INVESTIGATIONS IN THE RELATION BETWEEN CONVERGENCE AND ACCOMMODATION OF THE EYES. By Ernest E. Maddox, M.B. Edin., *Syme Surgical Fellow in the University of Edinburgh.*[1]

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

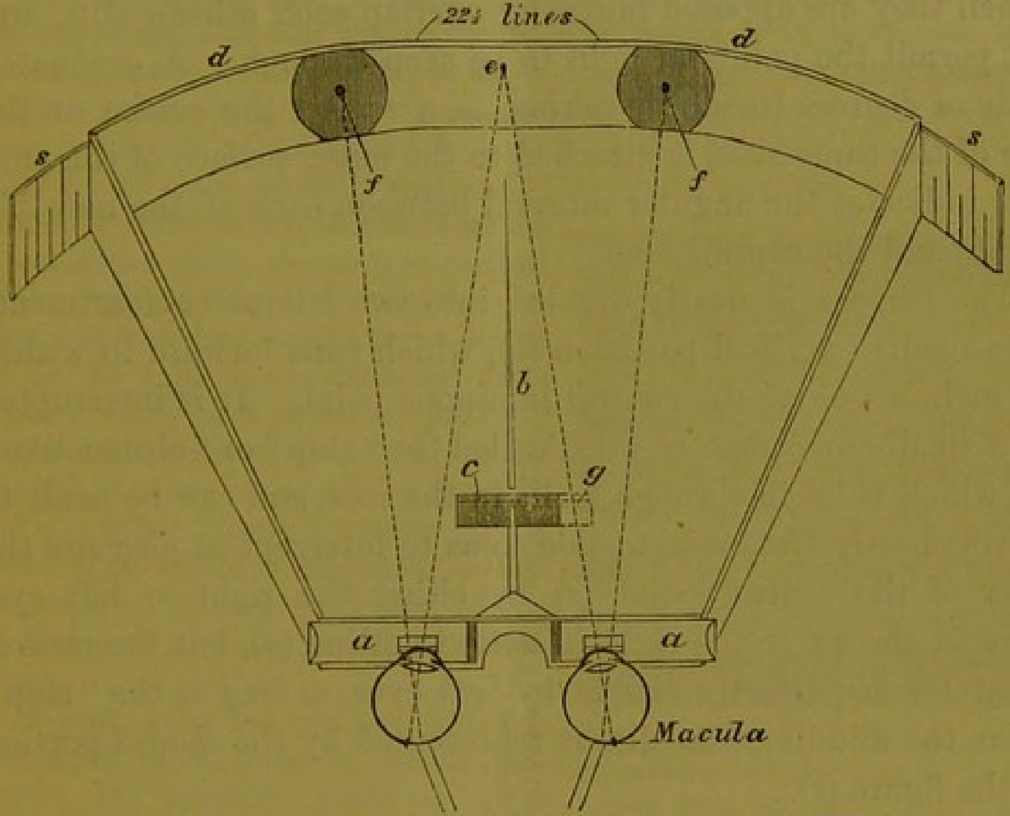


FIG. 1.—View of the visual camera with the roof removed.
(*Erratum.*—The dotted lines should cross *in* the crystalline lens instead of behind it; 22½ lines should be 28½ lines.)

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.

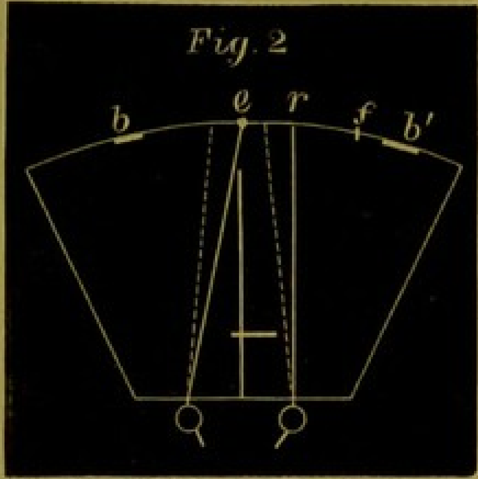


FIG. 2.—The vision of the central aperture (*e*) being cut off by the stop from the right eye, its axis has deviated from *e* to *r*, and its blind area (*b'*) has moved to exactly the same extent, so that it no longer conceals the point of light (*f*). The *left* blind area (*b*) does not move, showing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

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.

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.

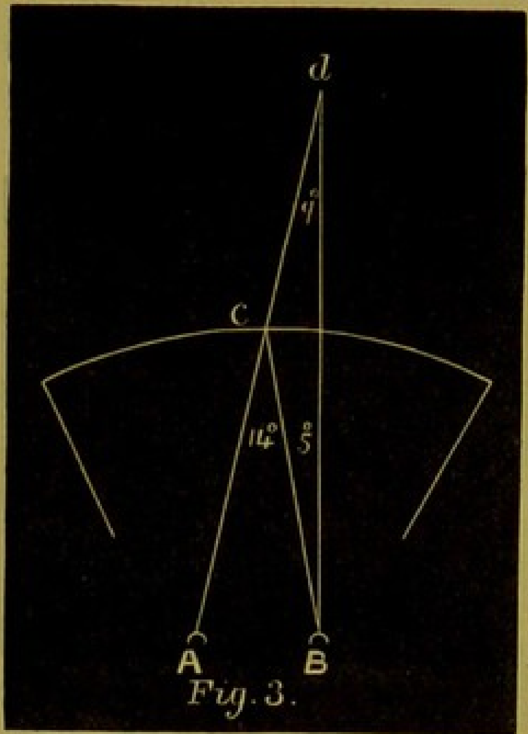


Fig. 3.—AcB was the optic angle before the right eye deviated. AdB is the optic angle *after* deviation; it is less than before, by the angle of deviation cBd.

[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 1½ dioptres less of accommodation to be in exercise (2½ 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 4½°, 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.	12½°	18¼°	5¾°	0°
2.	12½°	18½°	6°	1° or ½°
3.	12½°	19°	6½°	2½°
4.	11°	17°	6°	4°
5.	12½°	18½°	6°	4½°
6.	12½°	18½°	6°	5°
7.	13°	19°	6°	6½°
8.	13°	18½°	5½°	7°
9.	11½°	17°	5½°	7°
10.	12½°	18½°	6°	7½°
			Average, .	4½°

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 6½° 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°.

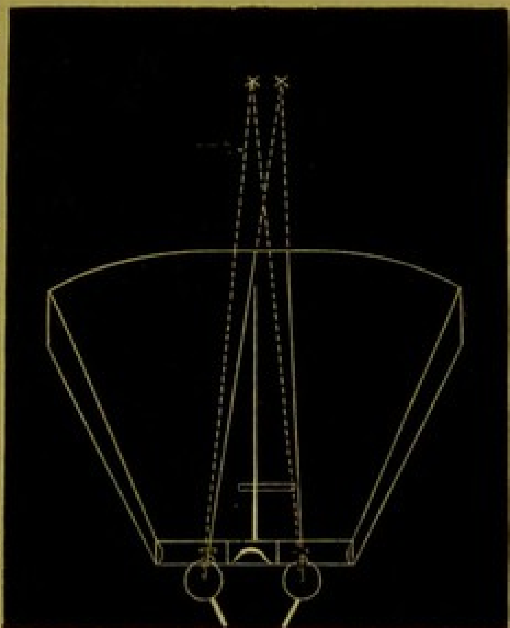


FIG. 3 A.—Convergence of the visual axis as if for the left hand cross is effected by the converging innervation; but they are jointly deflected to the right hand cross by the ranging innervation; in accordance with Hering's theory.

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

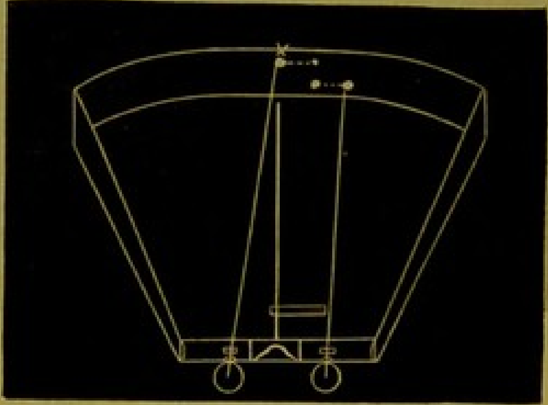


FIG. 3 B.—Illustrates the "direct method." The apertures *appear* superimposed though *really* separated by the deviating angle of the eye.

[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.

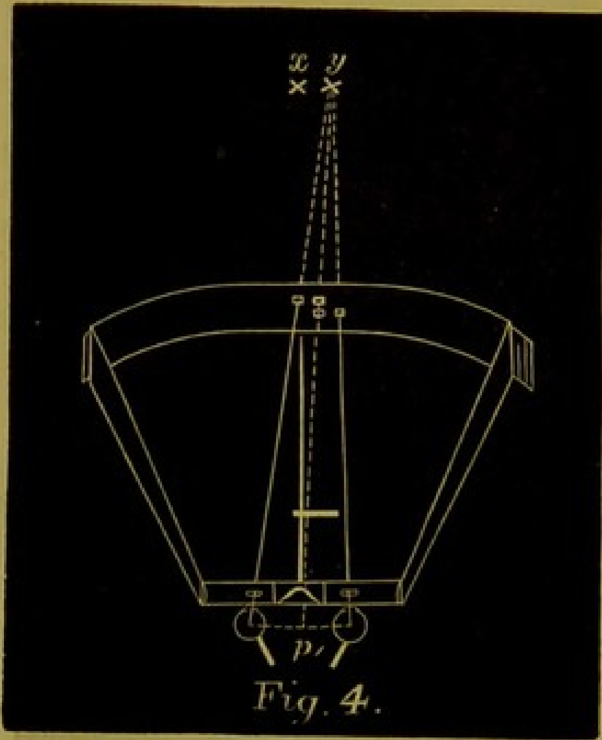


FIG. 4.—The direct method. Each luminous point throws an image on the fovea of the eye on the same side, so that both images are mentally referred to the plane which bisects the angle of convergence.

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

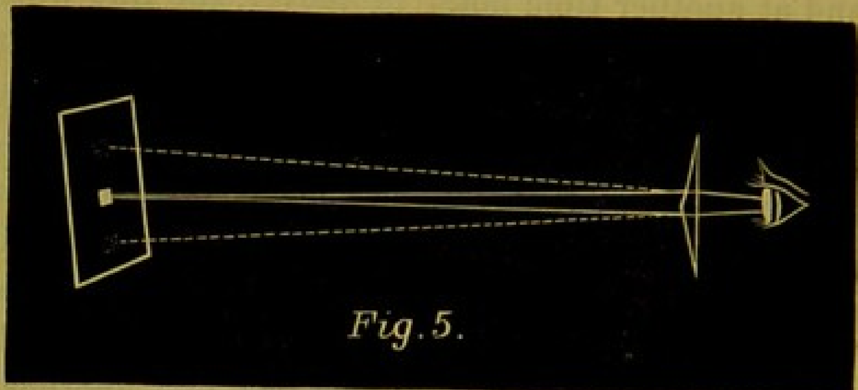


Fig. 5.—Side view of the right eye and the double prism. The false images seen by the right eye are dotted. The central one is seen by the left eye.

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

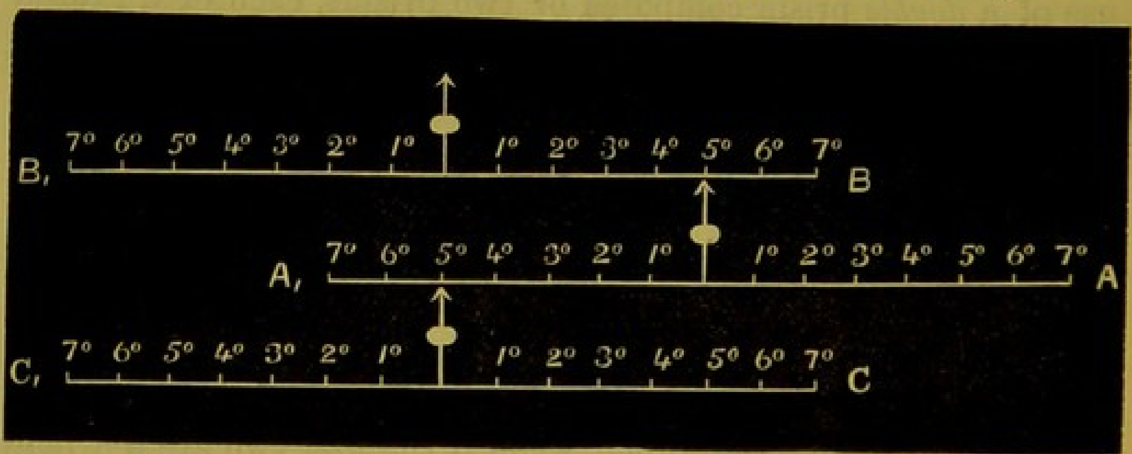


Fig. 6.—To illustrate how relative divergence is measured by the double prism. A is the only device on the card, and is seen by the left eye; B and C are false images of it, and are seen by the right eye. In this instance 5° of deviation are seen recorded. If the two lowest arrows are made continuous 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 $2\frac{1}{4}°$ (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 $2\frac{1}{2}°$ to the right of the middle line, and an effort to touch the left would show it to be likewise $2\frac{1}{2}°$ 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 $2\frac{1}{2}°$ 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 $2\frac{1}{2}°$ to the *right* of the middle line, the former must be $2\frac{1}{2}°$ 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 $2\frac{1}{2}°$? 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 $2\frac{1}{2}°$; *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 $2\frac{1}{2}°$, 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 $2\frac{1}{2}°$ 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 $2\frac{1}{2}°$ 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 $2\frac{1}{2}^\circ$ 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, $2\frac{1}{2}^\circ$ 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.*

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

By ERNEST E. MADDOX, M.B., C.M. Edin., *Syme Surgical Fellow in the University of Edinburgh.*

(*Continued from* p. 508, vol. xx.)

V. *Distant Vision.*

WE have seen that with near vision the visual axis of an excluded eye generally deviates *outwards* from its fellow; it appears to be just as usual with distant vision for an excluded eye to deviate *inwards.* This is easily shown by pricking two pin-holes through a piece of paper at a distance of rather less than 2½ inches from each other. On holding them horizontally before the eyes, and looking through the left aperture with the left eye at a distant object, the two circular images vary in their apparent relative position according to the distance of the paper from the eyes in such a way as to demonstrate the presence of relative convergence. More need not be said about this, since the camera acts upon the same principle.[1]

EXP. 26.—The central and right lateral apertures[2] are used as in fig. 6, the stop being to the right.

The observer, instead of looking at the central aperture (E), as in testing for near vision, now looks through it at any very distant object, and while doing so moves the right slide till its aperture *appears* to lie just below the image of the central one.

If then the distance between the *actual* apertures be measured, they will be found separated by an interval rather *less* than the distance between the centres of the two eyes. Now, since the left eye is looking directly at the object, the image of the object, as well as that of the aperture which encircles it

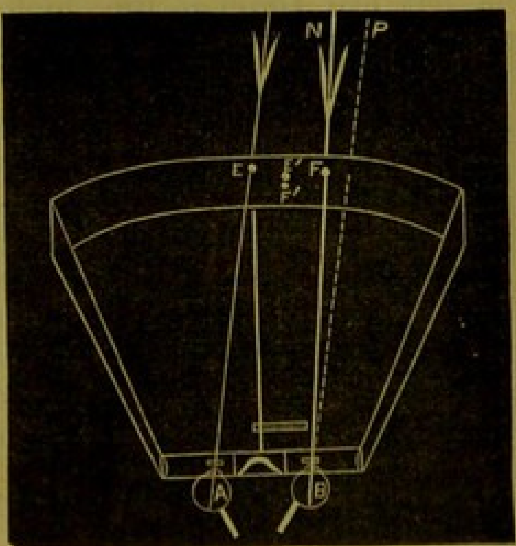


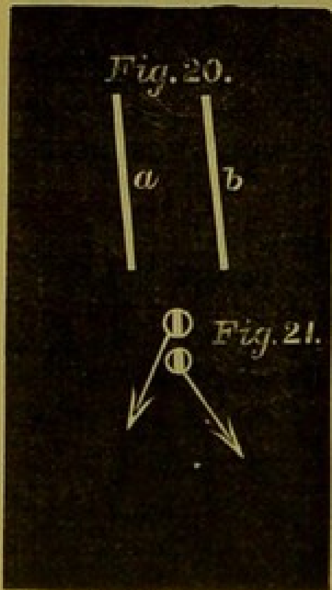
FIG. 6A. To illustrate the direct method with distant vision. (Erratum—E should be level with E′, and F with F′.)

[1] A notice of this experiment was kindly communicated for me by Prof. Crum Brown to the *Proc. Roy. Soc. Edin.* in 1883–1884.

[2] At the time when the figure was made I used the right or higher lateral aperture. I now use the left or lower one to eliminate more completely the desire for fusion.

as in a frame, must fall on the left fovea centralis or point of acutest vision. The encircled image therefore is referred—where all foveal images are referred—to the line which bisects the angle of convergence. But the other aperture has been placed so that its image appears to be in the same line, or rather slightly below it; it therefore must fall exactly above the right fovea, on the median vertical meridian of the right retina. Since each image falls on a median vertical meridian, it follows that if the apertures themselves were separated by an interval equal to the intercentral distance, the visual axes would be parallel; if the interval were greater there would be relative divergence, but as it is, the interval is less, showing relative convergence.

Moreover, if, while the apertures are still kept in position, *both* eyes be made to observe distant objects through them, the images none the less appear superimposed; the amount of convergence attached to distant vision remains unaltered, whether one eye or both is used.[1] Since the natural outflow of energy from the converging centre when the desire for fusion is absent is a delicate comparative index of the accommodating energy, this fact shows that the activity of the accommodating centre is no greater when both eyes are used than when vision is confined to one, and corroborates the statement that accommodation is the work of a single innervation affecting both eyes equally at the same time. The object thus seen by the right eye, through the lower aperture, is one which really lies in a space to the *left* of the object seen by the left eye through the higher aperture. Thus, if in figs. 7 and 8, *a* and *b* are two distant objects, *a* is seen by the right eye through the lower aperture, and *b* by the left eye through the higher one. Fig. 9 shows that for this to occur the visual axes must cross somewhere between the camera and the distant objects. This crossing point is at an average distance of about 112 inches from my own eyes.



Figs. 7 and 8.—Objects (*a*, *b*) seen through the apertures of the camera.

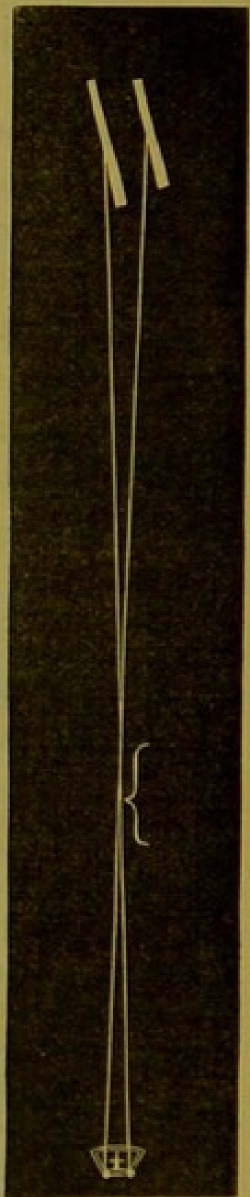


Fig. 9.

Another glance at figs. 7 and 8 will make it evident that for the same object (*b*) to be visible by *both* eyes, the lower of the two apertures must be drawn away from its apparent position just under the other to the right. Let this be done till "*b*" is visible in both apertures, the *actual* distance between them will

[1] This presumes the possession of eyes of equal refraction.

then be the same as the distance between the centres of the two eyes, while the *apparent* distance between them will indicate the degree of relative convergence present. To measure it, we need only take the *difference* between the number of degrees now recorded and that previously recorded when the lower aperture appeared just under the other. In taking these observations on others it is essential, to obtain accurate results, that the arms should be supported; or better still, that the camera itself should be hinged (as mine is) on a stand, so that by telescopic action it can be raised or lowered to any required height, or inclined at any angle. It is difficult otherwise to maintain the requisite steadiness, and the arms get tired before the observation is complete.

The amount of Relative Convergence with negative accommodation.—This varies in different individuals, being apparently much greater in some than in others. In my own case fifty observations of the amount of relative convergence, associated with very distant vision, gave the average of 1° 18′ 43″. By more than neutralising my half dioptre of manifest[1] hypermetropia I have never succeeded in reducing the convergence lower than 28′ 30″. For a hypermetrope such convergence excites no surprise; it would indeed be looked for, since the ciliary muscle, unlike that of an emmetrope, is never quite relaxed in distant vision, and a certain amount of attached converging effort might therefore well be expected to cling to it. But in fact the convergence noted is less in degree than with most of the emmetropic eyes I have tested. The unexpected feature is the comparative *smallness* of it, in the presence of hypermetropia. Were the connection between the two efforts as complete as it was once thought to be, as much as three and a half degrees of convergence would accompany each dioptre of hypermetropia.

Dr Bolton in six cases, without known hypermetropia, obtained an average inward deviation of 1° 38′, and I found that of a similar number of records to be 2° 16′ 14″. The variations in these twelve persons were from 0° to 4°. Large relative convergence in distant vision appears to be generally associated with small relative convergence in near vision, and *vice versâ*, though not without frequent and sometimes striking exceptions. In one instance slight *divergence* was found by Dr Bolton with negative accommodation, and in another, relative convergence

1 "Manifest" hypermetropia is that which is discoverable without the use of atropine, while the remainder is latent.

noted for some time gave way, after a great nervous expenditure, to parallelism or even slight divergence of the visual axes, which continued for several days. I find that in the absence of exceptional causes of disturbance the amount of relative convergence attached to negative accommodation goes through a fairly uniform variation through each day, becoming greater as the day advances, but suffering a fall after each meal, though more especially after the principal midday one. The average A.M. record was 1° 5′ 12″, and the P.M. record 1° 24′ 33″, while the after-dinner one was 1° 3′ 4″. The relative divergence in near vision (at 10 inches) diminishes through the day, though by far the greatest fall occurs during the first hour after rising. The convergence attached to negative accommodation and that attached to great positive accommodation do not, however, rise and fall together, for the relative divergence in the latter as noticed by the camera is *lessened* shortly after a meal, especially after the midday one, showing that the attached convergence is increased at the same time that that of negative accommodation is diminished. The average deviation, with vision for 10 inches, before the eyes were otherwise opened in the morning, was 7° 36′. In this I have not included an exceptional record of about 3° 35′ after disturbed sleep, consequent on the uncustomary taking of a supper on the previous night. It is known how irritation of the primæ viæ may cause temporary nervous strabismus in children; this record is so interesting, as showing how the same condition which causes a pathological effect at one age may at a later one cause only an unnoticed effect on the physiological condition of the centres, that I give here the observations of that day, and the one before and after it.[1]

	7.15 A.M.	7.55 A.M.	8.20 A.M.	9 A.M.	10 A.M.
First day, . . .	7° 45′	7°	6°	...	...
Second day, . . .	3° 35′	...	4° 15′	5° 46′	6° 26′ 28″
Third day, . . .	8° 20′	6° 37°*	...	...	...

It may be noted that while the records of the first and third

[1] Owing to an imperfect marking of the camera these records are all slightly too large, though uniformly so; they serve for comparison only. The one marked * was earlier than the time given.

days *diminish* as usual, those of the second day gradually *increase* to the usual amount. Before attempting to estimate the effect of drugs and different conditions on the brain centres, it would be well for the observer to become acquainted with his own diurnal variations.

Point of Coincidence.—When an object is moved along that horizontal line of the sagittal plane which is level with the pupils, and looked at with one eye, there is a certain distance at which the meeting-place of the two visual axes coincides with the object. I name it the "point of coincidence," since here the attached convergence coincides exactly with the accommodation to which it is attached. At this point there is neither relative convergence nor divergence; when the object is beyond it, there is the former; when within it, the latter. In some it might be rather a region than a point. Were the object moved along different radiate lines in the horizontal plane with direct oblique vision, the points so discovered would constitute a horizontal "line of coincidence," and similarly a vertical line of coincidence could be found.

My own point of coincidence in the line first mentioned, which runs straight forward from the root of the nose, has varied from 56 inches to 14 inches distant. As might be expected from the fluctuations in the amount of deviation attached to near and distant vision, it shifts from minute to minute, according, doubtless, to the occupation of the eyes as well as the conditions of the nervous system.

The camera furnishes the most exact method of finding the distance of the point of coincidence, but the double prism is a much more ready means. With the former an object is approached till it is visible in the central and one of the lateral apertures at the same time that the apertures themselves appear superimposed; or, better still, a card coloured red to one side of a vertical line and green to the other, and provided with printed matter to insure exact accommodation, is carried on a traveller of a strip of wood hinged on to the stand of the camera. The left brass slide of the camera is withdrawn more than half its own length, so as to leave a short stationary *slit* instead of an aperture. With the stop to the left, place the card so that the median line on it is visible through the central aperture by the right eye; it is then made to approach or recede till the lines seen in the slit and the aperture appear continuous. This method permits of great nicety as well as speed. It is very important to insure that the eyes are accurately accommodated for the object. When this precaution is taken, the variations are far less. Strangely enough, I have uniformly found the distance of the point of coincidence greater after very near vision of fine print, and less after distant vision. Another method I have employed is to use a long graduated strip of wood, at one end of which the double prism[1] can be fixed at pleasure; it rests on the mantelpiece or any convenient horizontal situation, and a night-light or other appropriate object is moved along it till the three images are exactly in a line. Objects should be avoided which have vertical edges if they are long enough to overlap. For self-trial a wooden traveller is used, which can be pulled backwards and forwards by a piece of string. This is the easiest expedient, but is not so exact or trustworthy as that with the camera.

When diplopia is heteronymous, it shows the point of coincidence is *beyond* the object, which must therefore be made to recede, and conversely to approach when diplopia is homonymous. It is easy to see the bearing of this point on the well-known clinical test, in which a flame at some distance serves for the object of view and a prism (base vertical) is held before one eye; of the two images which appear, one passes to the right or left of the other, according to the existing relationship between accommodation and convergence. The *distance* of the flame from the observer has been thought a point of little consequence, but the whole result depends upon it. If the distance is less than that of the point of coincidence, relative divergence will appear, while if greater, relative convergence will betray itself; provided, of course, that the prism is held absolutely vertical. If neither deviation is observed, the flame is exactly at the point in question; but if this happened with the flame at the usual distance of 7 or 8 feet, there would almost certainly be observed a wide deviation outwards if near vision were tested. Since the distance of the point of coincidence is generally less than that of the flame, slight relative convergence would be almost uniformly detected were the method sufficiently accurate, though the prismatic errors liable to occur in this test are far less than in those which require prisms of higher powers. The smaller the refracting angle of a prism, the smaller is the degree of accidental displacement of the image which attends any rotation of the prism from the vertical.

Seven cases examined by Dr Bolton showed an average of 43 inches for the distance of the point of coincidence. In one case it was 97 inches, about the usual distance of the flame in the clinical test spoken of, the result of which, therefore, in this case would have been negative, though with vision for 10 inches there was a deviation outwards of 7° recorded by the camera. Since the average distance of the point is less than that of the flame, relative convergence would generally show itself if the test were carried out with sufficient accuracy. There might be great outward deviation in near vision, or inward deviation in distant vision, or both, and yet neither be detected by the test as usually employed.

[1] The prism used is of a square outline, which gives no trouble in adjusting to the vertical, since one side rests on the wood.

The point of coincidence is of interest, because whenever vision of daily life wanders beyond it, the fusion effort, instead of acting through the converging apparatus, acts through some diverging mechanism, which is doubtless connected with the external recti, though it is just possible that it acts by inhibition of the converging centre. The nearer the point of coincidence is to the eyes the more fusion work falls through the day to the diverging mechanism and the less to the converging mechanism, and *vice versâ*, so that its position is what determines the "division of labour" for these two innervations in their connection with the desire for single vision.

I may add that when an object is seen just within the "near point" with the double prism before one eye, relative divergence may for a moment disappear, or even give way to relative convergence, so great is the fruitless effort to accommodate more. The effort shows itself by the sympathetic effect on the converging centre—a good illustration of the fact that it is not accommodation and convergence that are united, but the nervous *efforts* in the ganglia of the 3rd nerve which cause them. For this experiment care must be taken to avoid obliquity of vision or to allow for its presence.

If two horizontal slits are cut in a piece of paper or cardboard, so that the inner end of each terminates in the vertical middle line of the card, and yet so that one is on a level about $\frac{1}{3}$ inch higher than the other, the point of coincidence can be found in a very simple and inexpensive way, without either prism or camera, by looking through the slits at any vertical linear object till it appears continuous in each. It is not very trustworthy, however, in a patient's hand, since obliquity may cause fallacy.[1]

The Three Grades of Convergence—We may now distinguish the three elements of that complete convergence which occurs with binocular vision of a near object.

1.[2] *Initial Convergence*—which is that attached to negative accommodation.
2. *Accommodative Convergence*—which is that, in addition, attached to positive accommodation.
3. *The Fusion Supplement*—which is that excited by the desire for fusion to make up, when it can, for the deficit (or excess) of the other two.

[1] Two horizontal stenopaic slits at different levels, placed in the ordinary trial-frame, would act on the same principle.

[2] It is convenient to use contractions for these—I.C., A.C., and F.S. respectively; also, R.C. for relative convergence, R.D. for relative divergence, and P.C. for the point of coincidence.

Fig. 10 illustrates these three factors in looking at a point O. The first brings the visual axes from parallelism *pp* to *ii*, the second to *aa*, and the third to O. Were O at the point of coincidence it would coincide with *aa*, and the third factor would be engaged only in steadying the eyes by counteracting the tiny inequalities in the evolution of that nervous energy which maintains the other two. For objects beyond the point of coincidence there are only the first two factors in convergence in exercise, the fusion supplement being employed no longer in *augmenting* but in partly *neutralising* their effect.

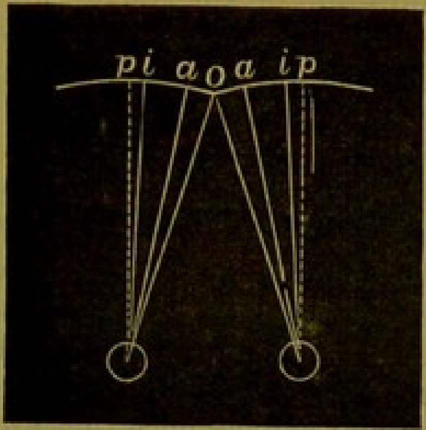


FIG. 10.—The three elements of convergence in near binocular vision of a point O.

The distinction between the initial and accommodative convergence is important, because they are unequally affected by different conditions. Thus we have seen that the effect of a meal is to diminish the former and increase the latter so much, that it more than makes up for the initial diminution. Initial convergence is affected directly by the sympathetic system through the external recti, while the accommodative convergence is only affected by changes in the cerebral centres of the 3rd nerve when vision is near, and the 3rd and 6th when vision is more remote. We have already seen how to measure the initial convergence and the relative divergence in near vision—the difference between the two gives the accommodative convergence.

Squints may be analysed into their initial and accommodative elements by careful measurement, first with negative accommodation, and then with positive, by the direct or blind spot method, as required.

The accommodative convergence can itself be analysed by finding how much of it belongs to each dioptre of accommodation. To do this objects are viewed at 1 m., $\frac{1}{2}$ m., $\frac{1}{3}$ m., and $\frac{1}{4}$ m. distance in turn; but an average of a great many experiments must be made to get results of any value. Dr Bolton's experiments[1] show that the convergence attached to the *first* dioptre is the greatest in his case, being 3° 18′ 27″. To the 2nd D was

[1] Kindly made at request, February 1886.

attached only 45′ 18″; to the 3rd D—2° 54′; and to the 4th D—2° 20′. The excess of the first over the others fully accords with the fact that the point of greatest R.C. is not at practical infinity. Objects at the same distances give in my own case 1° 53′ 42″—1° 13′ 37″—1° 41′ 40″—and 2° 44′ 24″ of accommodative convergence for these four dioptres respectively. The first is greater than the next two, the second is, as with Dr Bolton, the least, but the fourth is the greatest of all, owing doubtless to the greater effort needed for this dioptre than for the previous ones, just as a man must put forth more effort in raising a weight through the fourth foot from the ground than through the first foot owing to increasing mechanical disadvantages. A dioptre is a unit of work, not of effort, and since in a hypermetrope these four dioptres are not the *first* four, the effort required for each begins to increase more rapidly than in an emmetrope. No doubt each succeeding dioptre, and each succeeding degree of convergence, needs more effort than the preceding one, but the resistance experienced by one effort may increase more rapidly than that experienced by the other, and the amount of convergence attached to each unit of accommodation varies accordingly —at least in near vision uncomplicated by any diverging effort. In testing the *true* initial convergence of a hypermetrope, the compensating lenses worn must have their optical centres separated by the exact intercentral distance of the observer, to avoid their prismatic fallacy—even then an error exists, proportionate to the strength of the lenses and the degree of initial convergence, for which a subsequent formula gives the correction. I prefer, to avoid this, to place a single lens of 9½ inches greater focal length before the central aperture, or else before the slit made by largely withdrawing the left brass slide, the end of which can then be placed apparently under the central aperture.

The prismatic action of such a lens introduces no error at all. The farther a lens is from the eyes the more it relieves accommodation, because its focus is brought by so much the nearer to the retina; hence a weak lens at a distance has the same effect as a more proximate and stronger one. For this reason old people sometimes prefer to wear their spectacles on the tip of the nose.

Since spectacles are almost never worn nearer than half an inch from the dioptric centre their power does not exactly express the relief they give to the accommodation—half an inch at least must be substracted from their focal length to do this. Hypermetropia therefore is always slightly greater than the trial lenses indicate, and, conversely, myopia is always less. But this is a digression—it is to illustrate why a lens at the base of the camera must be weaker than one before the eyes. I have as yet by such lenses only been able to *reduce* my initial convergence, and have never obtained complete parallelism of the axes.

Point of greatest relative Convergence.—In glancing with one eye at objects intermediate between practical infinity and the point of coincidence I had thought that the amount of relative convergence diminished uniformily with each approach of the point of view, until at the point of coincidence it gave way to commencing relative divergence. But Dr J. S. Bolton discovered that in his own case the relative convergence at first *increased* as vision became less remote, reaching its maximum when an object is viewed at 5 metres; within which it gradually diminished up to the point of coincidence. His "initial convergence" was 1° 10′; relative convergence *increased*, with accommodation for 8 metres, to 1° 44′; for 6 m. to 2°; and for 5 m. to 2° 12′ 36″. It was here therefore nearly twice as great as the initial convergence. At 4 m. it fell again to 2°; at 2 m. to 1° 42′, so it was still greater than the initial convergence; at 1 m. to 1°; then came the point of coincidence at the distance of ·85 m. (34 in.).

At half a metre (20 in.) there was relative *divergence* of 1° 24′; and at the distance of the apertures of the camera (·25 m.) the divergence reached 6°. On testing another subject he found "the point of greatest relative convergence" to be 6 m. from the eyes. In this case the increase was not so marked, the "initial" convergence being 1° 5′ 24″, while at 6 m. the record gives only 1° 45′ 36″. At 2 m. the relative convergence was still greater than the initial, being 1° 18′. The point of coincidence was at 1 m.; and the relative divergence, though being 2° 15′ 36″ at half a metre, was at the distance of the camera 6°. In these two cases the conditions with vision for 10 inches were therefore the same, but the smaller initial convergence in the latter seemed to throw the two points of "coincidence" and of "maximum relative convergence" to a slightly greater distance.

The results in my own case vary too much to be of any value in regard to this phenomenon, the relative convergence at 8 m. being sometimes more and sometimes less than the initial convergence; but since the refraction is abnormal, this is no guide to the physiological conditions.

To summarise: If we look at accommodation and the convergence attached to it as two racers beginning from infinity, convergence has the first start, and gains still more up to the point mentioned by Dr Bolton; it then slowly falls behind, till at the point of coincidence the two are abreast; it then continues to fall behind, and thus creates the relative divergence of near vision; but just at the near point—the end of the race—it again gains ground, and comes in abreast or even in front of accommodation.

The Point of Repose.—It has been known for some time that the visual axes are not parallel when the eyes are completely at rest, *i.e.*, without being engaged in the vision of any special object, or affected by any act of volition;[1] but it is difficult to determine at what point the axes meet. It is not the point of initial convergence, for that is attached to negative accommodation. And it is well known that the ciliary muscle can by very few, if by any, be completely relaxed without the aid of a distant object to excite the desire for distinct vision; so that in a state of rest there is some positive accommodation, which cannot exist without being accompanied by a due proportion of attached convergence of the visual axes in addition to the initial convergence.

The natural position of eyes in the dark I have tried to determine by the following experiment.

Exp. 27.—Use the central and left lateral apertures of the camera, with the stop to the left; but in looking into it cover the apertures outside with a black pad, so as to produce the impression that the eyes are looking into absolute darkness. A momentary displacement of the pad (a penwiper does well) reveals the two apertures and covers them again before there is any attempt to accommodate for them. The left slide can be moved by repeated trials till, on moving the pad, the images appear in the same vertical line. The real distance between them then, of course, records the separation of the visual lines at the distance of the base of the camera. The records vary in my own case from 5° to 8° 30,[1] being generally only slightly

[1] Mr Brudenell Carter has expressed his belief that "in states of rest the internal recti possess the physiological preponderance over the externi which

different from the disposition of the axes when one eye is occupied in vision at 10 inches. This is somewhat unaccountable, since the radiate appearance of the apertures shows that accommodation does not occur for them.[1] The average of observations taken on nine different occasions was 6° 26′ 16″, while that of the relative divergence at 10 inches on the same occasions was 6° 49′ 36″, so that the visual axes would meet at the distances of 18½ and 19½ inches from the eyes respectively. Many of the observations were taken in the morning before the eyes were otherwise opened; but though the point was at that time rather more remote, the difference was not greater than that of the natural relative divergence in near vision at the same period. It is difficult to believe that so near a point of meeting of the visual axes should be the real position of rest. It may be that a psychical effect is produced by looking into blank darkness, since near objects are the ones that would be instinctively looked for in such a state. Still, on closing the eyes and then opening them suddenly, the apparent position of the apertures only confirms the results. When the light was so reduced that the mind could only detect the dimmest indications of apertures, they appeared together when really separated by 10°, so that the visual axes did not meet until nearly 34 inches from the eyes. On looking at a dull surface without attempt to accommodate, the results were intermediate between those of the last two methods (8° 15′). The position taken by the eyes of a patient under chloroform is not a very trustworthy indication of the position of rest; the effect on the pupils points to probable disturbance of other ocular centres. That the external rectus is partly supplied by the sympathetic is an interesting fact, as showing to that extent an antagonism between the sympathetic and the 3rd nerve in the ocular movements we are now considering, analogous to their antagonism in the movements of the pupil; chloroform would be likely to affect them in a similar way. In one case I noticed marked external strabismus under chloroform, which continued no longer than the anæsthesia.[2] The eyes made constant tiny oscillations from side to side, and the margin of the cornea was ¼ inch above that of the lower lid. The pupil at first widely dilated, but shortly after the disappearance of the conjunctival reflex it began

flexor muscles commonly possess over extensors, and the eyes during sleep or anæsthesia are just a little convergent, in the same way as the limbs are slightly flexed." "This preponderance," he says, "is probably increased by the fact that in the conditions of civilised life the externi are less used than the interni

[1] For these experiments, as indeed for all, a piece of black silk depends from the lower margin of each wooden slide at the narrow end of the camera, to exclude vision of other objects.

[2] To give full attention to the respiration, and yet have one hand free for the eyes, it is useful in administering chloroform to women to hold the left end of the towel between the thumb and the forefinger of the left hand, so that the tips of the middle and ring fingers rest continuously against the clavicle, and appreciate, the former the rolling movement of its anterior surface against the side of the last phalanx, and the latter its upward movement against the tip. The right hand is then free, when not adjusting the towel, to open both eyelids by its first and little fingers.

gradually to contract, taking a long time to get to the size of a pin's head. On the removal of the chloroform it remained thus for some time, then suddenly dilated nearly to its full, this preceding the return of the conjunctival reflex. On reapplying the chloroform it slowly began to contract again, taking as before a long time to reach its minimum size. This was its uniform behaviour during the two hours' administration for an intra-peritoneal operation. The strabismus may have been reflex from the abdomen.

The intercentral distance.—There are many methods of finding the distance between the centres of motion of the two eyes. (*a*) The simplest and best is to look at a linear vertical object at a great distance through the camera, moving one of the slides till the object appears to bisect the central and one of the lateral apertures, as attention is directed to each in turn. The *distance* between the apertures gives the intercentral distance;[1] while the number of *degrees* recorded between them expresses the exact magnitude of the "optic angle" (*i.e.*, the angle included between the visual axes) of the person tested when he looks with both eyes at the central aperture; since it is through the "alternate" angle that the one visual axis must rotate out from that point in order to become parallel with the other (Euclid, I. 29). This angular record is the one most convenient to preserve, since to obtain the "initial convergence" at any time it is only necessary to deduct from it the record given when the simple observation is taken of looking at a distant object through one aperture and placing the other apparently beneath it. To convert the angular dimensions into linear ones, it suffices to know the linear equivalent of each degree. It is ·1789 inch, with vision for 10 inches. From this a table may be made.

Table of the Linear Value of Degrees at the Base of the Camera.

	Inches.	mm.		Inches.	mm.		Inches.	mm.
12°	2·1468	54·468	13° 30′	2·4151	61·276	14° 50′	2·6537	67·329
12° 10′	2·1766	55·224	13° 40′	2·4450	62·033	15°	2·6835	68·085
12° 20′	2·2064	55·981	14°	2·4748	62·789	15° 10′	2·7133	68·841
12° 30′	2·2362	56·737	14°	2·5046	63·546	15° 20′	2·7431	69·598
12° 40′	2·2661	57·494	14° 10′	2·5344	64·303	15° 30′	2·7729	70·353
12° 50′	2·2959	58·25	14° 20′	2·5642	65·059	15° 40′	2·8027	71·11
13°	2·3257	59·007	14° 30′	2·5940	65·816	15° 50′	2·8326	71·867
13° 10′	2·3555	59·763	14° 40′	2·6239	66·572	16°	2·8624	72·623
13° 20′	2·3853	60·52						

[1] The measurement being partly on a curve, there is a slight error of ·004 inch. It may be avoided where great accuracy is required by using the left aperture 7° away from the middle line instead of the central one.

From this table, if either the intercentral distance or the optic angle in binocular vision for 10 inches be known, the other may be found. The radius is 10·25 inches, since the centre of motion is behind the optical centre.

(*b*) It is not always easy to find a suitable linear object at sufficient distance. If the camera is mounted on a stand it can, after adjusting the central aperture for any distant object, receive a slight tilt to bring the other aperture to the same level. The tilt would always be through the same angle, and can with my own stand be mechanically provided for when this method is employed.

(*c*) In *practice*, however, the object is far more convenient within the room. To open a window is not advisable always, and to look through it may introduce some prismatic error from the panes. Any linear object may be viewed—if at 17 ft. 1 inch from the *base* of the camera the distance between the apertures must be increased by $\frac{1}{20}$th, if at half that distance by $\frac{1}{10}$th. I now use a card marked with a vertical strip of white, $\frac{1}{4}$ inch wide, down the middle, to the right of which is all coloured red, and to the left green. After adjusting the camera so that the white strip is visible in the central aperture with an equal band of colour on each side, it is easy to know whether to push the lateral aperture in or out by whether the red or green colour is that seen through it. This expedient greatly compensates for want of intelligence in the patient. The one source of fallacy in all methods with a near object springs from the varying distance in different patients between the corneæ and the apparatus; but the greater the distance of the object the less the error. The card can either be connected to the base of the camera by a string of the required length, or it can be placed on the strip of wood already mentioned at the distance of 41 inches from the eyes; the latter is now my usual practice, being much the handiest and quickest indoor method. The record must be increased by adding a third as much again. An object nearer than this is not advisable. In testing for the relative convergence or divergence, when accommodation is exerted for objects nearer than practical infinity, it is useful to know how many marked degrees at the base of the box would be intercepted between the visual axes, were they both directed accurately at the object viewed. To find the amount of deviation it is then only necessary to deduct from this the degrees recorded when, in looking at the object through one aperture, the other is placed apparently beneath it. The separation of the visual lines at the distance of the base of the camera in binocular vision of any object is given by the formula $x = \frac{i \times d}{d - 10{\cdot}25}$, where x is the required number of intercepted degrees, i is the intercentral distance, and d the distance in inches of the object from the eyes.

VI. *Direct Oblique Vision.*

So far we have considered the relation between convergence and accommodation apart from any influence which may be exerted upon either by one of those innervations which turns both eyes to the right or left. Whenever the point at which the visual axes meet lies to one or other side of the median plane, a new innervation comes into play. Does the intervention of this new effort in any way disturb or alter the relationship we have been considering? We can only judge of efforts by their results, and these results are in oblique vision complicated by the fact that accommodation and convergence are required in differing proportions when an object is viewed with different degrees of obliquity. This, indeed, is in itself ample reason why the two are not so inflexibly bound together as they were once thought to be, and as the two accommodating mechanisms actually are. Complete central connection might at first sight appear the simplest conception. But in daily life the eyes need constantly to wander from side to side, and as they do so, accommodation and convergence are required in as constantly differing proportions, which make a certain "play" between the two innervations a necessity, and for which provision is made by superadded functions which can reflexly increase or diminish either as required. But might not these have been done without, by such an adaptation of the central connections that each ranging effort would in a direct way produce the required modification in the others? No, for up to a certain geometrical degree of obliquity accommodation needs to be relatively increased, while beyond it convergence needs to be increased. Why, then, if to attain this play central connection must in part be overcome, is there any such connection at all? It is a great saving of the reflex fusion-effort.

It is evident that geometrical considerations must precede the physiological study of the subject of oblique vision.

(*a*) *Accommodation.*—Apart from any connection with convergence, disproportion between the accommodative requirements of the two eyes is brought about by the slightest deviation of the point of fixation from the median plane. Fig. 11 illustrates this when any flat object is looked at, as in reading a book. The prolongations of the visual lines on the distal side of the line AB represent the disproportion.

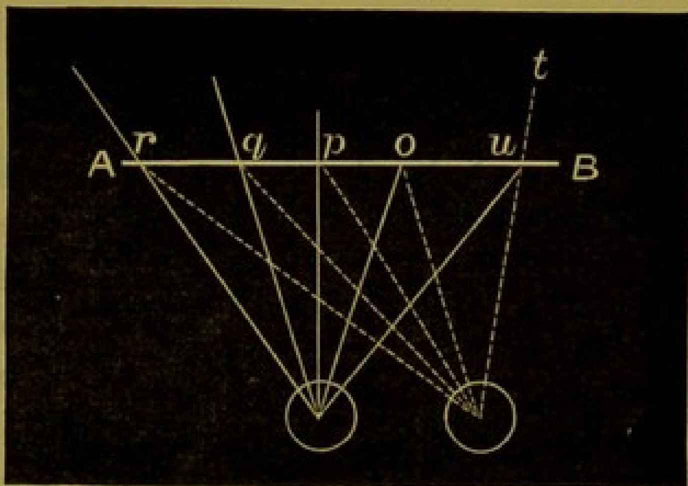


Fig. 11.

Thus, when both eyes are looking at "*u*," the object is nearer to the right eye than to the left by the distance *tu*, and so on. Every departure of the point of fixation from the middle line lessens the required accommodation in the opposite eye, while at first it increases that of the eye of the same side till the fixation point has traversed a distance *op* equal to half the intercentral distance, after which it falls through a similar interval *pq* to the original amount, and then continuously diminishes. But the centres for accommodation are so intimately connected that one eye cannot accommodate more than the other. When variations, therefore, exist either in the refractive power or requirements of the two eyes, "that eye has the bright image which attains it most easily at the expense of the other" (Donders). They do not split the difference; if they did so there would be diffusion circles in each eye. Since accommodation with normal refraction implies positive effort, that eye which is *farthest* from the object, and can see it with least effort, determines the accommodation for *both*. Fig. 12 therefore represents "the line of equal accommodation" for near vision, in whatever point of which the object is placed accommodation remains the same. It is composed of two arcs of equal radius, described from the centres of their opposite eyes. In hypermetropia the line is similar; the reasons for it being so are intensified, since accommodation is a greater effort; but in myopia accommodation is negative outside the line which limits the far point, and which is made by drawing each arc from the centre of the eye of the *same side*. Since in these cases relaxation is often attended with more effort than accommodation (owing to spasm of the ciliary muscle), the "line of equal convergence" would be of the same shape as the "far point line" for some distance within it.[1]

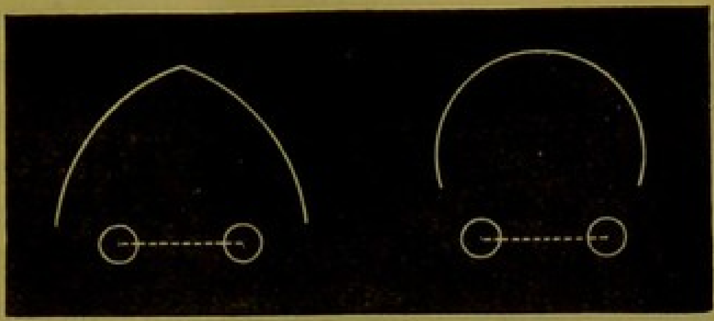


Fig. 12. Fig. 13.

(*b*) *Convergence.*—Fig. 11 shows that convergence, as well as accommodation, diminishes with oblique vision; but that they do not diminish equally is made clear by the fact that the line of equal convergence (fig. 12) is not of the same shape as that of equal accommodation (fig. 13).[2] The former is part of a circle which passes through the centres of motion of the two eyes (not like the horopter through the dioptric centre), and possesses these properties. (1) The angle of convergence is the same whatever point in it is made the point of binocular fixation. (2) In glancing from any one point in it to any other, both visual axes traverse equal angles; thus in fig. 14 the angles OB*c* and OA*c* are equal; while, in contrast to this, fig. 11 shows that in glancing from O to *q* or O to *p*, the left axis passes through a greater angle than the right; as it does, indeed, whatever point is looked at to the left in the straight line AB. (3) The line

[1] While what has been said holds good in normal vision, since attention is seldom directed to one side for long at a time, a fallacy must be guarded against in experiments which require *sustained* obliquity of the visual axes; the visual apparatus of the farthest eye might weary, and for a time permit the accommodation of both eyes to be governed by that of the nearest one, and thus be increased.

[2] These were called in the original thesis the "Isostigmal" and "Isogonal" lines respectively, but I do not see the necessity of naming them.

which bisects the angle of convergence is the one to which hypothetically objects upon the maculæ should be mentally referred. Whether, in fact, they are so physiologically remains to be proved. The line is obtained by uniting the point of binocular fixation (*c*, in fig. 14) to the posterior point of the circle (*b*). The position of this posterior point shifts, of course, with every variation in the size of the circle; the line *cb* itself is inclined to the median plane by an angle which measures the obliquity of vision, since it is equal to the angle which *each* visual axis has traversed in looking from the anterior point of the circle (*o*) to any other point in it (*c*). This angle, therefore, represents the "ranging" work to be done in looking at any such point. I do not say the ranging *effort*, for, as before mentioned, effort is often disproportionate to work, owing to greater resistance or other disadvantages. A certain evolution of nervous energy from the converging centre produces a definite angular deflection inwards of both visual axes, and similarly an effort of a ranging centre produces a definite deviation of both visual axes to the right or left. The nervous impulses perform angular work, if I may so say, as far as vision is concerned, and therefore we may assume that it is by angles that the mind judges of the work done in estimating the projection of the field of vision; but since the judgment is based solely upon the *effort* put forth, any discrepancy between effort and work would show itself in angular misjudgment, unless by habit the mind had come to associate a certain degree of effort with the work it *usually* performs, instead of with the work it *should* perform compared with a smaller effort. Such allowance is no doubt made, in whole or in part, except for unusual obliquities. Were "effort" and "work" exactly proportionate in both the converging and ranging innervations, all objects seen by the fovea of one eye or

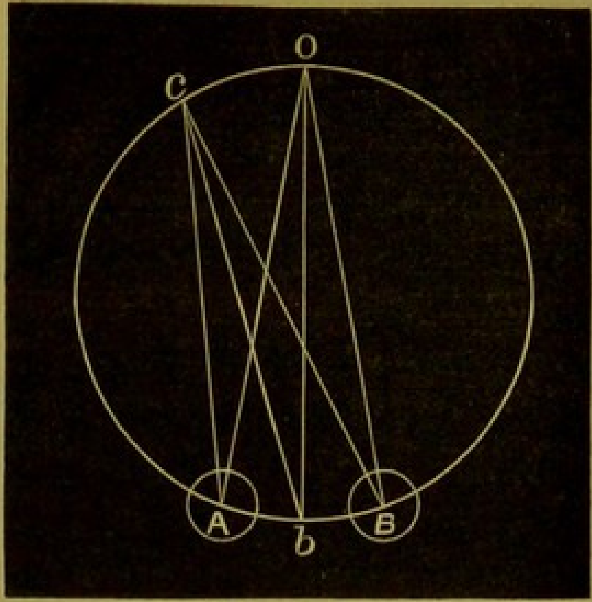


Fig. 14.

both, however obliquely, would be referred to the line which bisects the angle of convergence, since its inclination to the median plane would exactly express the angular impression in the mind produced by the ranging effort. I find, however, that if a large piece of card be held obliquely a few inches outside and in front of either eye, with a mark upon its upper border, looked at with both eyes, a finger passed up behind it generally at first misses the mark to the *outside* by nearly an inch, showing that the ranging effort is relatively so far greater than the work it accomplishes that the mind estimates as if more angular work were done, and mentally displaces the object from the median plane by an angle greater than that of the line which bisects the angle of convergence, and which only measures the work actually accomplished, not the effort put forth to accomplish it.

In fig. 15 the dotted arcs represent the line of equal accommodation, so applied to that of equal convergence as to illustrate the fact already mentioned, that within a certain degree of obliquity of vision the proportion of convergence to accommodation is greater than in the median plane, while for greater obliquity the proportion is less. At the points *dd*, where the two lines intersect, the proportion between convergence and accommodation is the same as at O; within these points convergence must be relatively increased; outside of them relatively lessened. The distance of each *d* from O is always exactly equal to the intercentral distance of the observer.

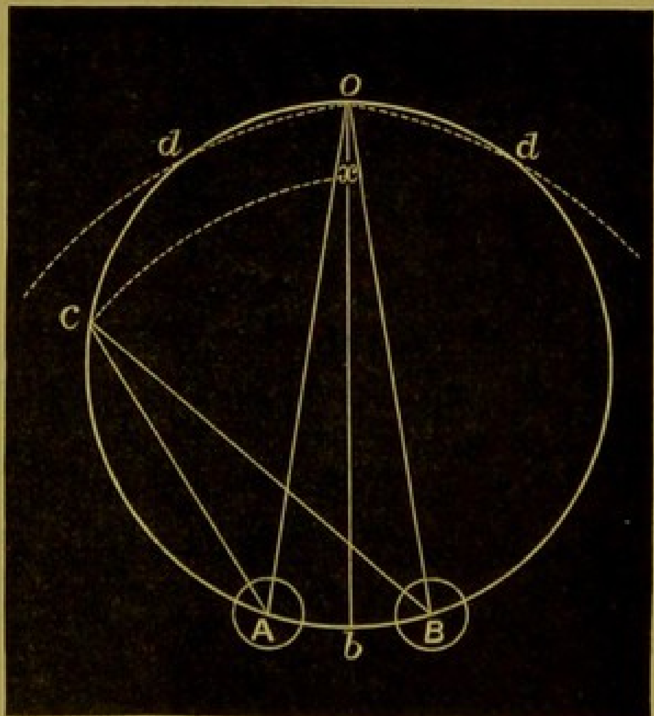


Fig. 15.

The same figure shows how, in looking obliquely at any point, the convergence and accommodation required may be compared with that in the median plane. All that is necessary for any given point (*c*) is to describe a circle through it and through the centre of each eye,[1] as in the figure, and from the centre of the farthest eye (B) to draw the arc *cx* from *c* to the median line. The circle is the line of equal convergence for that point, and the arc is part of the line of equal accommodation for the same. In binocular vision, therefore, of the point *c*, convergence must occur as if for O, and accommodation as if for *x*, while both eyes are deviated to the right through the equal angles O*Bc*, *o*A*c*. The point *b* gives the base of the line which bisects the angle of convergence, and which is made by joining *bc*, while the inclination of this line to the median plane expresses the angular ranging movement of the two eyes. Were the relation between convergence and accommodation inflexibly complete for objects in the middle line, there would be diplopia for any object out of the middle line, except at one point on each side, within which diplopia would be heteronymous from relative divergence, and without which it would be homonymous from relative convergence. The nervous ties, therefore, though strong enough to relieve fusion-effort, are not so strong but what they can be overcome to meet such requirements.

The lines of equal convergence and accommodation, if rotated round the intercentral line as an axis, would describe *surfaces* of equal convergence and accommodation respectively.[2]

[1] This may be done by drawing from A and B straight lines at right angles to *c*A, *c*B respectively. From the point where they meet draw a straight line to *c*, the bisection of which gives the centre of the required circle. Or *Euclid*, iv. 5.

[2] (1) To find the line of equal convergence for any required angle of convergence—the *radius* of the required circle is found by dividing half the intercentral distance by the sine of the angle. (It may be found *geometrically* by *Euclid*, iii. 33.) (2) To find the angle of convergence when an object is viewed in the median plane at a given distance from the eyes—half the intercentral distance, divided by the distance of the object from the eyes, gives the sine of half the angle.

(*To be continued.*)

20 (3)

INVESTIGATIONS IN THE RELATION BETWEEN CONVERGENCE AND ACCOMMODATION OF THE EYES. By ERNEST E. MADDOX, M.B., C.M. Edin., *Syme Surgical Fellow in the University of Edinburgh.*

(*Concluded from vol.* xx. *p.* 584.)

VII. *Direct Oblique Vision* (*Physiologically considered.*)

THE simplest conception of a connection between convergence and accommodation would be a complete connection, incapable of modification, like that between the accommodative acts of the two eyes. Why is it not so? It is true that spectacles (unless orthoscopic) could not be worn, but this does not account for it. I think the answer for the physiological eye is to be found in the daily requirements of direct oblique vision.[1] If this is *enough* to account for the partial attainable independent action of the two efforts, we should expect to find the independence cease at or within the extreme *limits* of oblique binocular vision. And this is the case, for there is a certain degree of obliquity of vision, in which a limit is found to the independence of the two efforts attainable by the desire for single vision. In other words, there is diplopia when an object, such as, *e.g.*, a pencil, is held in front and to the outer side of either eye, so as to be just visible by the other (farthest) eye in front of the root of the nose.

Thus, if with the right eye closed a pencil is held outside it, so far back as only to be just seen by the left eye, on opening the right eye a false image appears lying to the left of the true one. There is therefore *crossed* diplopia, since the image received upon the *right* retina is mentally projected to the *left* hand side of the mental projection of the other. Now, if each image were lying upon the macula, each would be projected to the same spot. To be projected as they are to different positions, if one falls *on* the macula, the other must fall to the outer side of the macula, to do which the visual axes

[1] I do not mean that the requirements *caused* the fusion faculty of attaining the needed dissociation, but that the faculty is adapted to meet the requirements. *Circumstances cannot create* a faculty, however much they may develop or retard its exercise, but we *can* conceive that faculties were created with a view to circumstances, and even capable (within limits) of being modified by them.

must meet at a point *beyond* the object looked at. There is therefore relative divergence. It is true that a strong effort to overcome the diplopia may yet succeed, but it is evident that the object is just about at the limit in which such union is possible. The nature of the diplopia, however, is just the opposite of that which, from geometrical reasons, we might expect to find were the two functions proportionately and rigidly correlated. *Then* the visual axes would meet *nearer* to the eyes than the object. From the fact that they actually meet *beyond* it, and cannot even by the desire for fusion be brought up to it, it is evident that obliquity of vision imposes great (mechanical or physiological) difficulties in the way of convergence. To overcome these difficulties, in order to maintain single binocular vision, a larger expenditure of nervous energy is required from the converging centre, while that for accommodation remains the same.

A certain measure of independent action is thus necessitated, a measure which is exhausted when the degree of obliquity is extreme. To state the matter briefly; the *angular* work to be done by the converging function, when accommodation remains unaltered, varies with the obliquity of vision, being increased with slight obliquity, and with greater diminished; but so great are the resistances and impediments to the performance of this work, and they so much more than nullify its variation, that in great obliquity, though the angular work to be done is less, the effort expended in doing it is greater, till at last the fusion effort becomes unequal to the task, and gives it up. These indications would also lead us to expect obliquity of vision to exert a considerable modifying effect on the relative divergence which occurs, as we have seen, when in near vision the desire for fusion is eliminated.

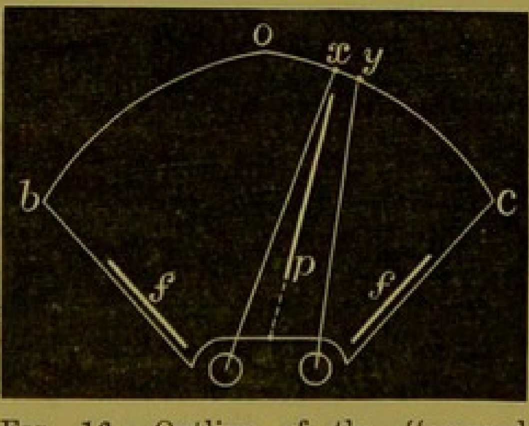


FIG. 16.—Outline of the "crossed camera," *p*, the moveable partition; *x* and *y*, the moveable apertures; *f f*, fans used instead of the central partition whenever there is relative convergence.

To examine more thoroughly the conditions of direct oblique vision, I proposed another form of camera, which has been taken up by Dr J. S. Bolton, to whom I left entirely both its construction and its use. Unlike the first camera, its curves are described about the centres of the opposite eyes, as seen in fig. 16. There are only two luminous apertures, each capable of being moved to any point in the whole curved side, *boc.*[1] The upright partition also differs in that, instead of being fixed, it is movable in a radiate way round a centre very near to the root of the nose, to which it is hinged by a piece of wire, represented by the dotted portion in the figure. This wire, after passing through the roof of the box, runs forward exactly over the partition, so that it serves both as a handle for its movement and a constant index of its position. The curvature of the arcs is the same as in the first box, to distinguish it from which it may be called the "crossed camera," since the radii of the arcs cross each other. Accommodation, therefore, is the same (4D) in both; and since the outline of the "crossed camera" is that of "the line of equal accommodation," the same amount of accommodation is in exercise whatever the obliquity of vision, provided that care be taken to ensure that the object seen by the further eye is always the one to which attention is chiefly directed. The investigator knows, of course, whether it is the upper or lower aperture which is seen by the further eye, and concentrates his vision accordingly.

The relative divergence can now be tested with vision of any required obliquity by placing one of the movable apertures, covered by printed paper, at the distance of the given number of degrees from 0, as marked on the graduated scale. The partition is then placed so that it points slightly to the outside of the aperture, and thus permits, but only just permits, its full view by the further eye. The other aperture is then moved from the end of the curve towards its fellow till they appear to lie in the same vertical line. The observation is now complete, for the real interval between the apertures records the relative divergence, but not truly, until certain mathematical corrections have been made, unless read off a second scale. Leaving these for the present, I will give some of Dr Bolton's *corrected* results of his careful investigation, and first in *near* vision, with accommodation accurately ensured by print for the distance of the base of the camera—10 inches. With the vision of the left eye directed to 0, relative divergence is necessarily the same as with the first camera—in his case 6° But with obliquity of 10° *to the right* it increased to 7° 10′; and, as he has shown, the visual lines, instead of meeting at the distance of 17 inches from the eyes, meet at 21 inches. With obliquity of 20° to the right, R. D. increased to 8° 54′,

[1] The head, when this camera is used, should always be kept square to it, either by adjustable buffers from below impinging on the cheek bones, or by holding between the teeth a horizontal stick of wood fastened to the bottom of the camera.

and the meeting of the axes was prolonged to the distance of 29 inches. At 30° obliquity, R. D. was 10° 45′, and the visual axes met at 140 inches; while at the obliquity of 35° R. D. was as great as 12° 36′, and the visual axes were positively divergent, so that if prolonged backwards they would meet at a point behind the eyes. At the same time, the aperture seen by the right eye no longer keeps steady, but begins to oscillate with great rapidity, and with a regular rhythm, though it is of course the eye which really oscillates. This remarkable phenomenon may be of interest in its bearing upon the etiology of miners' nystagmus. It is of a nature quite distinct from the slow irregular waverings. Indeed, the development of nystagmus in adult coal miners has already been attributed to the necessity which the miner is under of constantly looking in an unnatural direction, upwards or sideways for example (Nettleship's *Diseases of the Eye*, p. 319). The rapid rhythmical oscillation noted by Dr Bolton may perhaps be called physiological nystagmus; it must not be confused with the slow uncertain waverings of a dissociated eye, as already noted by the first camera. I will not here mention his other results and conclusions, but will only point out as a striking and evident summary *the very great diminution of "accommodative convergence" in oblique vision*, so that with great obliquity it makes almost no difference to its fellow whether the fixing eye is adjusted for a great distance or for 10 inches.[1]

The Use of the Camera in Strabismus.

Though the relative divergence or convergence, which occurs on exclusion of one eye, has been aptly termed "latent squint,"

[1] *Mathematical Corrections for the "Crossed Camera."*—Fig. 16 shows that, since each arc is described from the centre of the opposite eye, the graduated scale on each gives a correct record of the position and movements of the eye which forms its centre; but each scale gives a false estimate of the movements of the *near* eye. This necessitates the use of a second scale for each arc, so graduated as to record correctly the movements of the near eye. Suppose, for instance, in testing the relative divergence when the right eye is directed at 30° on the first scale, the left eye is directed to 35°, as shown by the vertical appearance of the apertures when each is at those respective parts of the scale. The *real* relative divergence is not 5°, but 5° 39′, as shown by the second scale. Where the second scale is absent an approximate correction can be made, when the fixing eye looks at 0°, by subtracting 1′ from each degree of relative divergence; when the fixing eye looks at 5° make no correction; at 10°, add 1½′ for each degree of relative divergence; at 15°, add 3′ for each degree; at 20°, 4′; at 25°, 6′; at 30°, 8′; at 35°, 9′; and at 40°, 11′ for each degree of relative divergence. Another consideration is that, *supposing no deviation* occurred, the "optic angle" included between the two visual axes would vary with the degree of obliquity. Thus if, when both axes meet at 0°, the optic angle between them is 14°; at 5°, it would be 30° 48′; at 10°, 13° 29′; at 15°, 30° 4′; at 20°, 12° 33′; at 25°, 11° 56′; at 30°, 11° 14′; at 35°, 10° 27′; at 40°, 9° 34′. To find the total diminution of the angle of convergence this must be taken into account.

it must always be decisively distinguished from the clinical strabismus. In the former the natural tendency to deviation is constantly overcome in actual vision by the desire for fusion: in the latter the fusion effort is *unable* to correct the whole of the deviation. There are no doubt intermediate and transition stages and cases, in which the fusion effort is strong enough to correct the deviation *only for a time,* or only when attention is strongly directed to a definite object, or when vision is not very near. So called "periodical" squints are of this class. Even in true persistent strabismus it is possible that, so long as the squint is not great or of long standing, the desire for fusion *lessens* the squint, though it cannot quite overcome it. This may be illustrated by placing a prism before one eye slightly too strong to be overcome, and thus creating an artificial squint; the images draw together with a swaying motion, though they cannot unite; this is most marked, I think, when a prism of low power is used with its base up or down.

It is just in commencing squints, and obscure paresis of small degree and almost doubtful diagnosis, that a true and delicate method of strabismometry is needed. But to distinguish manifest squint from latent squint, the practical proposition must be accepted, that while the latter is measured with the eyes dissociated, true strabismus is that which continues to exist *in the presence of ordinary binocular vision,* or at least with the full opportunity for it.

Thus, if a pencil be held 18 inches before the eyes of an apparently squinting patient, and then a card or piece of ground-glass be placed in front of the apparently sound or "working" eye, any change of position observed in the other suspected eye is a conclusive indication of *manifest* squint, since the movement proves that there was deviation existing while vision was yet *binocular*. If this movement, however, be not detected, no conclusion can be drawn from the position taken by the covered eye; for *latent* squint begins at once to set in, and might be mistaken for real squint (called the "secondary deviation") did it not require such quickness of observation to detect such small angular departures that they are generally overlooked. Since on exclusion of one eye relative divergence is most common in near vision, giving way beyond the "point of coincidence" to relative convergence, the effect of this would be to lessen convergent squints in near vision, and to increase divergent ones; but it is just in those cases of hypermetropia which develop into squint that the central connection between accommodation and convergence is unusually complete, so that the normal conditions would not apply. We have seen, too, that the relative divergence of near vision (when it occurs) again dwindles to a second point of coincidence just at the near point; so that a hypermetropic squint might diminish with moderate approach of the object, and yet increase again at the near point.

There is a variable nervous element in many squints which repeated exact measurements would be able to estimate the amount of.

Would the simple existence of latent squint indicate any danger of the supervention of actual squint? Not in itself, unless its extent be such that the fusion effort can only just overcome it in prolonged work. But I doubt not that *age* and the *character* of the deviation would greatly affect the answer to this question. Relative convergence, for instance, in a young hypermetropic child, would, if at all considerable, strongly point to subsequent development of squint, while relative divergence would be a certain remover of any fear on the point. It is difficult to test very young children, especially if their confidence is not gained, but for intelligent ones the camera by the direct method is very easy of employment. Relative divergence in near vision of adults other than myopes need, I believe, give no fear of squint, for the desire for fusion is so strong that, rather than give up single vision, the fusion effort continues to correct the tendency, till the neuro-muscular converging apparatus is thoroughly weary and the person is compelled to give up the close work; besides in these cases vision is single, without effort, at greater distances. Any existence of latent squint beyond the "manifest" will affect the prognosis of the squint's progress, if left untreated, since any diminution of the vigour of the fusion effort from mental abstraction from the false images will convert the latent portion into manifest.

After operations for squint, when the divided structures have healed, a knowledge gained by the camera of the exact deviation of a dissociated eye will give valuable information as to the likelihood or possibility of the squint's return. The effect of the cicatrising process day by day can also be studied by simply using the direct method, as on p. 401.

Since the operation is generally performed so as to leave slight divergence, how soon the contraction begins to lessen this divergence, how rapidly it proceeds, when it reaches its maximum, and what is its entire ultimate effect on the angular position of the eye, are questions it will thus be easy to answer, and which afford a more delicate index to the phenomena of cicatricial contraction than any surgical operation elsewhere.

Exophthalmic goitre, too, must surely affect the converging function, though I do not know in what way, or whether the effect of remedial measures could be measured by its variations.

Detection of Squint.

The use of the camera by the direct method is advantageous for the detection and measurement of squints where any subjective method is available. The retina of the eye least used (in some cases never used) becomes so dull in perception that ordinary objects make no distinct mental impression while the other eye is being employed. A point of light is clearly the best object, since it not only makes a stronger impression on the retina, but favours the concentration of attention, by the surrounding darkness and the absence of other objects.

Let the patient look through the camera, holding it up to the light, with the stop in the middle, both movable apertures out of sight, and type placed over the central aperture. If the double images of the central aperture persistently refuse to unite, squint may safely be diagnosed. If pushing the stop to the right obstructs the right hand image, the squint is convergent; but if the left hand image, the squint is divergent.[1] In either case it is "manifest" squint, since the desire for fusion is not abrogated, and the double images are such that they would readily unite if there were no strabismus. A glance at Exp. 25 of the "central method," on page 505 of the last number of the *Journal*, shows that, when *attention* is purposely diverted from the double images of the central aperture and concentrated elsewhere, diplopia can be made to continue even with physiological eyes. This shows the importance of fixing the patient's *attention* on the images of the central aperture, and of having no other object in the whole field of vision. Indeed, the aperture should be made larger than for physiological purposes, and accommodation accurately ensured for it by means of print, which the patient must be made to read.

Measurement of Divergent Strabismus.—After detecting divergent squint, it is now measured in a moment or two by

[1] Another method is to draw out one lateral aperture into view; if it appears between the other two, the squint is divergent; if not, convergent.

simply drawing out one of the lateral apertures till it lies just below one of the double images. The actual position of the aperture so placed gives by the graduated scale an automatic angular record of the degree of strabismus.

Measurement of the Latent Element.—Now push the stop to the same side as that of the aperture just mentioned. If the superimposed apertures maintain their position there is no latent squint, the whole is manifest; but if the higher image (that of the central aperture) move still outward, the squint has both a manifest and a latent element, the latter of which is measured by further drawing out the movable aperture till it lies again beneath the other.

By pushing the stop back to the middle and then to the other side, and using the other lateral aperture, the measurement of the manifest and latent elements can be repeated; for the squint may be quite different when one lateral aperture is used to what it is when the other is used. The difference may be due to different refraction in the eyes; but if this is not found, a paralytic element is present, and the difference is due to the altered obliquity of vision.[1]

Is the Squint alternating or monocular.—If there is any doubt on this point it may be tested as follows:—

Place the stop to the right, and the right lateral aperture in the right blind area. Now push the stop to the middle. If the hidden point spring into view when the stop is pushed, the squint is monocular, and the right eye is that used as the "working" eye by preference. If, however, the hidden point *remains* hidden, the test must be repeated on the other side by starting with the stop to the left, and the left aperture in the left blind area. The right eye is now of course fixing the object. Again push the stop to the middle. If the hidden point appear, it shows that the left eye has replaced the right, so that the squint is still monocular, but the left eye has the "working" preference. If on *each* side the hidden point *remains* hidden, the squint is alternating. If on either side the hidden point appears, the eye on that side is the "working eye." This procedure is in the same words equally true whether the squint be converging or diverging.

Measurement of Converging Squints.—These may be measured in one of three ways.

[1] Dr Joseph Bolton has shown by the crossed camera that, *physiologically*, relative divergence is *slightly* greater with right-handed than with left-handed obliquity of vision. This must be taken into account.

1. By the blind-spot method, which is the most difficult, and inapplicable for squints greater than 18°.

With the stop to the left one of the borders of the right blind area is noted, and again with the stop to the right. The difference between the records gives the measurement of the squint, though with monocular vision, so that the *total* squint is estimated. If the stop is kept in the middle, an intelligent patient can be told to fix his attention first on one of the double images of the central aperture and then on the other, the position of the blind spot being tested each time. The difference between the records gives as before the measurement of the squint, but now only of its "manifest" element, since it is tested in the presence of natural binocular vision.

2. A more accurate method, available for the largest squints, is afforded by the crossed camera, in which the central fan is replaced by two lateral ones (*f, f,* in fig. 16), each capable of rotation round a pivot at the ocular end.

When the two points of light are placed anywhere in the curved end, since each eye receives two images, there are four images visible. By moving the slides till the two innermost of the four images become superimposed the manifest squint is measured. By then cutting off the two outermost images by the fans the eyes are dissociated, and the total squint is measured by the superimposed apertures. The difference gives the "latent" squint. If the two apertures are equidistant from 0°, the squint is measured without obliquity, and therefore, if it be a "concomitant" one, without ranging effort. By moving the apertures any number of degrees to the right or left the effect of obliquity on the squint may be noted, always remembering that, if the apertures are on opposite sides of 0°, their distances from that point must be added.

In strabismus from paralysis or paresis of a single muscle the diplopia is always greater when vision is directed towards the side of the affected muscle. The simplest tests are generally, I suppose, sufficient to determine this, but in some slight and obscure paresis a simple and accurate method of analysis may be useful.

3. A third method might seem unnecessary, but since the last involves the use of an additional camera, it is well to say that squints can be measured by the direct method of the first camera, if the small wooden slides at the narrow end are for a time replaced by rectangular prisms of the same outline, each with its apex towards the nose.

The effect of these is, as it were, to give each visual axis a bend outwards.

The way to proceed is first to push the stop to the right, then to place the right lateral aperture apparently underneath the central one. Its record, deducted from the known effect of the prisms on convergence, gives the angle of the squint. This however measures the squint with a slight obliquity of vision to the right, a degree of obliquity expressed by half the angle recorded by the lateral aperture. The effect of similar obliquity to the left may be noted next by pushing the stop to the left and placing the left lateral aperture apparently under the central one; it will thus be seen whether the squint is greater in right-handed or left-handed vision. If now the central aperture be covered, and the left lateral aperture be moved half-way from its former position towards it, then, on placing the right lateral aperture apparently under the left one, and making each equidistant from 0°, the squint is measured without obliquity of vision at all.

This prismatic method is much the easiest and quickest, but the use of prisms for almost any clinical purpose is fraught with fallacies. In this case, their rectangular shape prevents the error which results from accidental rotation of a circular prism, but still the error remains connected with the fact that the deflecting power of a prism varies with different angles of incidence.[1] This would not interfere with comparative tests however. (See further, note 6, p. 42.)

The Effect of Spectacles on Convergence.

Spectacles are sometimes worn by myopes and presbyopes in using the camera. Their effect on accommodation is of course practically constant and easily discoverable, since it is nearly[2] expressed by the focal strength of the lenses. But their effect on convergence depends entirely on the proportion which exists between the separation[3] of the visual lines in passing through the glasses, and the separation of the optical centres.

The optical centre is a point in every spherical lens, in traversing which a ray of light emerges parallel to its previous course.

In testing, for instance, a myope whose near point lies at a less distance than the 10 inches of the camera, the direct method simply records the "initial convergence" if there be any present, when the record is deducted from the intercentral distance. To test the effect

[1] The prisms supplied with the camera should be accompanied with a notification of their effect on convergence. This, of course, depends on their "deflecting angle," and not on their "refracting angle" which is nearly double; the latter is what is usually marked on prisms. Their effect on convergence is about $\frac{9}{10}$ths of the sum of their deflecting angles.

[2] I say *nearly*, because, owing to their distance from the eye, convex lenses relieve A by *more* than their own strength, and concave ones affect A by *less* than their own strength.

[3] By "*separation*" I mean *the mutual distance between them.*

TABLE III.—*Table of the Deflection of a Ray of Light by Convex or Concave Lenses at different Distances from their Optical Centres.*

	1 mm.	2 mm.	3 mm.	4 mm.	5 mm.	6 mm.	7 mm.	8 mm.	9 mm.	10 mm.	11 mm.	12 mm.	13 mm.	14 mm.	15 mm.	16 mm.	18 mm.	20 mm.	22 mm.	24 mm.	26 mm.	28 mm.	30 mm.	32 mm.
·5D	1′ 43″	3′ 26″	5′	7′	8′ 25″	10′	12′	14′	15′ 28″	17′	19′	20′ 37″	22′ 27″	24′	26′	27′ 30″	31′	35′	38′	41′	45′	48′	52′	55′
·75D	2′ 30″	5′	8′	10′	13′	15′ 28″	18	20′ 37″	23′	26′	28′	31′	33′	36′	38′	41′	46′	52′	57′	1° 2′	1° 6′	1° 12′	1° 17′	1° 22′
1D	3′ 26″	7′,	10′	14′	17′	20′ 37″	24′	27′ 30°	31′	35′	38′	41′	45′	48′	52′	55′	1° 2′	1° 9′	1° 16′	1° 23′	1° 29′	1° 36′	1° 43′	1° 50′
1·5D	5′	10′	15′ 28″	20° 37″	26′	31′	36′	41′	46′	52′	57′	1° 2	1° 6′	1° 12′	1° 17′	1° 22′	1° 32′	1° 43′	1° 53′	2° 3′	2° 13′	2° 24′	2° 34′	2° 44′
2D	7′	14′	20′ 37″	27′ 30″	35′	41′	48′	55′	1° 2′	1° 9′	1° 16′	1° 23′	1° 29′	1° 36′	1° 43′	1° 50′	2° 4′	2° 18′	2° 32′	2° 46′	2° 58′	3° 12′	3° 26′	3° 40′
3D	10′	15′ 28″	31′	41′	52′	1° 2′	1° 12′	1° 22′	1° 32′	1° 43′	1° 53′	2° 3′	2° 13′	2° 24′	2° 34′	2° 44′	3° 6′	3° 26′	3° 46′	4° 7′	4° 27′	4° 48′	5° 9′	5° 29′
4D	14′	27′ 30″	41′	55′	1° 10′	1° 22′	1° 36′	1° 50′	2° 4′	2° 18′	3° 32′	2° 46′	2° 58′	3° 12′	3° 26′	3° 40′	4° 8′	4° 35′	5° 2′	5° 29′	5° 56′	6° 24′	6° 50′	7° 18′
5D	17′	35′	52′	1° 9′	1° 26′	1° 43′	2°	2° 18′	2° 35′	2° 52′	3° 9′	3° 26′	3° 43′	4° 1′	4° 17′	4° 35′	5° 9′	5° 44′	6° 17′	6° 51′	7° 24′	7° 58′	8° 32′	9° 6′
6D	20′ 37″	41′	1° 2′	1° 23′	14° 3′	2° 4′	2° 24′	2° 45′	3° 5′	3° 26′	3° 46′	4° 7′	4° 27′	4° 48′	5° 9,	5° 29′	6° 10′	6° 51′	7° 31′	8° 12′	8° 52′	9° 32′	10° 12′	10° 52′
7D	24′	48′	1° 12′	1° 36′	2°	2° 24′	2° 48′	3° 12′	3° 37′	4° 1′	4° 24′	4° 48′	5′ 12′	·5° 36′	6°	6° 24′	7° 11′	7° 58°	8° 45′	9° 32′	10° 19′	11° 5′	11° 52′	12° 38′
8D	28′	56′	1° 22′	1° 50′	2° 18′	2° 46′	3° 12′	3° 40′	4° 8′	4° 35′	5° 2′	5° 29′	5° 56′	6° 24′	6° 50′	7° 18′	8° 12′	9° 6′	9° 59′	10° 52′	11° 45′	12° 38′	13° 30′	14° 22′
9D	31′	1° 2′	1° 32′	2° 4′	2° 35′	3° 6′	3° 37′	4° 8′	4° 38′	5° 9′	5° 39′	6° 10′	6° 40′	7° 11′	7° 41′	8° 12′	9° 12′	10° 12′	11° 12′	12° 11′	13° 10′	14° 9′	15° 7′	16° 4′
10D	35′	1° 9′	1° 43′	2° 18′	2° 52′	3° 26′	4° 1′	4° 35′	5° 9′	5° 44′	6° 17′	6° 51′	7° 24′	7° 58′	8° 32′	9° 6′	10° 12′	11° 19′	12° 25′	13° 30′	14° 34′	15° 39′	16° 42′	17° 45′
12D	41′	1° 23′	2°.4′	2° 45′	3° 26′	4° 7′	4° 48′	5° 29′	6° 10′	6° 51′	7° 31′	8° 12′	8° 52′	9° 32′	10° 12′	10° 52′	12° 11′	13° 30′	14° 47′	16° 4′	17° 20′	18° 34′	19° 48′	21°
14D	48′	1° 36′	2° 24′	3° 12′	4° 1′	4° 48′	5° 36′	6° 24′	7° 11′	7° 58′	8° 45′	9° 32′	10° 19′	11° 5′	11° 52′	12° 38′	14° 9′	15° 39′	17° 7′	18° 34′	20′	21° 24′	22° 47′	24° 8′
16D	56′	1° 50′	2° 46′	3° 40′	4° 35′	5° 29′	6° 23′	7° 19′	8° 12′	9° 6′	9° 59′	10° 52′	11° 45′	12° 38′	13° 30′	14° 22′	16° 4′	17° 45°	19° 24	21°	22° 7′	24° 8′	25° 38′	27° 7′
18D	1° 2′	2° 4′	3° 6′	4° 8′	5° 9′	6° 10′	7° 11′	8° 12′	9° 12′	10° 12′	11° 12′	12° 11′	13° 10′	14° 9′	15° 7′	16° 4′	17° 57	19° 48′	21° 36′	23° 22′	25° 5′	26° 45′	28° 22′	29° 57′
20D	1 9′	2° 18′	3° 26′	4° 35′	5° 44′	6° 51′	7° 58′	9° 6′	10° 12′	11° 19′	12° 25′	13° 30′	14° 34′	15° 39′	16° 42′	17° 45′	19° 48′	21° 48′	23° 45′	25° 38′	27° 29′	29° 15′	30° 58′	32° 37′

of accommodation on convergence negative glasses must be worn, and their prismatic effect ascertained. This necessitates a brief study of the subject.

When each visual line passes through the optical centre of its corresponding glass, since it undergoes no practical deviation, the effect on convergence is *nil.* But if either traverses the glass away from this point, it undergoes a deflection—*towards* the optical centre in a convex lens, and *away* from it in a concave. The *amount* of deflection depends on—(1) the focal strength of the lens; and (2) the distance from the optical centre that the glass is traversed by the visual line. In the accompanying table I have worked out the angular *bend* or deflection experienced by the visual line in passing through lenses of different strength (left hand vertical column), and at different distances from the optical centre in millimetres (highest horizontal column).[1]

The deviation with glasses of low refraction is seen to be very trivial, but with cataract glasses of 20D each visual line would be turned more than 9° out of its course if each optical centre lay one-third of an inch to the inner or outer side—an angular effect nearly equal to that obtained by wearing two clinical prisms, each marked 18°, or a single one of 36°.

The table may be made of practical use in prescribing the exact distance between the optical centres required by different patients. It is often desired, in cases of so-called "insufficiency" of the converging function, to combine weak prisms with their bases towards the nose with the spectacles ordered, and thus more or less relieve convergence simultaneously with accommodation.

If the required prisms are weak enough, and the lenses strong enough to obtain the effect by adjustment of the mutual distances of the optical centres, all that is necessary is to run the eye along that horizontal column opposite the required strength of lens till the angle is found which is nearest to the declinating angle[2] of each prism. The number of mm. marked at the head

[1] The angle required is that whose tangent is obtained by dividing the distance of the visual line from the optical centre by the focal length of the lens.

[2] The real declinating angle of each prism is about half the refracting angle by which it is marked. Since the only clinical use of prisms is to deflect the rays of light which traverse them, and since the index of refraction varies with the

of the vertical column in which this angle is found then gives the required *displacement* of each glass.

Suppose, for instance, it is desired to combine two prisms with their bases towards the nose, each of 4° (according to the clinical marking), with convex lenses of 5D. The eye runs along the column marked 5D till it meets with the angle 2°, and then upwards; finding that each glass must be displaced 7 mm., and that therefore the distance between the optical centres must be shortened by 14 mm.

If the patient's intercentral distance were 62 mm., glasses for distant vision would be ordered, with a distance of 48 mm. between their optical centres.

The shortening may be made either by approximating the circular rims or by decentering the glasses in their rims, or, best of all, by a judicious combination of both. The optical effect is, of course, in either way exactly the same as if each lens were split, and a prism of 4° were inserted and cemented between its two curvatures (see fig. 17). The displacement should always be made by approximating the encircling metal rings as much as can be done without interfering with the field of fixation, leaving only the remainder of the displacement to be obtained by decentering the glasses in their rims, since this latter has always the disadvantage of increasing their *weight*, just as the insertion or addition of a prism would do.

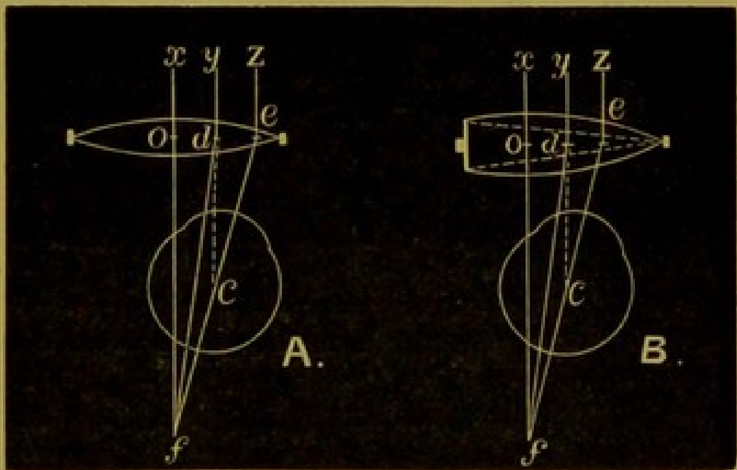


FIG. 17.—A. The right eye is looking at a distant object through a lens *displaced* inwards, *rim and all*, by a distance *o d*; *d c e* is the effect on convergence. B is the same eye looking through a lens of the same focal length, but *decentered in* its rim by the same distance *o d*. The dotted lines indicate the prism that would be equivalent to the decentering, and the effect on convergence is the same as with A.

To combine prisms, with their *apices* towards the nose, with convex

different specimens of glass, the usual method of marking prisms is not very satisfactory for exact purposes. After consultation with Messrs Pickard & Curry, they have now a quick method of testing the declinating angle of their prisms.

glasses (as for strabismus), the procedure is exactly the same, but the mm. found indicate the *lengthening* instead of the shortening of the distance between the optical centres.

The table also applies equally to concave glasses, but the conditions are opposite, for to combine with them prisms base inwards the number of mm. indicate the required *lengthening;* while for prisms base outwards they indicate the required *shortening.*

The *starting length* for the required shortening and lengthening should for distant-vision glasses be the patient's intercentral distance; but in near vision the starting length should be the separation of the visual lines at their passage through the glasses —a separation which is clearly *less* than the intercentral distance, about $\frac{1}{10}$th less if the reading distance be 10 inches, $\frac{1}{15}$th less if 15 inches, and so on. It varies, of course, with the distance of the spectacles from the eyes; but the average shortening at reading distance would be about 4 or 5 mm., according as reading distance is taken as 15 inches or 12 inches (preferably for this purpose the former).

If, therefore, the spectacles already mentioned were prescribed for reading purposes at an average distance of 15 inches, the glasses would be ordered with a distance of 44 mm. (4 mm. less) between their optical centres. If for close reading purposes, at 12 inches or 10 inches, with a distance of 43 or 42 mm. between them (5 or 6 mm. less). All this, however, though it shows the *prismatic* equivalent of decentering a lens, or shortening or lengthening a pair of them, does not tell us their exact *effect on the convergence*, and must not be confused with the subject we now come to.

Effect of Spectacles on Convergence.—At first sight the table already given would appear to tell us this, but it does not. Suppose a pair of 5D spectacles to be *shortened* by 13 mm., as already instanced. The table would truly tell us that their prismatic effect is the same as if they were combined with two prisms, each marked 4°, and therefore each with a *deflecting* power of 2°. But the joint deflecting power (4°) of the two superadded prisms would only represent the diminution of convergence they occasion when the object viewed is at the focal distance of the lenses. When vision occurs *within* the focal length of the convex glasses, their effect on the convergence is *less* than the joint deflecting power of the combined prisms; when *beyond*, the effect on convergence is greater, while for *concave* glasses the effect on convergence is *always less* whatever the distance. Fig. 17, A, shows a convex lens displaced, rim and all, by the distance *d o*, and used in distant vision. *x* is the "principal axis" of the lens; *y* is the pencil of light which *would* enter the eye if the lens were not intervening, and whose declination (*c d f*) therefore is that given by the last table. But the pencil (*z*), which really enters the pupil and coincides with the visual line, traverses the lens nearer its margin, and therefore is deflected by a greater angle (*d c e*), which is the exact expression of the effect of the lens on the convergence of the eye.

Fig. 17, B, shows a lens decentered *in* its rim to the same extent, the effect being the same as if a prism, seen in dotted outline, were inserted. That the difference between the declinating angle of this prism and the effect of the combination on convergence may be considerable, is seen from the fact that, with cataract glasses of 20D, the effect on convergence is nearly double, so that the practical inconvenience from ill-judged adjustment of the distance must be considerable *if* both eyes are used together.

For practical purposes, with distant vision an approximate estimate of the effect of a displaced or decentered convex lens[1] on convergence may be made from the table, by multiplying the angle there found by the focal distance of the lens in inches, and then dividing the product by the same distance, *less one;* but if the lens be concave, divide the product by the same distance, *plus one.* In this the distance of the glass from the centre of rotation of the eye is taken as one inch, rather less than half an inch from the cornea. When the glass is nearer, the difference from the table is less; when further, the difference is greater.

Suppose in a myope, with divergent strabismus in near vision, and latent convergence of 3° revealed by the camera in distant vision, it is desired to give distant vision glasses of 5D, such that there shall be 3° of convergence in their use instead of parallelism. Each eye would converge 1° 30′, and therefore each lens must be displaced enough to cause that. The focal distance of the required lenses is 8 inches. This *plus one* is 9 inches. Now, since with concave lenses the *effect on convergence* is *less* than the prismatic equivalent of the decentration as given by the table, it is clear that the decentration must be greater than is there indicated; and since 1° 30′ is the required effect *on convergence*, the prismatic equivalent of the decentration must be *greater* than 1° 30′. How much greater? So much that

[1] The exact formula for a convex lens, where x is the required angle of convergence, f the focal length in mm., and d the number of mm. of decentering or displacement, is $\tan x = \frac{d}{f-25}$, and for a concave lens, $\tan x = \frac{d}{f+25}$, where 25 mm. is taken as the distance of the lens from the centre of rotation of the eye.

when multiplied by the focal length of the lens (8 inches), and divided by the focal length, plus one (9 inches) it will give 1° 30′, according to the last rule. We need, therefore, only invert the fraction to find it; that is, multiply by $\frac{9}{8} = 1\frac{1}{8}$. We increase the angle 1° 30′ therefore by $\frac{1}{8}$th as much again, which gives 1° 41′ 15″ as the required prismatic equivalent of each glass, to produce which it must, according to the table, be decentered nearly 6 mm. With a lens of 6 inches focal length we should add $\frac{1}{6}$th as much again, and so on.

I do not give this instance to advocate the special proceeding in any given case, but to illustrate how the table may be used.

The rules may be given shortly as follows:—

I. *To find the prismatic equivalent of a known amount of decentration of a spherical lens.*

Rule.—Find in table the angle opposite the dioptric strength of the lens, and under the given number of mm. of decentration. This is the *declinating* angle of the required prism, without further correction. (*Doubled*, it gives the refracting angle of the required prism, but not quite, if of high power.)

II. *To find the decentration of a lens necessary to obtain the known effect of a given prism.*

Rule.—Look in the table, opposite to the given dioptric strength of the lens, for the angle which approaches nearest to the declinating angle of the prism, the number of mm. at the head of the column thus found gives the decentration required without further correction.

III. *To find the effect on the convergence of a known amount of decentration* (in distant vision).

a. With a convex lens. Rule.—Find the prismatic equivalent by Rule I. and *increase* it, by its own value divided by the focal length in inches *minus* one.

b. With a concave lens. Rule.—Lessen the prismatic equivalent, by its own value divided by the focal length *plus* one.

IV. *To find the decentration of a lens necessary to produce a required effect on convergence.*

a. With a convex lens. Rule.—Lessen the required effect on convergence by its own value divided by the focal length of the lens in inches, and find in the table, opposite the dioptric strength of the lens, the nearest approach to the angle thus obtained. The number of mm. at the head of the column thus found gives the decentration required.

b. With a concave lens. Increase the required effect on convergence by its own value divided by the focal length of the lens in inches, and find in the table, opposite the dioptric strength of the lens, the nearest approach to the angle thus obtained. The number of mm. at the head of the column thus found gives the decentration required.

These rules refer to a single lens, so that with spectacles the results are doubled. For cylindrical lenses their strength in the horizontal meridian should be taken into account. It is easy to find roughly the distance between the optical centres of a pair of spectacles by having parallel lines at different marked distances ruled on a piece of paper; the spectacles are then held horizontally above them (preferably with their ends resting on two small movable blocks of wood), and the two lines are found which appear to run continuously without deflection through each glass when the eyes are held about 12 or 18 inches above them, the left eye being shut on looking through the right lens, and the right eye on looking through the left. This method is not exact, of course, but gives a quick approximate idea.

Exact measurement, however, is preferable often, and is not difficult, though it needs either the sun or an artificial light. (1) With the sun, it suffices to hold a metric rule at the focus of the glasses; the distance between the two images is the distance between the optical centres. (2) With an artificial light behind a strong convex lens, and just at its focus, the method is of course the same as with sunlight. But with a bare artificial light, the distance between the images needs to be multiplied by their distance from the spectacles, and divided by their (greater) distance from the flame. (3) If, on the principle of the phakometer, the glasses are always midway between the rule and the light, the distance between the images must, of course, be halved.

Great precision in prescribing the *very best* distance between the optical centres is seldom necessary, but there are a few cases of asthenopia where it seems desirable, since slight differences in this particular exert a marked influence.

The position of equilibrium of the dissociated eye, as shown by the camera (direct method), must not, I should think, alone be taken into account, for though it does indicate the amount of work falling to the fusion supplement, and this is a great point in prescribing, it does not by any means indicate surely relative inefficiency of the converging function. The highest attainable relative convergence, as tested by the use of prisms in the camera, and which gives the reserve power of the fusion supplement for reading distance should be taken with it into *joint* consideration.

Spectacles which *lessen* the latent outward deviation, and the work of the fusion supplement for reading distance, of course bring the point of coincidence nearer, and therefore *increase* the fusion work for distant vision.

If patients are told to look through the camera with their spectacles on, the total effect of the refractive combination can be estimated by the macular method.[1]

For the exact effect of near-vision spectacles on convergence, I have not yet succeeded in making a simple enough formula. The complicated method I have arrived at, is (though I believe correct) far too cumbrous to recommend here for others, especially as I have no doubt that any mathematician would speedily reduce it to greater simplicity.

Treatment of Squints by Prisms.—Many slight squints, especially if paralytic, can be cured by the wearing of suitable prisms. Donders has shown that in hypermetropes, so long as strabismus occurs only intermittingly with fixing of an object, its development may be prevented by wearing convex glasses which neutralise the existing H.[2] But there are stages beyond this in which the fusion effort, even with appropriate glasses, is not quite able to overcome the squint. If vision is still binocular, which it is sure to be if the squint is still alternating, the *lowest* prism should be found which brings the double images near enough for the fusion effort to make them unite with ease. The effect of this prism should be divided between the two spectacle glasses, either by combining a prism of half the power with each or by having the mutual distance of the optical centres adapted according to Table III. In paralytic squints, without errors of refraction, simple prisms may be worn alone, and exchanged for weaker ones as the cure advances. The camera furnishes a quick method of discovering what proportion of a squint is overcomeable by fusion effort, and what proportion remains to be remedied by prisms.

For a case of "strabismus divergens," it is only necessary to replace one of the little wooden slides at the narrow end of the camera by a weak prism, base up or down, and of such a strength that it just makes the aperture seen by one eye appear level with that seen by the other.[1] One aperture is then made slowly to approach the other

[1] An adjustable trial frame can be used also, and the lenses approximated till the relative position of the superimposed images becomes that which the surgeon deems most suitable for the special case. Instead of the camera for distances greater than 10 inches the double prism can be placed in the trial frame before one of the lenses, which are then approximated till the three images of the object are vertical. In this way spectacles can be prescribed which bring the point of coincidence to any required distance.

[2] *Accommodation and Refraction*, p. 305.

till the desire for fusion makes them unite; it is then again withdrawn till the images separate. Its position on the graduated scale records, the first time, the power to *attain* fusion, and the second time the power to *maintain* it. Thus if, with a squint of 20°, the images united when the left aperture was at 5° and the right aperture at 3°, we should then know that 8° (3° + 5°) of the squint is overcomeable by fusion effort, and at least 12° remain to be overcome by prisms, in prescribing which *each* should be notified as rather more[2] than the angle it is decided to correct. "We may," says Donders, "in paresis of a muscle so far meet the disease by means of a prism, that in order to make the double images which have been brought near one another, run together, the muscles will become powerfully tense, which, for the alleviation of the paresis, appears to be no matter of indifference."[3]

For "strabismus convergens" the crossed camera must be used in the same way, or else one of the strong prisms, already mentioned, for the measurement of convergent squint must be combined with the weak vertical one, or, better still, be replaced by a combination prism which combines their effects, and which is made by cutting an ordinary prism at a known obliquity.[4]

These same simple tests are also available, apart from strabismus, to estimate at reading distance the power of the fusion effort to cause corrective relative convergence or divergence of the visual axes. They thus replace the more arduous task of trying one prism after another to find the highest prism, base in or out, compatible with single and distinct vision—a test which gives varying results from psychical reasons. Thus I have known the eyes at one moment overcome a prism of 26°, and at another, only with difficulty able to overcome one of 14°. Testing the power to *maintain* fusion with increasing separation of the images will, I think, give more uniform results than testing the *attainment* of it. The latter so greatly depends on the concentration of attention, and other things difficult to analyse.

[1] The prism would of course be always the same for the same camera. Both lateral apertures may be used, or the central one and one lateral. In the latter case the brass slides are better to be replaced by others with apertures equal in size to the central one, but this is not imperative.

[2] I say "rather more," because though the refracting angle is rather *less* than double the angle of declination, yet the distance of the prism from the eyes makes it affect convergence by less than its own angle—for 10 inches distance nearly $\frac{1}{10}$th less; for 15 inches, $\frac{1}{15}$th less, and so on.

[3] *Accommodation and Refraction*, p. 134.

[4] These prisms are supplied with the camera, when desired, by Messrs Pickard & Curry.

I would suggest that the greatest relative convergence attainable by the last camera-test indicates *the reserve power* of the "fusion supplement" at reading distance; and this would have an important bearing on the working efficiency of the converging function. We should not expect the latter to fail readily if we found a large reserve of energy. On the other hand, a very small reserve would indicate in asthenopes an insufficiency of the converging function at reading distance—a distance at which I need not allude to the practical advantage of making the test, since it is always in near vision that insufficiency is felt.

ADDENDA.

Note 1.—The theory has been advanced that "accommodation and the convergence are two primitive independent visual faculties," which, notwithstanding their original independence, "pass into a secondary dependence." This view appears untenable for a moment in the face of clinical facts, for (1) such secondary dependence could never account for the *excessive* attachment of convergence to accommodation which causes relative *convergence* of a hypermetrope. Dr George Berry also has found latent convergence in some emmetropes; (2) the very fact that strabismus develops from this cause shows the unyielding nature of the primitive *dependence;* (3) it would be strange if this early tendency to pass into secondary dependence should, after so great a manifestation of its effects, entirely cease; it would surely continue through life, even if in diminished amount, so that we should expect latent divergence to become gradually less with advancing years. Dr Joseph Bolton (though I do not think his observations were taken on a scale large enough not to need further confirmation) came to the conclusion, by using the camera, that the average relative divergence of emmetropia is *less* in children, and *increases* with advancing age. If this should be found true, it surely indicates the opposite of progressive secondary dependence, and would also lead us to expect, *cœteris paribus*, that internal strabismus would lessen, and external strabismus increase by age. It would also have a great bearing on the fact mentioned by others that internal strabismus sometimes disappears spontaneously as a child grows up. I do not attribute this to weakening of the internal recti, for the ciliary muscle also weakens.

Note 2.—In the original of this thesis I suggested the adaptation of a Sorby-Browning microspectroscope to an ophthalmoscope for the direct method to analyse by the spectrum the red glare from the choroid, and attempt to detect various drugs and changes in the blood. I also gave a drawing of an instrument to examine the refraction of an eye under chloroform, and note the behaviour of the ciliary muscle as the pupil alters; but as neither of these have been constructed, I need not dwell on them.

Note 3.—My attention has just (June 1886) been called by Dr George Berry, and I think justly, to the fact that I have not in the letterpress of Section II. laid sufficient stress on the importance of securing exact accommodation of the fixing eye. My original idea,[1] in the card-board model I first made, was to have a strip of printed paper fastened to the movable slide (along that slit of it which is made to let the central aperture be seen through), so that the very act of moving the slide kept a constantly changing series of letters in the central aperture before the patient's view. The purpose was to have a similar strip attached to the outer end of the slide for the surgeon's eye, to ensure that the patient kept truly reading the letters as they passed in succession; but although I have attempted to carry this out with the brass camera, the practical difficulties of manufacture have as yet been too great. As it is, I always make the patient read the letter placed in the central aperture,[2] (looking first through the camera myself to adjust it well in view), and generally, I believe, keep the attention fixed, not only before the slide is moved, but after, by such questions as "Has the letter changed?" "What is it now?" "Do you see any change *in* it?" And after taking the observation by the direct method, I ask, "*While* you are looking at the letter is the lower point of light still just underneath it?" Monocular accommodation so soon grows weary in some patients that the speediness of the direct method gives it a great advantage, though before taking the record, if results are to be at all uniform, a definite timed interval must be allowed to elapse, not less than 30 or 40 seconds, for the deviation to occur. Mere tiring of accommodation, so long as the patient is still made to read the letter, would, I should think, tend rather to diminish the deviation than increase it, since more central effort is needed.

Note 4.—Dr Berry also mentioned to me the effect of "the knowledge of the proximity of the object fixed." I became aware of this by a fact which at first puzzled me greatly, viz., that, try as I would to relax the accommodation of the fixing eye by + glasses before the central aperture, in looking at an object 40 inches distant I could not get the other eye to deviate outwards to the "initial" position, which, however, it took the moment a distant tree became the object (without glasses). I attributed it to the "mental conception of nearness." There was, if I remember, about 1° difference between the two methods, and when this was partially overcome the point looked at seemed to *recede* in a very strange manner, and looked quite distant, yet evidently with a mental struggle between the *knowledge* of its nearness and the optical illusion of its recession.

It would thus be quite easy to estimate in different cases the effect

[1] As previously stated in the *Trans. Opth. Soc.*, 1882–83, p. 292.

[2] Footnote 2, p. 479.

of "the knowledge of proximity" by looking at an object without glasses and then at a nearer one with lenses to make accommodation equal in the two trials. I find instillation of atropine into one eye does not affect the relative divergence so long as the other eye is used in fixation. But if the atropised eye be the fixing one, the divergence begins to diminish in less than half an hour, and is required to *nil* in time, though whether it gives way to relative convergence I was not able to discover. Even on the fifth day after thorough atropisation, when the pupil was considerably reduced towards its natural size, and the near point $6\frac{1}{8}$ inches, no deviation occurred on first using the camera, though with time the other eye slowly crept out to 1°, and then to 2°. At the same time, if the unatropised eye were used in fixation there was uniform divergence to 8°, perhaps due to the fact that vision had for some days been almost monocular.

A fact for which I am wholly unable to account is, that the distance of the point of coincidence appears to be lessened by a long distant gaze, as at the clouds, and increased by close reading.

Note 6 *to p.* 30.—There is one form of squint for which the ordinary methods of strabismometry are unavailable, owing to the fact that the point of acutest vision has shifted its position to another part of the retina.

The eye thus affected continues to squint even when the other is covered. So there is no starting-point to measure from.

The camera is specially adapted for these cases since the blind-spot furnishes an excellent index, and all the more so because it now lies nearer the point of acutest vision.

If the refraction of both eyes is the same, the distance between the fovea and the blind-spot may also be assumed to be the same in both.

Suppose, for instance, in the unchanged eye this angular distance were recorded by the camera as $12\frac{1}{2}$°, but that on testing the other eye the inner border of the blind area lay at only 5° from the new point of acutest vision, we should know that the latter point had shifted $6\frac{1}{2}$° from its former place. This of course is tested with the unchanged eye excluded by the stop; but if now the stop be pushed to the side of the changed eye, and the blind-spot of the latter be found at 4°, we should correctly infer that the squint, being 1° greater than the movement of the acutest point, was *advancing*, so that the latter could not keep pace with it. Should the eyes not be of equal refraction, or should there be reason to think the acutest point of *both* eyes is changed, the distance of the blind-spot from the fovea may be assumed to be $12\frac{1}{2}$°, rather more in hypermetropes and less in myopes (but *how much*, more or less on the average, it requires a series of observations to determine).