$$\theta' = \tan^{-1} \frac{2dD'}{D'^2 - d^2 + \left\{\frac{D'}{D}\left(x \pm d\right) \mp d\right\}^2} - \tan^{-1} \frac{2dD}{D^2 - d^2 + x^2} \quad \ldots \quad 3.)$$

On differentiating the two angles in 3.) with respect to x it is found that the rate of change with increase of x is much greater in the first (A') than in the second (α'). Approximately,

$$\frac{\frac{d}{dx}A'}{\frac{d}{dx}\alpha'} = \frac{D}{D'}$$

The maximum value for θ' is therefore got for the value of x which makes $\frac{D'}{D}(x \pm d) \mp d = 0$, *i.e.* $x = \pm d\left(\frac{D}{D'} - 1\right)$. This is also evident from the figures, as then $A' = \tan^{-1}\frac{2dD'}{D'^2 - d^2} = 2\tan^{-1}\frac{d}{D'}$.

In fixing with the left eye then, for instance, if the fixation

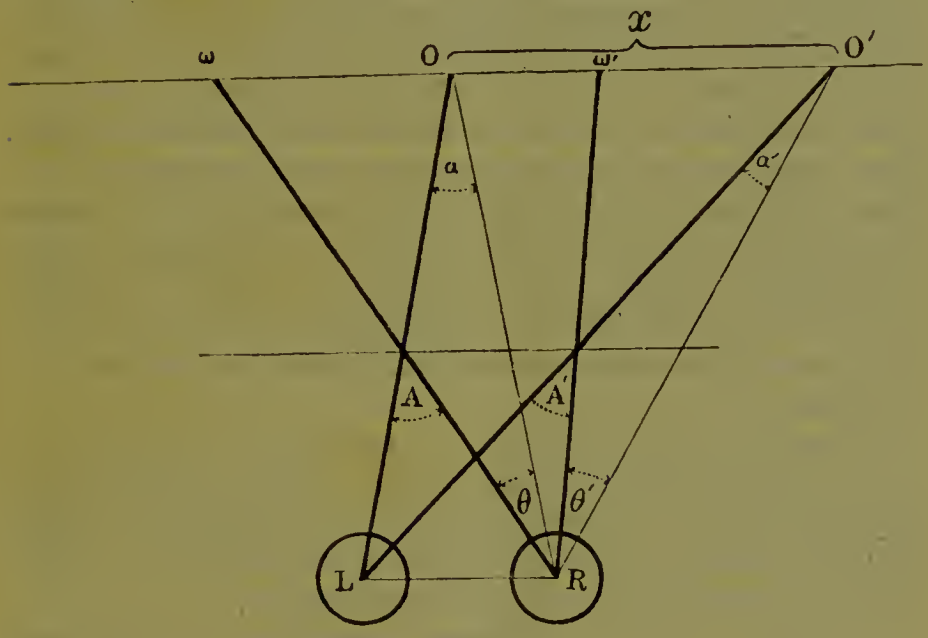

FIG. 4.

object be carried to the left, θ' diminishes constantly as x increases, while if the object of fixation be carried to the right θ' first increases until $x = d\left(\frac{D}{D'} - 1\right)$, and then diminishes, and *vice versâ* for fixation with the right eye.

An equal change in the angle to either side, and a consequent equal change in the degree of separation of the resulting double images would only be got when one eye fixes to one side and the

other to the other side, while the greatest and continuous decrease of separation would take place when the right eye fixed objects to the right, and the left eye objects to the left, probably a less frequent condition than the opposite, which is the rule in ordinary alternating concomitant strabismus.

It seems not improbable, when we take into consideration the fact of the approximation of the double images on lateral fixation in cases like the one here recorded, that such a retention of the habitual association which occurs between convergence and lateral fixation is actually, or at all events approximately, what takes place.

This consideration surely too points to *spasm of convergence* as the cause. We can readily understand that the effect of an irritation of the converging centre might be to withdraw the relaxation of convergence beyond a certain point (a point which may, and no doubt does, vary from time to time) from the control of the will, and yet to leave the play of convergence, as the eyes move from side to side, to be effected in a habitual manner. This would be similar, in fact, to the natural convergent movements which would take place if, on attempting to fix a more distant object moved from side to side, the eyes kept converged on points in a plane which lay nearer to them. This, while consistent with the idea of spasm, appears to me a much less likely state of matters to co-exist with anything of the nature of paralysis.

I have already stated that there was no obvious reason why the peculiar relations of the double images should to some appear in favour of the diagnosis, paresis of divergence. If they have any bearing at all on the diagnosis, it seems more reasonable to infer that they suggest spasm of convergence.

In support of such a diagnosis, however, it is perhaps better to rely upon arguments afforded by the clinical facts which are afterwards referred to.

The effect of habit indeed often asserts itself more or less even where the conditions are peculiarly unfavourable. Just as we often see in the case of abduceus paresis that the resulting double images are further apart on downward or upward fixation, because of the habitual associa-

tion of convergence with downward rotation and the absence of any call for convergence when the eyes are directed upwards, so we find a degree of abnormal convergence lessen when associated with wide lateral movements, as there is not habitually as great a degree of convergence associated with marked lateral fixation as for central fixation. In the case here referred to, both these effects of habit were evidenced.

It has sometimes been questioned whether the increase of convergence on looking downwards, which is common, more or less, to all forms of pathological convergence, may not be dependent solely upon anatomical conditions. That this is *not* the case I have shown by an examination of a number of individuals, as to the influence of downward and upward rotation of the eyes on the degree of what may be looked upon as physiological latent lateral deviations. The result of this investigation was communicated to this society. The cause is therefore, no doubt, as was first maintained by v. Graefe, the influence of habit.

The question as to whether this curious and rare form of strabismus is the result of convergence spasm or of divergence paresis, is not, perhaps, one which the consideration of such cases alone can enable one to decide. As the symptoms might be supposed to be the same in the one case as in the other, the proper conclusion to draw, apart from other considerations, is, as it appears to me, that the diagnosis, divergence paresis, is not justified. Such a diagnosis involves the admission of a divergence innervation which for many reasons is at least problematical. I have seen the same condition of suddenly arising convergent strabismus followed, after a short time, by a persistent associated lateral deviation of the eyes of an undoubtedly spasmodic nature, which

has also disappeared after some time. One case of this kind I communicated to this society. Such an association is more than suggestive of spasm. In the present case, the very curious great excess of latent over manifest convergence for near fixation would also seem to point to spasm as the cause of the symptoms. On accommodation for a near object, with one eye occluded, the tendency evidently existed to association with an undue degree of convergence. This too was presumably the result of an abnormally excitable state of the convergence centre. It was, however, counteracted by fusion the moment binocular fixation became possible.

This tendency to over-action is also seen sometimes in what is known as spasmodic myopia. In my experience there is no such thing as a constant spasmodic myopia. The nature of such cases is this: that whenever attempts at accommodation are made, they result in a degree which is in excess of the requirements. Often the subjects are slightly hypermetropic, and therefore have to accommodate to see smaller objects at a distance. This tendency is probably a purely innervational one, and seems analagous to the excessive latent convergence in the case which I have just described.

Apart from the above considerations, which point to spasm of convergence instead of paresis of divergence, there are, as I have elsewhere pointed out,[1] many reasons for altogether disbelieving in a real divergent innervation, *i.e.* one starting from what, from analogy, we might call a divergence centre.

The common conception seems to be that if one admits the existence of a convergence centre, there must surely be a divergence centre as well. There is a centre for

[1] *Ophthalmic Review*, October 1893.

associated eye movements to the right, and one for movement to the left, and these are antagonistic. And indeed all analogy would point to the arrangement of innervation centres in antagonistic pairs. I long ago pointed out that there were physiological and clinical reasons for rejecting the view that the cessation of convergence was brought about by *active* divergence, and therefore for concluding that no diverging centre existed.

Divergence would thus constitute an exception to the rule which obtains in the body generally, as well as an apparent exception to what takes place in regard to the lateral movements of the eyes. I say apparent, because the relative divergence which takes place on the cessation or relaxing of a converging impulse is no doubt effected in the same way as is the return of the eyes to the middle line on the cessation of an impulse which causes them to rotate to one side. The divergence, on the one hand, is due to the passive return of the external recti to the length which corresponds to their tonic state of innervation after the degree of greater stretching to which they have been subjected is removed. The return to the middle line of eyes laterally displaced, on the other hand, is due to the passive return of the associated internus of one eye and externus of the other to the physiological states which they have in virtue of their tonic innervation. An active innervation is not called for until rotation beyond the middle line in the opposite direction is begun.

This view of the matter was also entertained by the late Professor Alfred Graefe. In his admirable monograph on the anomalies of the ocular muscles in the second edition of the Graefe-Saemisch handbook he says: 'One can only agree with Berry when he rejects

as superfluous the assumption of the existence of a special divergence innervation, and consequently of a definite centre for this innervation.'

I shall content myself by repeating here only two arguments which can be adduced against the existence of a divergence centre. It is evident that divergence beyond parallelism of the axes is never a physiological requirement, and therefore that the only imaginable use for a divergence innervation could be to cause the return of the eyes to a position of parallelism from that of convergence. Like convergence, divergence must be primarily regulated by fusion, and as fusion is not called for with diverging axes, no habit of absolute divergence can be naturally acquired. Yet it surely ought to be possible to induce further divergence in the interests of single vision by means of abducting prisms were there an active centre for divergence. In the case of convergence, one finds that very little practice is required to overcome adducting prisms of a strength which cause a very much greater degree of convergence than is ever called for physiologically. No doubt abducting prisms can also be overcome so that it *is* possible to have an absolute divergence. But there is a limit to the degree of this absolute divergence even after prolonged exercise with prisms. This limit coincides with the degree of divergence which is assumed when there is a complete loss of any converging power. In other words, the limit of possible divergence is the same as the starting-point of convergence. This is the physiological reason for disbelieving in a divergence centre.

A more important, more convincing evidence is afforded by certain conditions of paralysis. For instance, cases occur in which there is a complete loss of power of

associated movement to one side, and even (though they are extremely rare) to either side, without any interference with convergent movements, and with return to parallelism on convergence being relaxed.

It might be argued, however, that just as the interni, though not receiving an innervation to lateral associated movements, are able to respond to a convergence impulse, so the external recti might retain the power of active contraction in response to an unimpaired divergence impulse.

This loophole of escape for those who cling to a divergence centre is surely closed, however, by the not unfrequent coexistence of complete paralysis of one externus with the power of bringing the affected eye back to the middle line on the cessation of convergence. In such cases the same result follows the relaxation of an active contraction of the internus in association with the externus of the other eye. In neither case would the mere removal of the internus innervation effect the return of the eye to the middle line, while the absence of any power in the externus makes it certain that it responds to no active innervation, which is antagonistic to that which is supplied to the internus. The return of the eye to the middle line must therefore have a purely physical cause, or must depend upon *the resumption by the muscle of that state or that length which corresponds to its tonic innervation when opposed by the tonically innervated internus.* That the latter and not the former is the true cause seems to me evident from the phenomenon of so-called secondary contracture of the antagonist in cases of ocular paralysis. This phenomenon may either slowly develop or be apparent from the outset. It is no doubt due to *the loss, gradual or sudden, of this tonic innervation in the paralysed or paretic muscle.* It is not

due to any change which takes place in the innervation or in the physical state of the antagonist. With the more or less complete disappearance of tonic innervation in an external rectus, for instance, a greater or less preponderance of action takes place in the internus whose tonic innervation is unimpaired. Thus is developed the position of convergence which in many cases characterises the position of rest in paralysis or paresis of the sixth nerve. The term secondary contracture is therefore, I believe, and have always maintained, a misnomer. It is one of the many terms expressive of conceptions borrowed from muscular changes in other parts of the body. It is very generally, I think, lost sight of that the eye muscles differ in various ways, both anatomically and physiologically from the skeletal muscles.

In conclusion, I may say that I have brought forward this case, a typical example of what, in my experience, is a rather rare affection, firstly to prove that the nature of the diplopia is an evidence of the pathological convergence being diminished on lateral fixation. Again I wished to offer an explanation of this symptom. As I have explained, it can only be caused by the influence of habit. Finally, and more particularly, I wished to make it clear that *the correct diagnosis is spasm of convergence*, and not paresis of divergence, any more than a persistent associated lateral deviation of both eyes is to be looked upon as an expression of paresis of associated movement in the opposite direction. Both conditions, moreover, are, in my experience, always transitory, whereas both paresis of convergence and of associated lateral movement are often permanent.

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