INVESTIGATIONS IN THE RELATION BETWEEN CONVERGENCE AND ACCOMMODATION OF THE EYES.

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I. *Introductory Sketch.*

WHY, if we see separately with *each* eye, do we not see *double* when both are used? This problem has taxed the ingenuity of many busy minds in past ages, and its history is by no means one of uniform progress.

Euclid, two or three centuries B.C., had advanced so far beyond some at a far later date as to recognise that both eyes were employed in unison, and that their dissimilar pictures were in some way united. Galen surmised that the union of the optic nerves at the commissure supplied a clue. Both he and Herophilus assumed that the two nerves were there united by mysterious pores; doubtless to permit the free passage and intercourse of the little spirits of both sides, whose remarkable unanimity in fitting the pictures together was evidenced by single vision. Later on Gassendus, Tacquet, and Joan Baptista Porta, the inventor of the camera obscura, escaped the difficulty altogether by assuming that one eye only at a time was engaged in vision.

In 1613, Francis Aguillon (Aguilonius), a learned Jesuit, called in the aid of what he termed a "common sense," which "imparts its aid equally to each eye, exerting its own power equally in the same manner as the eyes are converged by means of their optical axes." This was an advance, for the two pictures, we may truly say, are mentally united by a "common sense,"[2] of the real nature of which we probably know little more than Aguilonius, though we may notice more of its effects.

Dr Briggs appears to have been the first to have suggested "corresponding" or "identical" points in the two retinæ, that is, that each point on the inner side of one retina has a corresponding point on the outer side of the other, so that when images are thrown by an object upon these identical points, they are mentally united. This was a great advance, though the theory of "identical points in *the field of vision*" is now considered more correct. But he explained it in a

[1] The original of this memoir was the successful essay submitted in competition for the Syme Surgical Fellowship in April 1884. Before publication it has been revised and enlarged.

[2] It is now located in a theoretical "fusion centre."

curious way, by ascribing to *each fibre* of the optic nerve a different degree of tension, like the strings of a violin or piano, each vibrating in unison with its own retinal area,—" a tension," argued Porterfield, "impossible in the soft and pulpy structure of the nerve fibres."

From the fact that "in animals which look the same way with both eyes, the optic nerves meet before they enter the brain, while this union does not occur in those which do not, such as fishes and the chameleon," Sir Isaac Newton suggested an arrangement of the optic fibres at the commissure, which exactly tallies with that now generally received—" the fibres on the right side of both (optick) nerves uniting there at the commissure, and, after union, going thence into the brain in the nerve which is on the right side of the head, and the fibres on the left side of both nerves uniting in the same place, and, after union, going into the brain in the nerve which is on the left side of the head." I quote from the 13th Query at the end of his "Treatise on Opticks" (1718), the more remarkable because it was the belief of anatomists, like Vesalius, that no decussation occurred at the commissure, and that it consisted of fibrous tissue.

Dr William Porterfield of Edinburgh is believed to have first enunciated the correct, though still very partial theory of binocular vision. In his "Treatise on the Eye" (1759)[1] he showed that when the eyes are accommodated for any object their two visual axes are also exactly converged upon the same point, and " since each eye possesses the power, either intuitively or by acquisition, of localising points in space, the object *must* appear single, it being impossible for us to conceive two objects existing in the same place at the same time.

Single binocular vision therefore requires *a perfect concert* between the efforts of accommodation and convergence. The former secures *distinct* vision; the latter *single* vision.

Accommodation affects the *nature* of the images thrown on the retinæ; convergence affects their *position* on the retinæ, so that they still fall on the same portions whether the object looked at is near or distant. If distant, both accommodation and convergence are *nil.* With every approach or recession of the object, they increase or decrease simultaneously. The two efforts are not only associated in their daily exercise, but the nervous centres which govern them are linked in the brain by strong nervous ties, so that the slightest action of one affects the other. This is shown by Donders' experiments, for, though they demonstrate that the desire for single vision has power to *overcome* the nervous ties within limits, when lenses or prisms are used, yet they show also that the slightest alteration in

[1] To which I am indebted for most of what precedes.

convergence shifts both limits of the possible play of accommodation in the same direction.

Further evidence was given by Dr Loring, who, while looking at an object through concave lenses, *reduced* the desire for fusion by placing coloured glass before one eye, and thus produced diplopia. The distance between the two images varied with the strength of the lenses worn, showing that "for every degree of tension of the ciliary muscle there is a corresponding degree of tension of the interni."

Convergence, like accommodation, is brought about by a *single* effort. Hering's theory may well be mentioned here, since it receives striking and repeated confirmation in the following pages. It is that "each eye is supplied by two innervations—one directed to the turning of *both* eyes to the right or left, the other to turning both eyes inward or outward." "Both eyes are used in the service of the sense of sight as a single organ consisting of two separate limbs."

The movements of both eyes to the right or left may for convenience be called "ranging" movements. They depend on two distinct mechanisms, which have no known connection with each other. Of these, one supplies the external rectus of the right eye and the internal rectus of the left, and turns both eyes to the right; the other supplies the remaining lateral recti, and turns both eyes to the left. When both ranging centres evolve an equal quantity of nervous energy the result is simply increased tension of all four lateral recti, since each internus antagonises its fellow externus. If one centre predominates, both eyes are deviated to the right or left as the case may be.[1] Stimulation of Ferrier's area 12 in the frontal lobe causes among other movements turning of both eyes to the opposite side. It is clear, therefore, that "*convergence*" or intersection of the visual axes is not provided for by this innervation. It is brought about by a separate and superadded effort, and is provided for by a mechanism which affects both eyes equally.

[1] In the nates Adamuk finds a common centre for both eyes, stimulation of the right side producing movements of both eyes to the left, of the left side movements to the right, while stimulation in the middle line behind causes a downward movement of both eyes with convergence of the axis, and in the front an upward movement with return to parallelism, both accompanied by the naturally associated movements of the pupil.—*Michael Foster.*

When an object is viewed in the mesial plane the effort of convergence causes the two visual axes to intersect at the point of fixation, and no effort is needed on the part of either ranging centre. But if the point of fixation is carried ever so little to the right or left of the mesial plane, convergence must be supplemented by an effort of one of the ranging centres to carry the point of intersection into the required plane.

Is the central connection between the efforts of convergence and accommodation complete? Though the nervous association can be partly *overcome* when necessary by prisms or lenses, it does not follow that it should be naturally incomplete, and it has generally been supposed that a normal eye when excluded from vision would remain *in statu quo*. Consistently with this, since the demand for accommodation is relatively greater in a hypermetrope and less in a myope than in normal eyes, it has been supposed that under the same conditions the eye of every myope would deviate outwards, and that of every hypermetrope inwards. We shall find this is far from being the case.

II. *The Blind-spot Method of employing the "Visual Camera."*

The object of this method is to ascertain the behaviour of an eye placed subjectively in the dark when the other eye is employed in vision. The blind spot, or "punctum cæcum," is a nearly circular gap in the field of vision of each eye discovered by Mariotte, and shown by Donders to be due to the fact that the entire surface of the "optic disc" (the extremity of the optic nerve at its entrance into the eye) is wholly insensible to light. When one eye is closed, therefore, there is an area in the outer part of the field of vision of the other entirely devoid of visual impressions, and large enough, according to Helmholtz, for eleven full moons to stand in a row in it (*Handbuch der Physiologik Optik*, 1867). The method of its employment for our purpose is illustrated in fig. 1, which represents a dark box or camera of a flattened pyramidal shape, measuring about a foot from side to side and nine inches from before backwards.[1] The narrow end contains two visual apertures, pierced through slides (*a*, *a*), which permit their mutual distance to be regulated as the eyes of different observers require.

[1] To be obtained from Messrs Pickard & Curry, Gt. Portland St., London.

The curved border of the box is built up of two arcs (*d*, *d*) united by a straight line nearly 2½ inches long, and therefore equal to the average distance between the centres of the two eyes, while each arc is part of a circle drawn from the centre of motion [1] of the eye of the same side. This end of the box is provided with three luminous points, one fixed (*e*) and two

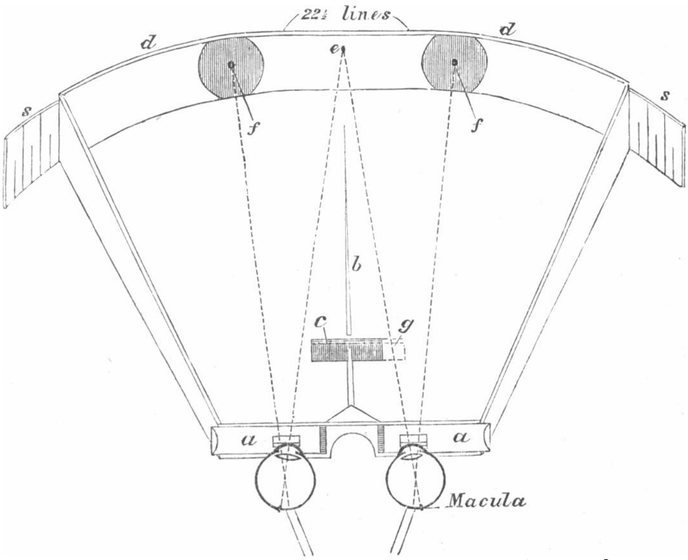

FIG. 1.—View of the visual camera with the roof removed.
(*Erratum.*—The dotted lines should cross *in* the crystalline lens instead of behind it; 22½ lines should be 28½ lines.)

movable (*f*, *f*). They are tiny apertures, which become luminous when the box is held up to the light. The central one (*e*) is stationary, and since it is used as the point of fixation, should be provided with a piece of ground glass, a letter, or cross wires, to fix attention.[2] The lateral points (*f*, *f*) are preferably coloured,

[1] This point is about 13 mm. (Donders) behind the anterior surface of the cornea. Nearly half an inch is allowed for the distance of the corneæ from the visual apertures, so that since the box is 9·2 inches from before backwards, points on its *further* border are 10 inches from the *dioptric* centres, and therefore when looked at require 4 dioptres of accommodation to be in exercise. A dioptre is the chosen unit of refractive power; it is that possessed by a spherical lens of the focal length of a metre (nearly 40 inches). *Four* such lenses would represent the *increase* in the refractive power of the crystalline lens required to focus on the retina distinct images of points 10 inches distant.

[2] In default of these it suffices to moisten a piece of printed paper and apply it to the outside of the aperture.

and are pierced through brass slides (*s*, *s*) which travel in grooves, so that each aperture can be moved at pleasure along its own half of the curved end independently of the other and of the central one, and without the admission of any additional light. This is brought about by a system of long slits so cut in the brasswork that the two slides and the side of the box against which they are apposed mutually overlap each other's slits, and yet permit the points of light to be seen through. A graduated scale of degrees (made by taking as a radius the centre of the eye of the same side) is attached to the outer surface of the arcs, and indicates the angular interval between each of the movable points and the central one.

The camera is nearly divided into two lateral compartments by a median vertical partition (*b*), which runs forward to within an inch or two of the central luminous point. It is interrupted by a small cross-piece of wood called the "stop" or "obstructive" (*c*), which is let in through a slit in the roof, and can be made to travel shortly from side to side so as to intercept at pleasure the view of the central point (*e*) by either the right or left eye. This is shown to the right in dotted outline (*g*), but the central point (*e*) is perfectly visible by *both* eyes, *so long as* the "stop" is in the middle of its slit, as represented by the *shaded* portion of the figure (*c*).

Since the optic nerve enters the eye to the *inner* side of the visual axis, and since all projections are reversed in position, there is an area on each side of the curved end of the box (represented by a shaded circle) which corresponds to the projection of the blind spot of the eye of the same side, and which may be called the "blind area." Each is about an inch in diameter at this distance from the eye. It may be observed that vision of the *movable* points is *always* monocular, since the medium partition (*b*) cuts off the view of each from the opposite eye; whereas vision of the *central* point is either monocular or binocular at pleasure according to the position of the "stop," the motion of which is too short to interfere with the view of either of the *movable* apertures, though wide enough to interfere (when desired) with the view of the central one by either eye.

Exp. 1.—As a preliminary, push the *left* brass slide inwards until the point it bears is overlapped by the brass work and thus disposed of. It is not needed in the observation. Put the stop in the *middle* of its slit, and leave the *right* movable point within the usual limits of the right blind area. Now let the subject of the experiment hold the camera up to the light and look steadily with both eyes at the central fixation point. The right luminous point, being in the blind area, is then out of sight *so long as* the stop is in the middle. Now push the stop to the right, and it will be found that though the observer does not know what has happened, and still thinks he sees as before with both eyes, yet in most cases, after the lapse of a moment or two, the hitherto hidden point springs into view, showing that the eye has deviated from its former position, and has allowed the image of the luminous point to fall on a sensitive portion of the retina, as in fig. 2. The only effect of which the observer is conscious when the stop is pushed to the right is that the fixation aperture appears less bright,[1] yet by so doing the right eye is excluded from vision entirely, and placed subjectively in the dark, since of the two apertures the fixation one is cut off by the stop and the other throws its image on the blind spot where it produces no impression. He is aware neither of the exclusion of the eye nor of its deviation.

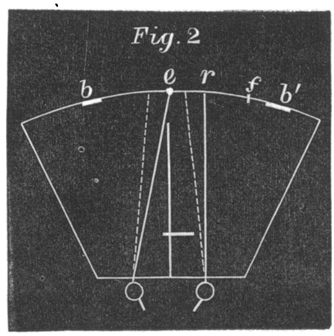

FIG. 2.—The vision of the central aperture (*e*) being cut off by the stop from the right eye, its axis has deviated from *e* to *r*, and its blind area (*b′*) has moved to exactly the same extent, so that it no longer conceals the point of light (*f*). The *left* blind area (*b*) does not move, showing that only one eye deviates.

If now, *after* the eye has deviated, the right brass slide is drawn outwards, the movable point it bears again becomes lost to view in the blind area, showing that the deviation was *outwards.* Its exact extent may be measured in degrees by reading off from the graduated scale, the position of the inner border of the blind area *before* and *after* the eye has deviated, that is, *first* with the stop in the middle and *then* to the right. The difference between the two records gives the angular deviation of the visual axis. In my own eyes it is about 5° as a rule, though it varies from 3° to 7° or even 8°, according to the time of day, the temporary comparative anæmia or congestion of the brain, the previous occupation of the eyes, and doubtless many other conditions. It appears to be greater in the morning than in the evening, and less after much reading, or with congestion of the eyes from close work or hot rooms. That there should be any *outward* deviation at all in my case was an unexpected result, owing to the presence of at least 2 D of hypermetropia, for it has hitherto been supposed that when excluded from vision a hyperme-

[1] The central aperture sometimes also appears to move slowly to the right, but this is not generally noticed unless attention is called to the fact.

tropic one deviated *inwards.*[1] I believe, however, that a great many eyes with minor degrees of hypermetropia would be found to deviate outwards, and that if this were duly estimated some of those difficult cases might be more readily relieved which are so sensitive to any disturbance of the requisite relation between convergence and accommodation.

The psychical factor furnishes an occasional difficulty in the observations when there is a constant *expectation* of seeing the hidden point appear. It may be guarded against by registering the position of the outer as well as the inner border of the blind area in both records, which thus mutually correct each other, since the same mental effort which might prematurely bring the hidden point into view when one border is being tested would do the very reverse when the other is under trial. Moreover, if the recorded breadth of the blind area be found equal in the two observations, before the deviation and after it, the coincidence is reassuring as to the exactness of the records. Variations in the shape and size of the "disc" in no wise affect the experiments, since the same definite point in each border is taken as the index of deviation. The shape of the curved end of the box is such that each movable aperture in any part of its range still throws a tiny and *distinct* image upon the retina instead of a diffused one; for, as Donders has said, "in the emmetropic eye the whole curvature of the retina lies in the focal surface of the dioptric system." The image is about $\frac{1}{17}$th the size of the aperture, so that the latter being half a line wide its image is about $\frac{1}{400}$th of an inch in width.

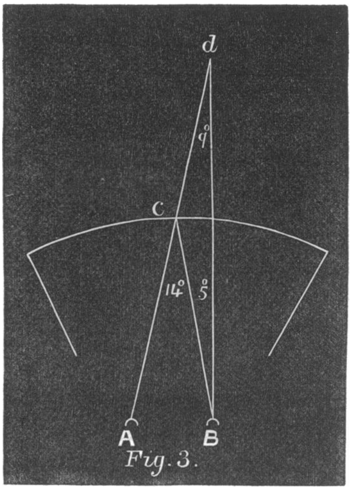

Fig. 3.—*AcB* was the optic angle before the right eye deviated. *AdB* is the optic angle *after* deviation; it is less than before, by the angle of deviation *cBd*.

[1] I am indebted to Mr Brudenell Carter's "Defects of Vision" for the fact that Hansen has recorded a few instances of "central defect," though Mr Carter had not identified them (1877, p. 141), and says: "In every case of myopia the tendency of the visual axes would be towards divergence, and in every case of hypermetropia the tendency would be towards convergence as soon as the control exercised by the demand for fusion was withdrawn" (p. 138). To Hansen then belongs the first notification of the fact that in "a few persons" an excluded eye diverges with the ordinary tests at reading distance. I think, however, the camera will show that instead of being a rare exception, this is the *normal* condition, though not the invariable one. Doubtless Hansen's cases were, in one sense, *really exceptions* to the normal, in that the degree of deviation was large enough to be detected by the ordinary methods.

It may be stated as a simple geometrical necessity[1] that the angular deviation of either eye alters the "optic angle" (or "angle of convergence" contained between the *two* visual axes), by the *same number of degrees* (fig. 3). When both eyes fix the central aperture the optic angle is 14°. A deviation therefore of the excluded eye to the extent of 5°, reduces the optic angle from 14° to 9°. From this it is easy to calculate that, while *accommodation* still remains in both eyes for a distance of 10 inches, the visual axes intersect at a distance more than half as much again (15·7 in.), and which, if it in turn became the point of fixation, would need $1\frac{1}{2}$ dioptres less of accommodation to be in exercise ($2\frac{1}{2}$ D instead of 4 D).[2] I have tried a sufficient number of cases to assure myself that *outward* deviation of the excluded eye is the *rule* where refraction is apparently normal or only slightly hypermetropic, though here and there an exception is found. Of ten recorded cases the average deviation was $4\frac{1}{2}$°, as shown in the following table, which also gives the angular interval between each border of the blind area and the visual axis *before deviation*—the difference between them gives angular dimensions of the blind spot.

TABLE I.

No.	Inner border of blind area.	Outer border of blind area.	Breadth of blind area.	DEVIATION.
1.	$12\frac{1}{2}$°	$18\frac{1}{4}$°	$5\frac{3}{4}$°	0°
2.	$12\frac{1}{2}$°	$18\frac{1}{2}$°	6°	1° or $\frac{1}{2}$°
3.	$12\frac{1}{2}$°	19°	$6\frac{1}{2}$°	$2\frac{1}{2}$°
4.	11°	17°	6°	4°
5.	$12\frac{1}{2}$°	$18\frac{1}{2}$°	6°	$4\frac{1}{2}$°
6.	$12\frac{1}{2}$°	$18\frac{1}{2}$°	6°	5°
7.	13°	19°	6°	$6\frac{1}{2}$°
8.	13°	$18\frac{1}{2}$°	$5\frac{1}{2}$°	7°
9.	$11\frac{1}{2}$°	17°	$5\frac{1}{2}$°	7°
10.	$12\frac{1}{2}$°	$18\frac{1}{2}$°	6°	$7\frac{1}{2}$°
			Average, .	$4\frac{1}{2}$°

If this table is at all representative (and I expect it is fairly so), it shows that, while deviation occurs in nearly all, its amount varies greatly in different individuals; in No. 10 only $6\frac{1}{2}$° of convergence is left, as attached centrally to the accommodative effort—less than one half. A more extensive set of observations is much to be desired to arrive at a more reliable average, and to seek, if possible, to note some of the *causes* of these variations, but for taking records the "direct method," to be described presently, is far to be preferred.

[1] Euc., bk. i. prop. 32.

[2] See the footnote on page 479.

It has been considered by Donders, a fact at present unaccountable, that only a *small* proportion of hypermetropes should develop strabismus, and that the *same* refractive anomaly should lead to squint in some cases and not in others. No doubt an explanation is afforded by these great variations which exist in the amount of convergence *naturally* attached to the effort of accommodation. So long as every hypermetropic eye was supposed to deviate inwards when excluded there was no reason why *all* hypermetropes should not squint. The *minor* degrees of deviation which the camera detects come thus to have importance. The advantages of angular measurements over linear ones are obvious. The latter would vary with cameræ of different sizes, and would not permit of direct comparison, whereas the former are invariable.

It is evident from the results obtained that the central connection between the efforts of convergence and accommodation is still considerable, though not complete. If there were *no* central connection the excluded eye would deviate outwards nearly 14° instead of only 4½°. If the connection were complete it would not deviate at all. In ordinary vision there is perfect concert between the two efforts, since the two visual axes meet exactly at whatever point is accommodated for. To bring this about a "supplementary" effort must be in exercise whenever central connection is insufficient. This effort is connected with the instinctive desire for single vision, of which the seat is yet unknown, so that we may say the relatively *complete* convergence of ordinary vision is maintained partly by central connection with accommodation and partly by this additional effort, which is *first* roused into activity by the sensible presence of double images, and then *maintained* in exercise by the fact, of which the nervous centre is every moment kept sensible, that were the effort abated the mental image would immediately resolve itself visually into two. To keep it from doing so the joint sensations from the retinæ must all the while be bearing between them the message of continually impending (yet as quickly averted) double vision, by threats of double images so slight and frequent that they produce the required effect without our being conscious of their existence. It is difficult to conceive the exquisite mechanism at work so assiduously when we remember that, if double images are produced artificially or by disease, it is impossible for the mind to tell to which eye each image belongs—whether, therefore, the visual axes are crossed or not, and whether convergence needs to be increased or relaxed to bring the images together.

By Hering's theory, convergence is a single effort, exerted in equal amount in each eye.

It is also clear that impressions from both eyes are necessary to maintain the supplementary factor in convergence connected with the abhorrence of double images. When, therefore, the obstructive in the experiment is placed before the right eye, and vision is confined to the left only, this common effort ceases, and *both* internal recti receive correspondingly diminished impulses from the converging centre. Were this all that happened, *e.g.*, in my own case, each eye would deviate outwards $2\frac{1}{2}°$ as represented by the dotted lines in fig. 2. As a matter of fact, however, the active one remains stationary, fixing the central aperture, while the uncontrolled one moves outwards 5°.

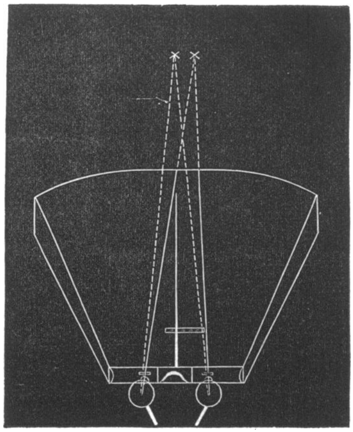

FIG. 3 A.—Convergence of the visual axis as if for the left hand cross is effected by the converging innervation; but they are jointly deflected to the right hand cross by the ranging innervation; in accordance with Hering's theory.

This can be proved by commencing the experiment with *both* lateral apertures in their respective blind areas, when it will be found that if the stop is pushed to the right, although the right lateral aperture comes into view, the left one remains hidden the whole time; if the stop be pushed to the left the left aperture appears while the right one continues hidden, showing clearly that in each case it is the seeing eye which continues stationary, and the excluded one which deviates. Another innervation, therefore, distinct from that of convergence, must come into play to keep both the eyes from deviating equally. This is found in that centre whose ordinary function it is to turn both eyes to the right, and which, therefore, presides over the internal rectus of the left eye, and the external of the right eye. *It* compensates by a slight effort for those impulses which the left internal rectus has lost from the converging centre; but since it governs both eyes equally, while it maintains the convergence of the left eye, which would otherwise fall back $2\frac{1}{2}°$, it moves the right eye through an additional $2\frac{1}{2}°$ (*see* fig. 3 A).

The effort put forth by this fresh innervation is determined entirely by the requirements of the seeing eye; it only affects the deviating eye because it cannot help influencing one as much as the other. Its intervention is *proved* by the next two experiments. The result is that exactly half the deviation of the right eye is due to relaxation of the internal rectus, and the other half is due to contraction of the external rectus; but since in the left eye the *diminishing* converging effort and the *increasing* ranging effort have each to do with the internal rectus, it remains stationary.

Exp. 2.—With the stop in the *middle*, fix the central aperture with both eyes, and try to place the right forefinger exactly upon the central aperture from outside. The attempt will succeed in proportion to the perfectness of the observer's muscular sense. Now push the stop to the *right*, and repeat the attempt. The finger will be found to have missed its mark, and to be *actually* on the right side of it; and similarly to the left side of it if the stop is pushed to the left. The miscalculation will be *slight* if the attempt is made *directly* after the exclusion of the eye, and greater with every increase in the interval which elapses till the maximum miscalculation is reached, which in my case is about a distance which corresponds to $2\frac{1}{2}°$ on the graduated scale. The right eye, we have seen, has meanwhile moved 5°. It may therefore be accredited as a rule that the angle of miscalculation is *half* that of the deviation of the excluded eye; it is slight at first, because the deviation is slight, and they increase together in the proportion of 1 to 2.

It has long been known that when one eye is closed, and a finger is pushed forward from under a book, it misses its mark to the side of the closed eye; but I believe this phenomenon will be absent in those with whom deviation of an excluded eye does not occur at the distance of the test; and that the extent of miscalculation will be found to depend entirely on the *amount* of the deviation, and to be half as great.

Exp. 3.—If the central aperture is very closely watched its apparent position may be observed to move slowly to the right as soon as the stop is pushed to the right. Now, it is remarkable that the point of view should *seem* to be moving when not only is the point really stationary but also the image it throws on the retina, and the retina itself. Since only one eye is in this case engaged in vision, and that (as may be shown by the immobility of its blind area) keeps quite still the whole time, there cannot be the slightest change in the comparative *tension* of its recti, to account for the apparent movement of the image. Moreover, though the excluded eye deviates, we shall see later that the oculomotor muscular sense is purely *central* and not peripheral, since the *same* degree of tension in a muscle is mentally estimated or mentally ignored, according to the central source of the impulses which cause the tension. The stillness of the seeing eye therefore proves that the illusion is due to some alteration in central nerve effort of which the mind takes (what is now) unnecessary cognizance, and thus forms a false estimate.

The new effort is also shown by the nature of the apparent movement to be the one which the mind has been accustomed to associate with *lateral displacement* of the point of fixation, and with the *joint* movement of both eyes to the right, which such displacement makes necessary in the ordinary vision of nature. The illusion cannot be due to the diminution of converging effort, because *that*, as we shall see, is mentally associated only with the idea of *distance*, not at all with the angular departure of the object from the median plane, or its position in the field of vision. The *slowness* of the apparent movement is a striking feature; it shows how gradually the ranging effort is put forth, consistently with the gradual diminution of the converging effort for which it exactly compensates.

It is a fact which affords some food for thought, that although the *stimulus* which causes the "supplementary" converging effort ceases *suddenly* when the stop is pushed to the right, yet the *effort* itself continues for some time decreasing only *gradually*. This is in striking contrast to the speed with which full convergence is again effected when the stimulus is restored. The gradual relaxation of the converging effort when the stimulus is withdrawn, causes *both* internal recti to receive growingly feebler impulses from the converging centre, so that each eye has a constant and momentary tendency to deviate outwards, which is only prevented in the left one by the wonderful vigilance of the nervous mechanism which every instant appreciates this tendency, and as quickly compensates for it, *not* by again stimulating the flagging convergence, but by causing a strictly proportionate and gradual increase of that effort whose output causes in the mind the impression that the point of view (really stationary) is moving to the right. It need hardly be said that all this naturally accords with and establishes Hering's theories mentioned on p. 477.

The apparent movement of the central aperture is *through half the angle* and *at half the rate* of the real movement of the deviating eye. A little reflection on the preceding experiment will show the truth of this, as nearly as it can be determined, and also that when an object is fixed not far from the middle line its position is mentally referred to the vertical plane which bisects the angle of convergence, and which, as we shall see, runs through a point midway between and slightly behind the centres of the two eyes. (See the line *yp* in fig. 3.)

After a few attempts to touch the point thus miscalculated, the mind allows for the error, and the attempts begin to succeed. It has already been suggested that thousands of such attempts in childhood contribute to the wonderful correlation between the muscular sense of the eye and the hand. How perfectly they may by practice be made to co-operate is seen in a good cricketer or marksman.

The *senses* are there to begin with, but the mental apprehension of their import, both singly and jointly, seems to be largely left to be perfected by *education*. Indeed, it is known how any sense itself may be quickened by receiving a larger share of psychical attention, or dulled by its prolonged abstraction.

The human body is thus made capable of adapting itself within limits to adventitious circumstances; it is not made, like an ordinary loom, capable only when once set of turning out material of one texture,—but it is like a loom, if one can be conceived, made with such wonderful skill and forethought that it can automatically adapt itself to the requirement of any new material and other altered circumstances.

I find, on trying to touch the central aperture with my left hand, that when the stop is to the right, instead of missing its mark to the right side of the central aperture aimed at, it misses it to the left side, and when the stop is in the middle it misses it still more to the left side, though its miscalculation is not very precise. Its muscular sense is therefore less perfect.

Exp. 4. On first opening the eyes in the morning the divergence is greater than during the day; it falls just after the mid-day meal and perhaps after the others.

Exp. 5.—When vision is directed *through* either the central aperture or the left lateral one at an object placed at different distances, accommodation is, of course, diminished in proportion. It will be found that the excluded eye moves *outwards* with each *removal*, and *inwards* with each *approach* of the point of fixation. This shows how *delicate* is the connection between the two efforts, since the slightest difference in accommodation causes an alteration in the degree of convergence.

Exp. 6.—If convex glasses of increasing strength be placed in turn before the active eye, the blind area of the obstructed eye moves outwards with each increase in the refractive power of the lens employed. With *concave* glasses, on the other hand, it moves inwards with every increase. This experiment, of course, differs only from the last in the method employed; which, indeed, is far less satisfactory, owing to the fallacy introduced by prismatic action of the lenses, if their optical centres are not placed exactly in the line of vision—a precaution of great difficulty.

Exp. 7.—When the box is sloped downwards from the eyes, I have records which show that the deviation of the obstructed eye is reduced by 2° or 3°. I am not quite satisfied, however, with the observations—the bridge of the nose almost obliges the box to be held at a greater distance. The way to get over the difficulty would be to use prisms with their bases upwards, which would permit the box to be held horizontally, and yet record the effect of a downward direction of the visual axes. The ordinary circular prisms used in practice are not available for this purpose, owing to the difficulty of placing the centre of the base exactly in the vertical line which bisects the prism. A slight shift to either side not only *reduces* the *vertical* deflection of the line of vision, but introduces a still greater *lateral deflection*, which vitiates the result. Small prisms fixed *in* the visual apertures would be most satisfactory.

Exp. 8.—If the central aperture be fixed by the left eye, with the obstructive to the right, it is possible to place the right lateral aperture so precisely upon the inner border of the right blind area that the point of light alternately appears and disappears, showing an evident tendency in the nerve centre to rhythmic, or at least irregular action. This irregularity furnishes a striking contrast to the fixedness of gaze and precision of movement in ordinary binocular vision. It devolves upon the supplementary effort in single binocular vision to fill in these irregularities in the fluctuating basis, besides meeting the new and changeful requirements constantly introduced in glancing from point to point. It is interesting to notice that this *fluctuating* effect in the converging centre is connected with the evolution of a *steady* stream of nervous energy from the accommodating centres. It may perhaps bear some comparison with the rhythmic automatism which manifests itself in the vasomotor centre under the uniform stimulation of venous blood, as evidenced by Traube's curves.

Exp. 9.—With both eyes fixing the central aperture, and with the obstructive in the middle, place the right lateral aperture in the outer part of the blind area at a definite number of degrees from its inner border. Push the obstructive to the right, and note how long a time elapses before the hidden point comes into view, by listening to a clock pendulum beating half-seconds. As might be expected from Exp. 8, the interval is a variable one. Thus, at one sitting, my right eye was engaged from $12\frac{1}{2}$ to 22 seconds in rotating outwards $3\frac{1}{2}°$.

Exp. 10.—After wearing convex spectacles for some hours, I find that for a time the relative divergence is diminished (by the training the converging centre has undergone in the increased relative demand made upon its energies). How long this effect lasts I have not been able to observe.

Exp. 11.—*Measurement of the Blind Spot.*—I have found the angular dimensions of the blind spot in its horizontal meridian, as far as the box measures it, very uniform. In nearly all cases it was approximately 6°. So far as the observations are worth, they go therefore to confirm Landolt's estimate of 6°, rather than Helmholtz's of nearly 7° (6° 56′).[1] The method they both employed was that of moving a pencil on a piece of paper till the point became lost to view. With one who has thoroughly practised indirect vision this suffices, but for others it is very uncertain. Thus Helmholtz says: "I have even seen men of education and information—doctors, *e.g.*—not able

[1] It must be remembered, however, that any error of the box from not measuring the exact horizontal meridian tends to give too *small a result.*

to prove the disappearance of small objects on the blind spot."[1] Hanover and Thomson, in 22 eyes (quoted by Helmholtz), found the breadth to vary from 3° 39′ to 9° 47′. I believe cases of less than 5° or more than 7° will be found exceedingly rare. In taking measurements, the stop should be either in the middle or to the opposite side of the eye under examination. I believe it is better to start with the point hidden, and let the observer exclaim at its first appearance at either border, rather than to note its disappearance, though the two may check each other.

A point of light is peculiarly fitted for the purpose, owing to the comparatively great susceptibility of the peripheral parts of the retina to light. Brewster[2] stated that astronomers, when they cannot see a minute star by looking *directly* at it, may often bring it into view by looking somewhat *away* from it. Landolt,[3] however, finds "the perception of light remains almost exactly the same throughout the whole extent of the retina." He instances that in his right eye the perception of light at a part 30° from the centre remains the same, while the visual acuteness is reduced to $\frac{1}{48}$; but certainly, in my own eyes, the point of light appears to be more easily discerned on its emergence from the *inner* (macular) border of the blind area than from the outer border—it may not be so with others. *Clinically*, the measurement of the blind spot may be useful, both to determine the increase of the posterior staphyloma of progressive myopia and to trace the progress and decline of such affections as optic neuritis, in which the adjacent retina loses its perception awhile by infiltration.

A disadvantage is, that in the original instrument the two lateral apertures are not upon the same level, and therefore one of them (the highest) measures the blind spot *above* its horizontal diameter, and gives a uniformly smaller and fallacious record. This may be rectified by using, instead of slides, two flexible ribbons arranged circularly, so as to have the lateral apertures on the same level.

It is well to have the point coloured *blue*, since the peripheral parts of the retina perceive this colour most readily. If we assume that an angle of 4°, with its apex at the optical centre of a normal eye, subtends 1 mm. of the retina, then 6° would subtend $1\frac{1}{2}$ mm.; showing the close coincidence between the anatomical and physiological dimensions of the disc. The angular distance between the visual axis and the border of the blind area I have not found so uniform as the breadth of the blind spot. Landolt and Dobrowolsky found the interval greater in hypermetropes and smaller in myopes.[4] It would be well to confirm this by the camera.

[1] *Optique Physiologique*, p. 735.

[2] Brewster on *Stereoscope*, 1856, p. 44.

[3] Landolt, on *Examination of the Eyes* (translated by Dr Burnett, 1879, Philadelphia), p. 214.

[4] *Examination of the Eyes*, Landolt, 1879, p. 216.

III. *The Direct Method.*

This method is far more useful clinically, and not less interesting physiologically. The eye is not placed in the dark, nor is the blind spot made use of. It depends upon the fact, that when each eye receives a single image upon its median vertical meridian, from whatever points they are thrown, the two are mentally referred to the same vertical line.

EXP. 12.—Place the *left* aperture out of sight and the obstructive to the right; the observer then sees the central and the *right* lateral apertures. As he looks, they appear to approach. The right slide is then pushed inwards till they seem to lie in the same vertical line. The process is now complete; it will be found that a *real* interval separates the *apparently* superimposed apertures. This interval expresses in degrees the relative divergence of the eyes, for one visual axis passes through one aperture, while the second lies either above or below the other. I have found this method quite easy in a child of six.[1] In comparing its results with those obtained by the blind spot method, I found that they coincided, showing that the mere additional presence of an image upon the retina does *not* affect the convergence and accommodation, so long as the desire to unite double images is eliminated. In the *blind spot* method there is an image in *one* eye, in the *macular* method in *both.* Its explanation is simple. Since the view of the right point by the left eye is intercepted by the median partition, and that of the central aperture by the right eye is cut off by the obstructive, each eye sees only one point, and that a different one, as shown in fig. 3 B. From the nature of the curve at the base of the camera, accommodation is required from each eye in equal amount (or practically so). If now the brain relationship were *complete*, when attention is directed to one aperture, say the central one, *both* visual axes would converge toward it, while the image of the right point would fall to the *inner* side of the macula of the *right* eye, and would be correctly referred *outwards* to its real position in space. This, in fact, does continue momentarily, when first the points are looked at. As soon, however, as relative divergence commences, and the right eye deviates outwards, the image of the right point approaches

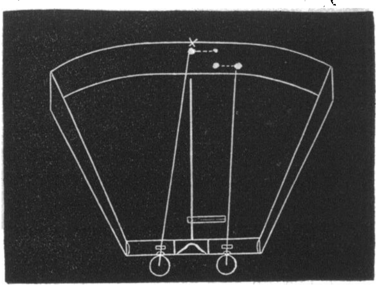

FIG. 3 B.—Illustrates the "direct method." The apertures *appear* superimposed though *really* separated by the deviating angle of the eye.

[1] It is convenient for children to remove altogether the little wooden slides bearing the visual apertures.

the macula, or, more correctly, the macula approaches the image, for it is the eye which moves and not the point. While this is going on, the two stationary apertures *appear* to be getting nearer to each other, for the cerebral centres are unconscious of the divergence, and make no allowance for it. The images do not appear to *meet* completely until each falls upon the median vertical meridian of its eye. It is well to begin the experiment with the apertures at some distance from each other, and after allowing a short time for them to approach naturally as far as they will, to push the right slide inwards, and let the observer say when they come into the same vertical line. In this part of the process the *eye* remains stationary while the *image is moved*, on to its median vertical meridian.

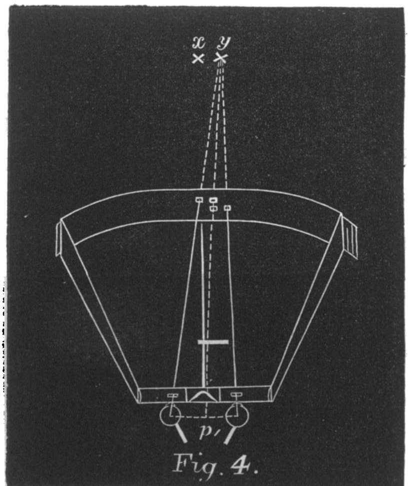

FIG. 4.—The direct method. Each luminous point throws an image on the fovea of the eye on the same side, so that both images are mentally referred to the plane which bisects the angle of convergence.

The dialogue would be something like this:—

Q. What do you see?—*A.* Two bits of light.

Q. How far apart?—*A.* An inch or two.

Q. What happens? (pushing on the right slide slowly).—*A.* The right one is moving to the left.

Q. Say when they are *quite* together, that is, when the right point comes to be *exactly below* the left.—*A.* Now!

This concludes the observation. The *real* interval between the two points, automatically recorded by the graduated scale at the base of the box, has only to be read off to give in degrees the relative divergence of the eyes. This method dispenses with the use of prisms and the fallacies which attend them; it saves the trouble of special measurement, and gives an angular instead of a linear record, which is therefore always ready for comparison. It is equally available by daylight or artificial light.

But the best *practical* evidence of its efficiency is afforded by the ease with which it reveals the *physiological* prevalence of relative divergence in near vision, while the ordinary methods have only hitherto detected the grosser *pathological* exceptions. I may not be acquainted with all of them, and therefore cannot indicate the reasons of their failure, but I think I can suggest what they are in Von Graefe's well-known test, which when carried out as usually directed, does not reveal the slightest relative divergence in my own eyes, though, as we have seen, 5° really exists on exclusion. I have not had access to Von Graefe's own directions. I may quote those in Mr Carter's valuable treatise on *Defects of Vision*, as I followed them:—

"In this more delicate test the object of vision is a small black dot, bisected by a vertical line. A card thus marked is fixed in the median line at a distance of 8 or 10 inches from the eyes, and the patient is directed to look at it steadily. A prism of ten or twelve degrees, with its base either upwards or downwards, is then placed before the eye; and as the power of the superior or inferior rectus to overcome double vision is very limited, this prism necessarily produces a vertical diplopia. The patient will therefore see two dots, one above the other. If the original convergence for the object is accurately maintained, the duplication of the vertical line will only cause it to appear elongated, and the two dots will be seen one above the other on the same line. If, on the contrary, the convergence be not maintained, the patient will see two lines with a dot upon each; and when the diplopia is a consequence of relative divergence of the optic axes, the double images will be crossed, and the extent of the divergence will determine the distance between them."

On carrying out these instructions the dot truly duplicates and the line elongates, but that is all. The line still continues single. The reason of this becomes evident when the further step is taken of covering one eye for a short time; on again uncovering it, two lines appear, separated by a considerable interval, but they quickly run together again. This shows that the desire for fusion, though doubtless *weakened,* is not removed altogether, for the overlapping portions of the two linear images are sufficient to excite it. We shall see that images need not be similar in shape to excite an effort to unite them. Indeed, in ordinary vision the two pictures, as illustrated by the stereoscope, are slightly dissimilar except when the objects viewed are at a practically infinite distance. But I find if the upper part of the line be drawn very wavy, and the lower part straight, so that in the experiment the wavy portion overlaps the straight portion, there appears to be no attempt to unite them, though even then would not be quite sure that there is not a faint effort to keep them nearer to each other than they would otherwise be.

The fallacy may also be demonstrated in another way *without* temporary exclusion of either eye, by simply holding the line at first horizontally (with the prism as before) and then quickly returning it to the vertical position; the two images for a moment or longer are quite separate, and hesitate a little before they run together.

"Why then," it may be asked, "if the test does not eliminate the fusion effort, does it *ever* reveal relative divergence?" It does so because, though it does not, like the camera, *remove* the desire for single vision, yet it lessens it to such an extent that it becomes inadequate to the demands made upon it in certain pathological conditions. The test *weakens* the desire for single vision, not only by the effect on one of the images of the slight light-absorbing (especially when the prism is not perfectly clean and free from moisture) and chromatic properties of the prism, but also by *shortening* the linear extent of the overlapping portions of the two images of the line. It would therefore detect relative divergence in such conditions as (1) those probably very rare cases in which the normal desire for fusion is defective. By lessening the desire still further it might be rendered incapable of rousing a sufficient "supplementary" converging effort. (2) Where the mechanical difficulties which attend convergence are so great that no effort can overcome them unless prompted by a *strong* fusion stimulus, as in some extreme cases of myopia, or where there is weakness of the internal recti or functional disability in their innervation. (3) Where almost the whole of the required convergence devolves on the fusion effort.

In all cases of myopia a larger share falls to the fusion effort than in the normal eye, because there is less demand for the effort of accommodation in looking at any point, and therefore the degree of convergence *due to central association* is correspondingly small. The smaller it is, the more work it leaves for the fusion effort, so that, "cæteris paribus," the greater the refractive anomaly the larger is the required proportion of supplementary or fusion effort.

A great effort needs a great stimulus. The latter is so weakened by the prism that, while still adequate for the requirements of normal refraction, it may be inadequate for those of high myopia, in which, moreover, mechanical difficulties almost always exist as well from the altered shape of the globe.

To make the test of any relative value even in these cases, care must be taken to make the line of always the same length, or if not, to adjust its distance from the eyes in proportion; so that the reduplicated portion of the line may always be of the same length, and thus ensure *uniform* diminution of the desire for fusion, otherwise the test might at one time detect an insufficiency and at another time not. Moreover, the line which joins the apex and base of the prism must be exactly at right angles to the line uniting the centres of the two eyes (intercentral line); otherwise, though the lines continue parallel, their very opposition would only prove that convergence is *not* complete—if it were so, the lines would be *separated* by an interval determined by the strength and degree of rotation of the prism. Even when the *prism* is held correctly, if the *line* looked at is not also held exactly at right angles to the intercentral line another fallacy ensues, for the linear images, though still parallel are oblique, so that coincidence of their overlapping portions, instead of showing convergence to be complete, can only take place when it is incomplete, for were it complete an interval would separate them, varying as before with the degree of rotation of the card.

These difficulties, I would suggest, may be overcome by the use of a *double* prism composed of two prisms, each of 2°, fused together by their bases[1] (see fig. 5). The patient, shutting the left eye, holds this prism before the right one, and looks through it at a card marked with a single dot or *short* line. Two false images appear, one 2° above and the other 2° below the real position of the dot, and both are seen by the right eye. It is easy for the patient to hold the prism so that the two images appear in the same vertical line, and then when the left eye is opened as well to say whether the *real* image of the dot lies to the right or left of this line. Even if the first two are not held vertically, if all three images are in one straight line it shows that convergence is complete. If the central one lies to the right of the line, uniting the other two, there is relative *divergence*; if to the left, there is relative *convergence*.

Simple as this expedient is, and though it yields the same result as the camera, it is inferior to the use of the latter by the

[1] In reality, of course, it is a *single* prism of 176° though double in its use, since three faces are used instead of two. The large face (or base) should be towards the eye, the two smaller faces towards the object.

direct method. The camera ensures uniformity in the distance of the object from the eyes without the trouble of measurement; it needs less intelligence in the patient, and gives an automatic angular record. The double prism, however, would I think be found useful for rough analysis at greater distances. The

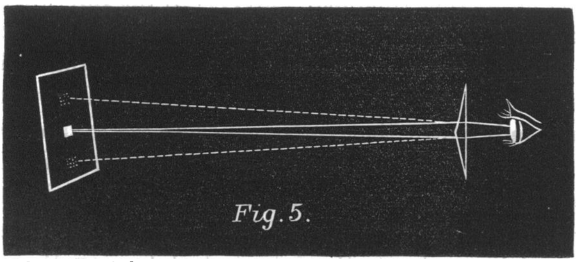

FIG. 5.—Side view of the right eye and the double prism. The false images seen by the right eye are dotted. The central one is seen by the left eye.

radical difference between Von Graefe's test and the camera is that in the latter a *separate object* is used for each eye, while in the former the *same* object is reduplicated by a prism. The camera also not only reduces the desire for single vision, but abolishes it altogether when the lower of the two lateral apertures

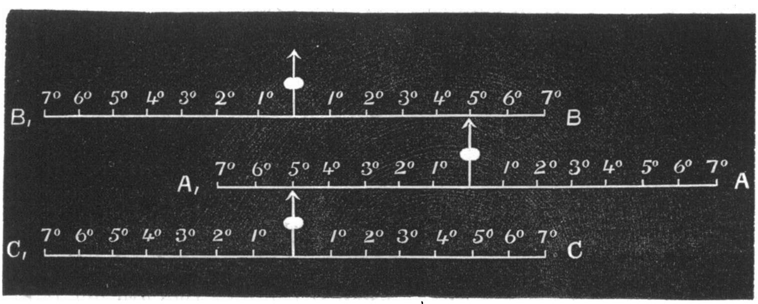

FIG. 6.—To illustrate how relative divergence is measured by the double prism. A is the only device on the card, and is seen by the left eye; B and C are false images of it, and are seen by the right eye. In this instance 5° of deviation are seen recorded. If the two lowest arrows are made continuous by rotating the prism, the middle one points to *twice* the divergence, for as C moved to the right, B moves equally to the left, A of course remaining stationary. The arrows would all but touch the lines above them when the card is held at the appropriate distance of 10 inches.

is used in conjunction with the central one, so that the eye takes a position determined solely by the converging effort which is associated with the accommodation.

If when, in the "direct method," the two images are in the same vertical line, as in fig. 3 B, an effort be made from outside to place the finger on them, it will miss both, for it will be just half-way between the two actual apertures, which, though they *appear* superimposed, are, as we have seen, really separated by an interval of nearly an inch, so that the vertical plane in which the two images appear to lie is that which bisects the angle of convergence, as represented in fig. 4. At present we have only to do with movements of the eye in the horizontal plane, and with the head stationary. The converging apparatus appears to be solely connected with the union of double images and the estimation of distance. With the relative position of points along the horizontal meridian of the field of vision it has nothing to do. This must be determined entirely by—

(1) The part of the retina on which images fall.

(2) The innervation which turns both eyes to the right or left.

As regards the *first* indication, since each image falls on the median vertical meridian of its eye, the effect is the same as though they were both thrown from one vertical line, for which convergence were complete; and, since the relaxation of the converging effort is not taken into account, there is no reason why the images should *not* be referred to the *median plane*, for there is nothing so far to give any preponderance in favour of either side.

As regards the *second* indication, however, as seen in fig. 3, while convergence occurs to the left-hand cross, both axes are directed to the right-hand cross by the ranging innervation of which the mind *does take cognisance.* Now, the inclination of *the plane which bisects the angle of convergence*, to the median plane, exactly represents the angular effect of the ranging energy which is in exercise—hence the images are referred to this line. It is now easy to observe the fluctuations in the stream of nervous energy noticed with the blind spot method on p. 487, for one point continues to make tiny excursions to the right and left of the other, though without any regular rhythm. This makes it useless to take very exact records in minutes and seconds. It is also clear that a more accurate method is scarcely to be desired, since it would only magnify these irregularities. Care must be taken that the difference in level of the two apertures is enough to avoid continued effort to unite them. It is remarkable that, when their vertical separation is only *slightly more* than enough to prevent optical union, *a tendency may be noticed for them to keep near the same vertical line.* Even when one is pushed a little way to the right or left the other is apt to follow it, and this in spite of their dissimilar *shape.* It is even noticeable when the apertures are *coloured* differently; but disappears very rapidly with increasing vertical separation of the two points, and is not in my own eyes detected in the slightest degree when the *lowest* of the two lateral apertures is the one employed, in conjunction with the central one, which is, of course, the highest of the three, being separated from the *lowest* movable point by an angle of 2¼° (from the eyes), and from the *highest* point by slightly less than 1°, in the camera with which I experimented. This latter interval is one which can be overcome at times by the superior or inferior rectus in order to satisfy the desire for fusion, especially when the eye has succeeded a few times, and acquired the facility. After allowing it to do this for a time, the following experiment may be made:—

Exp. 13.—Place the stop to the *left.* Let the left aperture be as before entirely occluded, and the right one be placed 3° or 4° away from the central aperture. Look through the camera thus for about a minute, during which interval the *right* eye sees *both* the images, and the *left neither*, so that the latter is deviating outwards.

Now, push the stop from the left to the right. This proceeding *transfers* the view of the central aperture from the right eye to the left one, which, being deviated 5°, miscalculates its position, and refers it to the *right* of the other point still seen by the right eye. The two points thus separated now by a small interval run together, though to do so it is clear the relative divergence is diminished by a slight converging effort. If to start with the right lateral point is placed 6° away from the central one instead of 4° when the experiment is repeated, though the points appear separated by the same interval as before yet their position is of course reversed; yet they still run together. In this case the relative divergence is *increased* to meet the desire for fusion, instead of being diminished. Whether this is brought about by inhibition of the centres for the internal recti, or by antagonism of the external, remains yet to be found out.

Exp. 14.—Were the right lateral aperture, to start with, placed 5° away from the centre, and the last experiment repeated with the stop to the left, the two points would appear separated by nearly an inch, and with the stop to the right they would appear in the same vertical line. This enables the observation of a patient by the "direct method" to be easily confirmed, for all that is needed after taking the observation to ensure that fusion effort is eliminated, is to push the stop back to the right, and again to the left, to see whether the point at its first reappearance occupies the same position as before.

Exp. 15.—Use the *highest* lateral aperture on the right side, and with the stop to the right, move it till it meets and appears to fuse with the image of the central aperture. After fusion, push the right brass slide inwards slowly and steadily, and it will be found that the two blended images move to the left together, for the one which really moves carries the other with it to preserve fusion, and this goes on until the moving aperture travels right up to the central one, or at least as near to it as the construction of the box will permit, so that even this *false fusion* has sufficient power to undo the whole of the relative divergence.

If on the other hand the brass slide with the movable point it bears is drawn *outwards*, resolution of the two images does not occur till the points themselves are *really* separated by 10°. The desire to continue the false fusion is thus strong enough to *double* the previous divergence. Albeit the experiment makes the eyes water and feel uncomfortable. Whether this discomfort is due to *peripheral* antagonism of two sets of muscles, the external recti and the internal, I cannot tell; or whether it is due to a *central* struggle to overcome by inhibition a nervous connection probably never before invaded to that extent. Perhaps if the two points were on the same level the attainable divergence might be still greater. As it is, the deviation of 10° brings the eyes to within 4° of parallelism. This false fusion is of the same nature as that described by Sir D. Brewster, when, in looking at a patterned wall it is possible to converge the eyes for a point so far behind, or in front of the wall, that fusion of the laterally adjacent patterns takes place. The strength of the effort put forth to *maintain* accomplished fusion is much greater than that instigated by the desire to unite the two images when they are separated to begin with. The relative divergence attainable by the present experiment is much greater than, and must not be confused with, that attainable by the effort to overcome a prism, for in the latter case the two images are separated to begin with by the act of placing the prism before the eye. It may be deduced from what has preceded that, when the brass slide bearing the right luminous point is drawn outwards or pushed inwards, the *fused* images appear to follow at *half the rate*. But if they are *not* fused only one of them appears to move, and that at a rate *equal* to that at which the slide travels; the difference being that in the latter case both eyes are stationary, whereas in the former, while the left eye remains fixed the right one moves at the same rate as does the brass slide which bears the point of light it perceives. If the slide is moved jerkily slight momentary separations of the images result. If the effort to maintain false fusion be so strong, probably that to maintain *true* fusion is greater still. To measure it a camera should be used with two apertures at the same level, or else with a prism let into one of the small wooden slides before the eyes to just rectify the difference in level. It is the effort to maintain *existing* fusion which is tested by the common practice of approaching a finger to the eyes till one of them rolls out, though in this test *accommodation* increases at the same time, while in the camera accommodation is unchanged. I may note in passing that in my own eyes the nearest point of *single* vision by the finger test is closer to them than that of *distinct* vision, which illustrates a fact almost self-evident, that the relative divergence which occurs on the exclusion of one eye does not indicate deficiency in the converging function itself, but only in the *link* which connects it with accommodation. Accommodation assists convergence, and convergence accommodation, but they do so *only* through the central link which connects the two efforts, and enables one to influence the other. The slighter the link the less the effect one has on the other—but that has nothing to do with the individual strength of either.

Donders has shown that hypermetropes fix more easily when they look through prisms which make them converge more strongly. Is this because the converging effort assists that of accommodation by means of the central link between them? Apart from any pathological affection of either centre it is reasonable to suppose, since the sympathy is mutual, that if accommodation exerts only a weak influence on convergence, convergence will have a correspondingly weak influence on accommodation; a unit of either will contribute less than usual to the other.

If this be so, relative divergence, as revealed by the camera, since it indicates imperfection in the channel of mutual assistance, would lessen the advantage of the prisms above mentioned—though it would remove in their use all fear of their causing squint. But this is theory and needs practical confirmation.

A little confusion has arisen from the incorrect supposition that the *strength* of the *internal recti* is tested by prisms base outwards, and the ability to overcome them; whereas it is the conditions of the converging reflex as a whole which are thus estimated, including the existing intensity and activity of the desire for single vision. This is clear from the fact that when both eyes are directed to the right or left the contraction of the internal rectus may be greater than can possibly be attained by converging effort, the innervation called into play being a different one. Inability to overcome such prisms of high power *might* of course be due only to weakness of muscles, but in that case the ranging and converging movements would be equally impaired. Moreover, since accommodation remains unchanged, such prisms only indicate the limits of attainable relative divergence or convergence, which depend largely on the strength of the central nervous connection between convergence and accommodation. Approaching the finger to the eyes till one rolls outward is another method of testing the strength of converging effort, though *still* it is not the efficiency of the internal recti only that is indicated, but of the whole converging sensory-motor apparatus—afferent, central efferent, muscular, and mechanical. Strength of fusion effort is also influenced by the nature and doubtless by the size and number of the images to be fused, as well as by the amount of attention directed to them.

Exp. 16.—There are some cases of strabismus, especially of the divergent kind, in which, as Donders has pointed out, the mind becomes conscious of the direction of *each* eye. Such a person can correctly calculate the position of any object seen by either eye, and, indeed, employs one or the other just as convenience requires, being able to distinguish very readily which he is using for observation.

In testing a case of slight external strabismus with the camera, one would rightly expect to find that when the two images by the direct method are in the same vertical line there would be a very great interval between the actual apertures. This would be so in recent cases, or any in which one eye is disused in ordinary vision, for however much deviation might really exist, the images on both maculæ would still be mentally referred to the same vertical line.

But in cases like those mentioned by Donders the fact is that the eyes *correctly* estimate the distance between the two apertures, so that the images do not appear superimposed at all, unless the apertures themselves are made so, which the construction of the camera does not quite admit. The axis of the deviated eye does not follow the moving point of light, but correctly estimates its position as its image travels along the retina. An instance of this rather puzzling anomaly was tested with a camera by Dr Joseph Bolton. The "direct method" is useless for such cases, and will not detect even any deviation, being like the usual prismatic ones too subjective; but the "blind spot method" enables the exact position of either eye to be noted. A careful examination of a few of these cases might yield interesting results. All that is necessary to discover them is to try the two methods and see whether their records differ.

Exp. 17.—When the two images in the "direct method" are looked at for some time with a dim illumination they may be observed to alternately disappear altogether. This favours the current view that the part played in vision by the visual apparatus of the two eyes alternates in intensity; as each in turn becomes tired, the other gives it a rest.

Exp. 18.—While looking at the two images by the "direct method" shut the right eye; the image seen by it moves upwards and to the left, showing the eye itself has moved downwards and to the right. Why?

Exp. 19.—To ascertain the *rate* of the deviation of an excluded eye. With the stop to the right, and the right luminous point a considerable distance from the centre, look through the camera till convinced that no more deviation will occur. Move the right point inwards till its image seems just below the other, and *leave it in position.* After a timed interval of rest, take up the camera again and look at the central aperture with the stop *in the middle*, listening to the beats of a clock pendulum. At one end of a beat push the stop to the right, and count the number of seconds which elapse until the two luminous points meet, for the right one has been left in position all the while. The interval varies considerably in different cases and at different times, from half a minute to a minute and a half. In one case the total divergence of $6\frac{1}{4}°$ appeared to take from 68 to 73 seconds.

In the same case, when the two points were separated to begin with by 5°, the images took 40 seconds to meet. When separated by 2° they took 5 to 8 seconds. In this way the rate can be estimated for each degree. It is clear that the eye deviates outwards with *diminishing rapidity*, at any rate in the latter parts of the deviation.

IV. *Central Method.*

Exp. 20.—Place *both* lateral apertures out of sight, and use the central one only, looking at it with the stop to the *right*, for some seconds. Then push the stop quickly to the left. The first image disappears, and a second one takes its place to the left, at a distance from the former determined by the degree of divergence which has occurred. *Each* eye rotates through the same angle, sometimes as quickly as the stop is pushed, sometimes not until after an appreciable interval.

This is proved by repeating the experiment with the right luminous point out of view to start with in the right blind area, while the stop is to the right. This hidden test-point sometimes springs into view when the stop is pushed to the left almost simultaneously with the appearance of the left image of the central aperture, but sometimes not till a moment or two after. With the stop to the *right*, the *left* eye fixed the central aperture while the *right* deviated. Pushing the stop to the *left* excludes the *left* eye and lets the *right* eye see, so that either at that moment or a little later both eyes swing through an angle equal to the deviation, to let the right eye fix and the left eye deviate. Why, then, does the new image of the central aperture not seem to move? Because the *converging* innervation is unaffected by the change, and to it alone are all false estimates in the camera primarily due. The only innervation called into play for the movement in question is the "ranging" one, of whose efforts the centres are so well-informed that though the eyes move the image does not seem to. So wonderfully is the correction made that even in nystagmus, though the eyes continually oscillate unknown to the patient, he never, according to Helmholtz, sees fixed objects moving. So truly, also, through the higher centres does the mind estimate the amount of this effort which is in exercise, that artists are said to be able to judge more correctly the lateral distance between the two objects by glancing rapidly from one to the other, than by any other visual method.

Exp. 21.—To ascertain *how soon* the deviation begins. Let a thin strip of india-rubber connect one end of the stop with the left side of the box, while a piece of string passes from the other end of the stop round the forefinger of the right hand. Give the string, to begin with, just such a degree of tension as to keep the stop in the *middle* while both eyes look through the camera at the central aperture. Listen to a clock pendulum beating half-seconds. At one end of a beat pull the stop instantaneously to the right, and at the other end of the beat let it fly back to the left; in so doing it exposes a transitory double image of the central aperture, or at least a perceptible widening. This shows that divergence has commenced *in less than half a second.* In how *much* less still remains to be noted, which may be done by shortening the pendulum; but great quickness of observation would be needed to attain any degree of accuracy.

EXP. 22.—Let the central aperture be alone used, and push the stop, alternately from left to right and right to left, at definite intervals. If the intervals are not too short relative divergence sets in, as shown by the apparent displacement of the image to right or left at every movement of the stop. The vision of the central aperture is alternately monocular and binocular, the proportion between the two being determined by the *rapidity* of each movement and the *length of the interval* between them. At certain rates the relative divergence gradually *increases*, as shown by the greater and greater apparent displacement with each excursion, which proves that a certain proportion of binocular vision to monocular is required to overcome the natural tendency to divergence. The desire for single vision is, as it were, *diluted* by interruptions. There appears to be a certain rate at which the relative divergence neither increases nor diminishes, and a quicker one still at which, if divergence is present to begin with, it slowly diminishes, and yet a further rate at which it does not appear at all; but a mechanical apparatus would have to be used to obtain really reliable results. The width of the stop, of course, greatly affects the proportion of binocular vision, as also does the length of its slit. If the stop were just wide enough to cut off in the *middle* of its course the view of the central aperture by both eyes, vision would be wholly monocular; with each diminution of size there would be a larger proportion of binocular vision.

It may be that this method would furnish a comparative means of estimating the efficiency of the fusion centre, by noting the *amount* and *nature* of the dilution required to make the desire for fusion incapable of preventing either the occurrence of relative divergence or its continuous increase. Two points would have to be considered—the *frequency* of the interruptions, and the *length* of each; the former would depend on the *number* of side to side movements per minute, the latter on the *rate* and length of each movement, the *pause* at the end of each, and the *width* of the stop. It is possible that a certain *duration* of the two pictures in the brain is necessary to elicit the desire to fuse them at all.

If a wide stop were used, and an up and down movement given to it in the slit, binocular vision would be diluted, not by monocular, but by intervals of no vision at all. Whether the result would be the same I do not know.

EXP. 23.—If, after looking at the central aperture for a little time with the stop to the *right*, the latter then be pushed quickly to the middle, the central aperture appears duplicated for a moment by the addition of another image to the left, and the two run quickly and with equal velocity into one. The appearance of this second image shows, of course, that the eye has deviated. The apparent angular separation of the two images is equal to the angle of deviation. If they could be kept in their first position (and this we shall see may be done), an effort to touch the right one would place the finger 2½° to the right of the middle line, and an effort to touch the left would show it to be likewise 2½° to the left of the middle line. We have seen how when the stop was at first pushed to the right, and the right eye deviated 5°, that the only then existing image of the central aperture seen by the left eye appeared to move till it was referred 2½° to the right of the middle line. The right eye was then *excluded*, but now, when the stop is put back in the middle, it receives an image on the retina 5° to the right of the macula, and which is therefore referred 5° to the *left* of the image seen by the left eye.[1] Since the latter image appears 2½° to the *right* of the middle line, the former must be 2½° to the left of it. The left eye remains stationary throughout, while the right one (no longer excluded) now returns through the same angle through which it deviated. Why, then, if the right eye *only* is moving, and that through an angle of 5° (and this is it easy to verify by testing the positions of the *blind* areas), does *each* aperture appear to traverse an equal angle of 2½° ? It is just the *undoing* of what happened when the obstructive was pushed to the right. *Then* the image seen by the left eye seemed to move *slowly* to the right for 2½° ; *now* it *quickly* appears to return, because the desire for single vision has aroused the supplementary converging effort, which so affects the innervation of the left internal rectus that there is no longer any need for that effort which usually turns both eyes to the right; it therefore ceases, and just as the mind took cognisance of its introduction, and imagined the point seen to move to the right, so it takes cognisance of its cessation, and refers the point again to its original position. In like manner the second image (seen to the left by the right eye) though it traverses 5° of the retina, only *seems* to move 2½°, because that is the only moiety of the movement which is due to cessation of the mentally-recognised ranging effort; the other half being due to positive converging effort, of which no mental account is taken as regards horizontal position. Half the movement of the deviated and returning eye is due to relaxation of the external rectus, and the other half to increased contraction of the internal rectus. The former is taken into account mentally, the latter is not. It is quite clear from this that the oculo-motor muscular sense is *purely* central, for the *same* contraction of a muscle is appreciated or not according to the *central* source of the effort.

Exp. 24.—(*a*) Place the right lateral aperture, to begin with, 4° or 5° away from the central one, and the stop to the left. Two images are now seen by the right eye, the right lateral and the central one, and their *relative* distance is correctly estimated, though the position of both is miscalculated 2½° to the left. Now push the stop in the *middle*; this uncovers the deviated left eye, and reveals another image of the central aperture miscalculated 2½° to the *right*, so that it appears

[1] These angles have their apex at the principal dioptric centre.

just above the right lateral aperture, which is miscalculated $2\frac{1}{2}°$ to the left, as we saw. But the two images of the central aperture are only thus separated for a moment, for they quickly run together, and normal fusion takes place.

(*b*) If, however, instead of starting with the stop to the *left*, the stop be first placed to the *right*, and then replaced in the middle, the images do *not* run together, though the relative position of the three points is exactly the same as before. In the latter trial, the desire for false fusion at the near distance is greater than the desire for true fusion at the greater one; but why it should not be so in the first trial I cannot certainly explain. It may be that the desire for false fusion of two objects not in the same vertical line takes longer to develop its strength than that for true fusion, and has not time in the first trial to do so before the movement for the fusion has begun. The effect of attention, as seen in Exp. 26, has probably more to do with it. From the construction of the camera, the vision of the right lateral aperture cannot be other than monocular, so there must be only one image of *it*, but the vision of the central aperture, though *mon*-ocular when the stop is either to the left or the right, is *bin*ocular when it is replaced in the middle, so that, deviation having in the meanwhile occurred, there are two images of it. The only difference between the two trials lies in the *order* in which these two images appear. In a third modification they may be made to appear simultaneously.

(*c*) Push inwards the left brass slide till it just occludes the central aperture. Let the right luminous point (now the only one visible) be placed as before, and the stop in the middle. On quickly drawing out the left brass slide both images of the central aperture appear at once, and generally run together, though the result depends somewhat on the position of the right lateral point and the amount of deviation that has been permitted to occur. This experiment may be repeated with many variations by anyone desirous of ascertaining the laws of fusion; the left lateral aperture, *e.g.*, may be used instead of the right, and each in different positions, or both may be used.

Exp. 25. To ascertain the effect of *attention* on the desire for single vision. Place the right lateral aperture, coloured blue, $2\frac{1}{2}°$ to the right of the central one, which is covered with a piece of paper or ground glass, and therefore white. On looking into the camera with the stop in the middle, the two points are seen in their true positions, the white one appearing nearly half an inch to the left of the blue one. Now push the stop to the right; the two images begin to move slowly together till they come to be in the same vertical line, and again separate by each pursuing its movement till they have just changed places. The white image is now nearly half an inch to the *right* of the blue one. When the stop is replaced in the *middle*, another white image appears nearly half an inch to the *left* of the blue one, which now has a white image on each side of it, and at equal distances from it. If, while moving the stop, *attention* is directed to either of the *white* images, they quickly run together, while the blue returns to its original position. But if the attention is concentrated on the *blue* point the whole time, the white ones do *not* run together, but remain as at first, one on each side of the blue one. This shows that the mere *presence* of double images, when they are perfectly well defined, does not excite the desire for single vision, unless one of them becomes the special object of attention.

If the blue point is not exactly halfway between the two white ones, move the brass slide which bears it until it becomes so, and then read off its angular position, which, when doubled, will give the angular deviation, or relative divergence of the eyes.

This, then, is the *third* method of measuring it by the camera, not of any clinical value, but useful as showing that the deviation is practically the same in extent under such varying tests, for—

(1) In the "blind-spot" method there is one image upon the fovea of *one* eye.

(2) In the "direct" method there are two images, one upon the fovea of *each* eye.

(3) In this "central" method one eye receives an image on its fovea, and each eye receives an image away from its fovea.

It would be wearisome to recount more experiments, as the use of the camera permits of so many variations. Before passing to the next section on "Distant Vision" it may be well to give a convenient summary of the results obtained in near vision.

1. When one eye is excluded from vision and placed subjectively in the dark, it nearly always deviates outwards (Exp. 1).

2. The same deviation occurs if *each* eye is made to receive an image or any number of images, provided that the desire for fusion is in abeyance (Exp. 12).

3. The average angle of deviation appears at present to be about $4\frac{1}{2}°$ with vision for 10 inches.

4. There are four methods of measuring this angle—three by the camera, and one by a double prism modification of Von Graefe's test.

5. When the record by the "blind-spot" method differs from that of the other three, it is because the mind has learnt to estimate the position of each eye separately (Exp. 16).

6. There are reasons why Von Graefe's clinical method has not revealed *physiological* divergence.

7. The divergence begins in less than half a second (Exp. 21), and continues gradually at decreasing speed (which may be measured at any point) for from half a minute to a minute and a half (Exp. 19).

8. Half the deviation of the excluded eye is due to contrac-

tion of its external rectus, the other half to relaxation of the internus.

9. The oculo-motor muscular sense is purely central; the same contraction of a muscle is mentally appreciated in one way or another according entirely to the central source of the effort (Exp. 23).

10. The truth of Hering's theory that the horizontal movements of the eyes are governed by two innervations, each acting on both eyes as a single organ, is repeatedly demonstrated.

11. The object fixed by the seeing eye appears during the deviation to move in the same direction; the apparent movement is at half the rate and through half the angle of the real movement of the excluded eye (Exp. 3).

12. An image on the fovea, whatever the real position of the eye, is referred to the plane which bisects the angle of convergence, and which therefore passes through a point midway betwen and slightly behind the centres of the two eyes (Exp. 2 and 3).

13. A fixed object seen by a stationary eye may appear to move, and the same fixed object seen by a moving eye may appear stationary according to the innervations in play.

14. The degree of deviation which occurs on exclusion is greater in the early morning, often falls after meals, and is subject to oscillations, according to conditions which affect the nervous system.

15. A large degree of convergence is still centrally connected with the accommodating effort, though its amount differs greatly in different persons, being in some more than twice as much as in others.

16. It is probable that these differences account for the fact that squint develops in many cases of hypermetropia where there is less refractive abnormality than in other cases where squint shows no tendency to occur.

17. The connection between the converging and accommodating efforts is still very delicate; the slightest alteration in the latter is accompanied by an alteration in the former (Exp. 5).

18. The degree of convergence centrally attached to accommodation is subject to slight waverings (Exp. 8). It may to a certain extent be either increased or diminished by the desire to

unite two images, and to a still greater extent by the desire to *maintain* their fusion (Exp. 13 and 15).

19. Images at slightly different levels in the two eyes, even when the difference in level is great enough to prevent their fusion, are often kept near each other by the desire for it.

20. This tendency decreases rapidly with increasing difference in level, and is not perceptible with the images separated by a vertical angle of 2° or 3°.

21. The desire for this false fusion at a near distance may be greater than the desire for true fusion at a greater distance, though this is affected by the *order* in which the desires are roused (Exp. 24).

22. When the images are *coloured* differently the desire for fusion is weakened but not altogether removed.

23. The desire for single vision can be interrupted to any required extent by causing alternations of binocular and monocular vision, so regulated that, with different rates, deviation may be either prevented, retarded, arrested at any part of its course, or made slowly to retrogress (Exp. 22).

24. The effect of *attention* exerts a well-marked influence on the desire for fusion (Exp. 25).

25. The ordinary test of placing a prism (base in or out) before one eye estimates simply the degree of relative divergence or convergence which is attainable by the desire to rectify the diplopia created by the prism, and which is compatible with the existing effort of accommodation.

26. Approaching a finger to the eyes tests the power of *maintaining* existing fusion with proportionately increasing accommodation. This is true up to the nearest point of distinct vision, within *that* it tests the relative convergence attainable by the desire to maintain fusion complicated with increasing indistinctness of the images.

(To be continued.)[1]

[1] The writer will be greatly obliged for the pointing out of any omissions and errors detected in this paper.—*Address,* E. E. Maddox, M.B., *Shipton, Chipping Norton, Oxon.*