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COMMUNICATIONS

THE MEASUREMENT OF HETEROPHORIA

BY

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"The whole question of the disturbance of muscular equilibrium of the eyes occupies the parallel in ophthalmology that the fifth proposition of the first book of Euclid does in Geometry . . ." (LAUDER, 1910)

PART I

Introduction

"I am persuaded that while an immense amount has been written on this subject, much of it has not been so clearly expressed as to edify every reader."

(DEAN, 1895)

"... the rather vexed subject of heterophoria, about which much that helps little has been written. . ."

(CHAVASSE, 1939)

FORTY years on—and the burden of the ophthalmic song is unchanged.

Fafner still guards the cave, but the fire is being slowly heated by the fuel of research against the day when Nothing shall be forged. It is the purpose of this paper to collect, in some measure to collate, and occasionally to assay, the material which is distributed over the

literature of seventy years and so, perhaps, to prepare for Siegfried's triumph.

"The Epicritic power of the bifovea is reflected in the exactness of the binocular posture" (Chavasse, 1939). By what means that exactness is maintained is now substantially known: by what methods the effort involved may be measured is not so satisfactorily or generally agreed. The diversity of methods employed is a measure of the uncritical attitude with which they have been accepted, and that absence of criticism is due largely to the lack of neuro-physiological knowledge in the light of which they must be assessed. Unlike wine, the age of a statement is no indication of its soundness; and in drinking, at the well of science as at the fountain of Bacchus, though due respect should be paid to reputation, that respect must be epicurean, not slavish.

It is obviously desirable first to define what is being measured, and to proceed from that to a discussion of the difficulties which hamper the attempt at measurement. The desiderata of the perfect method will emerge naturally and in the light of these desiderata the existing methods may then be examined.

Section I

The position of the visual axes in orthophoria is, perhaps, the first thing to define clearly. There are two main views as to what exactly is meant by orthophoria, and the difference between them is best summed up by Beyne (1932) who says that to the Americans orthophoria means visual parallelism, but to the French the position in which images of a distant object are fused. The English view was given by Maddox (1929), albeit somewhat cryptically, when he said, "We now all consider orthophoria to exist for any distance, whenever a screened or dissociated eye maintains its correct posture." This, translated into terms of orthophoria for distance, coincides with the French view.

Stevens (1887) certainly regards orthophoria as visual parallelism in the distance. Ballantyne (1922), though not defining orthophoria, has drawn an analogy which, for its vividness, deserves to be remembered:—"The attitude of the eyes in binocular vision is that of *attention*: the position revealed by the Maddox rod and other heterophoria tests is that of *stand at ease*." Stutterheim's term carries something of the same content, for he describes the eyes fixed on a distant object as being in the "functional primary direction" (Stutterheim, 1932). Ball (1913) also lays stress on activity in his statement that the divergence of sleep "is immediately succeeded on waking by the functional position of rest, in which the visual axes become parallel." The difference between the two views is not purely academic. The patient with an interpupillary distance of 60 mm. who is examined on American

standards will show an esophoria of exactly 1 prism-dioptre, whereas by French or English standards he will show orthophoria, at 6 metres. The definition, visual parallelism, has the advantage that it is independent of the interpupillary distance. Perhaps this confusion explains Standish's curious statement "Normal muscular condition is such that, when looking at infinity, there should be an absolute esophoria of one or two degrees" (Standish, in Norris and Oliver, Vol. IV, p. 781).

There is, however, another aspect of this problem which is instinct in Stutterheim's and Ball's terms. It is obvious that at no time when binocular vision is in use can the visual axes diverge. On the other hand, the angle of convergence of the visual axes is small with the minimal amount of convergence in play, can be made smaller the greater the distance of the fixation point and, in the limit, can be made infinitesimal whenever we regard, for example, the night sky. Indeed, in 1885 the use of a star as fixation point in heterophoria tests was recommended (Stilling, 1885). This infinitesimal angle, which can always be made still smaller, is then the zero from which convergence must start. It is the angle between the visual axes when the eyes are in the functional primary direction and it will introduce only an infinitesimal error to take this angle as zero. In brief, therefore, the functional primary direction is effectively visual parallelism, and the definition of basic orthophoria is that state of the eyes in which no deviation occurs from the functional primary direction when fusion is partly or completely suspended.

Throughout this paper the word "basic" prefixed to any term in "-phoria" will be used to mean the "phoria" measured, in effective emmetropia, at infinity. The reasons for this are more appropriately discussed later (Section III).

Section II

Next, what is meant by the more commonly used term, the anatomical position of rest? Duke-Elder puts the facts concisely: "The absolute position of rest (or the anatomical position of rest), that is, the position assumed by the eyes when they are dissociated from all neuro-muscular control, is impossible to determine in ordinary circumstances since the extra-ocular muscles are continuously in a state of tonus. In total ophthalmoplegia the eyes rarely look straight ahead in a parallel direction, but usually diverge slightly, and very exceptionally converge" (Duke-Elder, Vol I, p. 579). The anatomical position of rest is unrealisable in a normal pair of eyes "because it is impossible to eliminate the totality of nervous influences" (Bielschowsky, 1934); it is a conception which only attains concrete and measurable form in total ophthalmoplegia. Stutterheim (1933) calls it the "primary

position"—an objectionable term since it is liable to confusion—and says of it that it is one of divergence, elastic, with the eye held there by anatomical structures. He describes it as "stable, passive, elastic equilibrium."

There is, however, another school of thought which does not demand the removal of all nervous influences, and speaks of the anatomical position of rest as alternatively attainable when the muscles "are all perfectly equally innervated so as accurately to counter-balance each other" (Hansen Grut, 1890). Chavasse agrees with Duke-Elder's view: "Only when both eyes are cut off from every source of stimulation . . . do they assume a position of rest that is *absolute*. No reflex action, no reflex muscular tonus then disturbs the anatomical picture" (Chavasse, 1939).

The introduction of the idea of balanced tonus immediately involves an indefinite and, in any individual, variable entity, for the tonus may be balanced at different levels of muscular tension in different physical states of the same individual. Every element of indefiniteness in a definition lessens its utility, and the definition of the anatomical position of rest as the position of the eyes when completely deprived of all nervous influences stands as the least indefinite statement that it is possible to choose. It is desirable to emphasize that the conception of the anatomical position of rest is unattainable in the normal eye: it remains therefore a conception.

The factors determining the anatomical position of rest may be briefly noted. They include the shape and size of the orbit and its relation to its fellow of the opposite side, the size and disposition of the orbital contents, the insertions of the muscles, the elasticity of, and the tension in, the various structures contained in the orbit, eyelids and, indirectly, the skin of the face. Finally, the turgidity of the orbital contents due to their contained fluids and, of course, the blood pressure, also play their part in fixing the anatomical position of rest. Dean (1936) has added to these "malposition of the fovea."

It will not escape notice that there are certain possibilities of variation, due to these factors, in the anatomical position of rest itself. There has been no useful definition which excludes these possibilities and the range of variation in the same individual which they could cause is unlikely to be significant. It must not, however, be forgotten that one of the fundamentals on which the study of heterophoria is built is not, itself, absolute.

As to the position which the eyes would occupy when in the anatomical position of rest, there is general agreement that it is, in the vast majority of cases, divergence. Hansen Grut (1890) set out some of the arguments when he pointed to the divergence in sleep, in death before rigor sets in, under anaesthesia, and in cases of blindness, and to the fact that the orbital axes diverge, by as

much as 40° to 45° (Whitnall, 1921). Many other authorities have expressly or by implication signified their adherence to this view, although precisely what each has meant by the term chosen for the position of rest has not always been made clear. One of the most interesting contributions to the support of divergence as the anatomical position of rest is the fact that exophoria is the almost invariable finding after prolonged occlusion of one eye. Chavasse is quite definite: "The anatomical position of the eyes—the position of absolute or dead rest—is one of divergence, associated with some sursumvergence" (Chavasse, 1939).

This problem is intimately linked with that of the existence of a divergence centre. It is a question on both sides of which strong views have been held. There is first the existence of paralysis of divergence. Several cases are quoted in the literature (*e.g.* Zentmayer, 1911, Dunne, 1905*b*), and Walter (1916), examining these, concludes that there is "convincing evidence of a distinct center for divergence." Peter (1928) agrees, more cautiously, with this, and Bielschowsky (1935) holds the same view. The latter says also, however, "it would be wrong to assume that the limit of divergence is identical with the position of rest resulting from a complete relaxation of the convergence innervation" (*ibid.*) Marlow (1915), although dealing here with his prolonged occlusion method, which will be examined later, concludes "That the total exophoria may greatly exceed the abduction as measured previous to occlusion." Taking these last two remarks together, it is surely legitimate to conclude that this limit of divergence or total exophoria is, in fact, either the anatomical position of rest, or nearer to it than can be reached by methods such as ductions. There are three records in the literature of potential voluntary divergence (Barrett, 1921, Maddox, 1929, and a case reported by Holloway & Spiller, which is referred to by Peter, 1928). Of these the first two suggest an extreme degree of voluntary inhibition, comparable to the fakir's control of pain, or the same phenomenon in fire-walkers, while the second immediately raises the additional question:—"How is it possible to differentiate between voluntary excitation of one set of muscles and voluntary inhibition of another by introspection alone?" Of the third no details are known.

On the other side, Berry (1912) clearly holds the view that divergence is essentially an inhibition of convergence. Livingston (1932) refers to "an ocular fatigue of sufficient amount to produce a palpable exophoria," which obviously precludes the idea of an active divergence in the cases to which he refers. Hansen Grut (1890) is very emphatic, although his statement does no more than imply an active process: "no one is able to bring about a divergence of the two eyes . . . because the habit and practice of such

a movement is altogether absent." Again, Thomson (1924) says, "The eyes lie in an anatomically divergent position—the cadaveric position." That the anatomical position of rest is divergence is a necessary preliminary to Stutterheim's conclusion that "the extrinsic muscles act as tensors They act by contraction in the direction away from the primary position"—his "primary position" is the same as the anatomical position of rest. He goes on, "*in the direction towards the primary position they act by inhibition alone*" (Stutterheim, 1933: the italics are his). Kletzky (1920) makes the interesting suggestion that there is also a negative accommodation, *i.e.*, normally some tonic accommodation is present which cannot be relaxed, and adduces the evidence of myopes who require a weaker concave sphere when the ciliary muscle is toxic or paralysed. This tonic accommodation would produce a part of the convergence which transfers the eyes from the anatomical position of rest to the functional primary direction, and its presence even in myopes would lead one to suppose that the anatomical position of rest is always divergence.

It would appear, therefore, that the balance of evidence is against the existence of divergence as an active movement. Maddox, on the other hand, definitely progressed to the conclusion that there was active divergence. In the Doyne Memorial Lecture of 1920—a model paper from the point of view of arrangement and phrasing—he tentatively includes the divergence reflex in his Kingdom I of the innervations of binocular vision, describing it as "the only one I have not been able to prove positively" (Maddox, 1921). In 1929 he says, "von Graefe and his pupils doubted the existence of a diverging innervation altogether, on the ground that it had no useful purpose to serve since we never *need* to diverge; but, to borrow another's analogy we might as well say that because we cannot extend our fingers beyond a slight degree, therefore there are no extensor muscles in the forearm. The extensors exist in order to balance and steady and reciprocate with the flexors" (Maddox, 1929). Like all analogies, this is a dangerous form of proof. There is no natural spring of the fingers tending always to extend them, whereas the eye tends to return to divergence in the vast majority of (if not all) cases.

Mackay, in the discussion that followed Maddox's paper in 1929, gave the answer to Grimsdale's claim to possess voluntary divergence. He said that he had seen cases which, with the Maddox rod, seemed to show a deviation of only 2° or 3° , "but with a relaxation of effort it mounted to 10° or 12° " (see Maddox, 1929). Stevens emphasises the importance of relaxation: "In all examinations relating to the equilibrium of the eye muscles, the fact that the element of voluntary effort on the part of the person examined can rarely be eliminated is not to be underrated" (Stevens, in

Norris and Oliver, Vol. II, p. 170). Duke-Elder sums up the position thus: "The existence of a centre for divergence is much more questionable, for divergence can most usefully be regarded as a relaxation of convergence" (Duke-Elder, Vol. I, p. 288).

In view of the absence of unequivocal evidence as to the existence of a divergence centre, the conclusion would seem to be not unreasonable that divergence is the invariable anatomical position of rest, and that the infrequent cases where an esophoria has been found after such thorough elimination of nervous control as is procured by the prolonged occlusion method are explained by the fact that the elimination has not been thorough enough. While recognising that there may be rare cases which do not conform, it is certain that arguments based on divergence as the anatomical position of rest can take the problem much further than any not so based.

It should be noted that asymmetry of any of the factors which determine the anatomical position of rest may result in a difference, between the two eyes, of the deviations of the visual axes from the sagittal and horizontal planes of the body in that position of rest. Such asymmetry is common and, in that the anatomical position of rest influences all other positions of the eyes, is one factor in explaining the different heterophoria found in the two eyes, such as is quoted by Valk (1912), Savage (1891, 1901), Taylor (1932), Friedenwald (1936), and Dolman (1920a).

Section III

The next "position of rest" to demand examination is that generally described as the "physiological position of rest" (Duke-Elder Vol. I, p. 579, who also quotes the "relative position of rest," the "fusion-free position," and the "blind position" as other terms employed). The factors involved in transferring the eyes from the anatomical position of rest to the functional primary direction are all nervous reflexes, but of various origins. They fall into two main groups, the postural and the psycho-optical (Duke-Elder, Vol. I, p. 612). The postural group are concerned with the orientation of the eye in space, and are essentially not concerned with binocular vision, for they direct the eye in relation to the body, not the eye in relation to its fellow. These reflexes are initiated by changes of position of the body or by changes of movement, and they are present in cases with monocular, as well as with binocular, vision. They direct the blind eye equally with the seeing, and it is to be noted that in the monocular cases their activities cause no symptoms. It may safely be assumed—though it is to be noted that it is an assumption—that in cases with binocular vision also they cannot be held responsible for such symptoms as may be ascribed to heterophoria.

The psycho-optical group consists of three main reflexes, the fixation reflex, the fusion reflex, and the accommodation-convergence reflex. Of these the fixation reflex is ineradicable and primordial, nor could its elimination be desired, but it has potentialities for falsifying the estimate of a heterophoria, as will be seen later. It is not a reflex subserving the needs of binocular vision, except indirectly. The same assumption as was made in the case of the postural reflexes may, with equal justification and for the same reasons, be made about the fixation and the accommodation-convergence reflexes. In the monocular case neither gives rise to symptoms, and they are no more likely to do so in the binocular case. The fusion reflex, on the other hand, is prepotent in binocular vision. It is the only reflex which mediates between both retinae, which demands a stimulus from both eyes before it can affect either, which is, in short, binocular on the afferent side. The fusion reflex overwhelms the accommodation-convergence reflex and in so doing it subjects the link between these two functions to a degree of strain which varies with the refractive error of the eyes concerned.

The effect of the accommodation-convergence reflex is complex and, since it materially affects the results in emmetropes in near vision and in ametropes at all distances, requires careful consideration. It must be recognised, as a preliminary, that the accommodation-convergence link is very elastic (Berry 1912, Sheard 1920, Landolt in Norris and Oliver, Vol. IV, p. 3), and that in all cases where that elasticity is called into play there is a strain on the link caused by the demands of fusion. Considering first the case of the emmetrope who has basic orthophoria, let it be supposed that the normal perfect ratio between accommodation and convergence is one metre-angle per dioptré. In fact, it is generally accepted that the normal ratio is about $\frac{3}{4}$ metre-angle per dioptré (Jameson-Evans, 1922), but the only effect that this will have is that all results will be more divergent or less convergent than those calculated on the 1:1 ratio. The argument is unaffected whatever be truly the normal ratio. On the 1:1 ratio, the emmetrope's convergence exactly matches his accommodation and the effect of dissociation will be to reveal an absence of latent deviation, because the demands of fusion do not strain the link in his case. The hypermetrope, on the other hand, has always to put up less convergence than corresponds to his accommodation on the normal ratio, and while fusion holds sway he is tied to a new ratio in which x metre-angles of convergence correspond to 1 dioptré of accommodation. The numerical value of x is not constant, for it is always less than unity, but it approaches that value progressively the shorter the distance of fixation. The establishment of this habit leads directly to a perversion of the instinctive natural ratio,

and the effect of dissociation will now be that the proportion between convergence and accommodation will revert to some new ratio $x^1:1$ (the basic abnormal ratio), where x^1 is some value less than unity, but greater than x (cf. Hansen Grut, 1890). As it were, the elastic has taken on a permanent stretch, and is unable to revert to its previous length when the tension due to the demands of fusion is removed. If, now, the heterophoria be measured at infinity, an esophoria will usually be revealed whose amount will be less than that to be expected from his refractive error as calculated on the natural ratio, but more than the amount as calculated on the ratio $(x:1)$ which is habitual to him. It is probable that, although the figure x is not constant, the figure x^1 is substantially so. It is worthy of note that this perversion was included by Parinaud (1893) among the factors which oppose the re-establishment of binocular vision as "*L'altération plus ou moins définitive de l'innervation de convergence*" (also quoted by Hughes, 1907). Randall, too, noted this when, speaking of von Graefe's dot-and-line test for near vision, he said that it also tests the "*habitual relation between accommodation and convergence*" (Randall, 1890).

Consider, next, the effect in this same hypermetrope of providing the correction for his refractive error for the first time when an assessment of his heterophoria is to be made. No longer, at infinity, is any accommodation in play. The habitual partial inhibition of convergence in relation to accommodation is still active, with the result that to the esophoria previously found is added a relative divergence which may be of sufficient amount actually to convert the esophoria into an exophoria. It follows, therefore, that the effect of wearing a correction during heterophoria tests is likely to give equivocal, if not totally unexpected, results, if the correction has not been worn long enough to have restored the basic abnormal ratio $(x^1:1)$ to a normal value.

Similar reasoning applied to the case of the myope shows that an exophoria is to be expected, which is liable to be converted into an esophoria when the correction is worn for the first time. The correlation between esophoria and hypermetropia, and between exophoria and myopia, is noted by Pasetti (1911).

Turning, aside from the main argument, to the question of the deviations in near vision revealed by dissociation, it is easily seen that the deviation is the algebraic sum of various factors which may conflict with, or reinforce, each other. To the heterophoria at infinity is added the effect of the refractive error acting through the basic abnormal $(x^1:1)$ ratio between accommodation and convergence. Further, the proportion between the accommodation demanded by the distance of fixation and the total available accommodation will materially modify the resultant of the other factors.

The large proportion of people who are substantially orthophoric for infinity and do not deviate materially from emmetropia show, on dissociation by screening, a latent divergence for near vision. With decrease of the distance of fixation, the divergence naturally becomes more marked. But if such a person be screened while fixing at the punctum proximum of accommodation, the latent divergence will often become a latent convergence, because the maximal stimulus to accommodation is also maximal to convergence and the motor outcome of the reflex is therefore maximal. Loring (1868) pointed this out.

With so many factors determining the deviation for near, it would be inadvisable to use the terms compounded of “-phoria” for any near vision test. Logically, such terms should all be reserved for the latent deviations determined at infinity, when the patient has been rendered emmetropic for long enough to have recovered the normal accommodation-convergence ratio. These terms are, however, too deeply rooted in the clinical vocabulary to be susceptible of such restriction in general use, and, as the difficulties may most easily be surmounted by qualification of the terms, it is desirable that this convention should be established:—

(1) “Basic heterophoria” is that deviation latent in a patient who is tested at infinity and is either emmetropic or, if ametropic, wearing his correction and having worn it for sufficient time to have re-established a normal accommodation-convergence ratio. The adjective and the definition apply to all terms compounded of “-phoria.”

(2) Under all other conditions, the distance of the test must be specified, *e.g.*, “exophoria at 20 feet.”

Of this second statement it is pertinent to remark that the difference between basic heterophoria and heterophoria at infinity represents the amount of correction of muscle-balance errors to be obtained from wearing glasses. The amount of this available correction is not given, as Pasetti (1911) states, by the difference between the heterophoria found with and without atropine.

With these reflexes in mind, it is now possible to consider what is the physiological position of rest, whether this is the best term, and how to define it. As defined by Duke-Elder it is “the result . . . of a minimal and balanced tonus of all the muscles acting together” (Duke-Elder, Vol. I, p. 580). This implies the reduction to a minimum of all muscular activity arising from all or any of the reflexes mentioned above. It is to be noted, therefore, that it implies the exclusion of the fixation reflex, which would make its measurement substantially impossible by any test in which the patient retains the power of seeing. For in the primary position of the gaze, that is, “eyes front,” any patient with a heterophoria will have muscular activity above the minimum in the muscles of

the fixing eye. If the point of fixation be so moved, the head remaining still, that the image of this point falls on the macula of the eye in its position of minimal and balanced tonus, then the problem would be solved if it were possible to determine when the eye had reached that position of minimal and balanced tonus. Finally, if the fixation point be kept still and the head moved to secure the same position of the eye relative to the head, the previous objection holds with the added objection that the postural reflex from the neck-muscles is also in play. It follows that Duke-Elder has defined a position of rest which is nearly incapable of measurement and only potentially capable of realisation. This is not to say that the definition is not of value, but it is open to criticism from a practical point of view, and it suffers, of course, also from the same possibilities of variation as were indicated against Hansen Grut's definition in the section on the anatomical position of rest.

The realisation of Duke-Elder's physiological position of rest has been attempted by Fischer. He estimated in himself the position of minimal and balanced tonus purely introspectively, and considered that the constancy of the results which he obtained justified the assumption that the "tonic balance position," in which he got the impression that each eye tested separately was looking "apparently equally high" and "straight ahead," was very close to the anatomical position of rest (Fischer, 1922). While this is undoubtedly an attractive method, it is equally undoubtedly not practicable, except in laboratory work and in persons trained to introspection. It presupposes further that there are proprioceptive receptors in the ocular muscles, whose impulses can be brought up to the level of consciousness. Irvine and Ludvigh (1936) reached the conclusion that the "proprioceptive sense, if extant in the extra-ocular muscles, plays no part in projection, stereopsis, or the interpretation of motion on the retina."

Chavasse, speaking of the position of comparative rest (which term he uses as equivalent to the position of physiological rest), says "when the stimulation of the two eyes, and the activity of their central nervous connections, are thus greatly and *equally* reduced on the two sides, then this position of comparative rest differs only in degree from the position of absolute—dead—rest" (Chavasse, 1939). The approximation of the physiological position of rest, and the tonic balance position, to the anatomical position of rest removes much of their utility in the solution of the practical problem of treating heterophoria. Since muscular activity which is the outcome of the reflexes which are not purely binocular reflexes causes no symptoms, there is little immediate importance in measuring it. It is of more value to know or, more correctly, to infer from the deviation which appears the extent of

the effort which is called forth by the demands of binocular vision. The position, then, which it is desirable to know and to define is the position which the eyes take up relative to each other when binocular vision is not required, in fact, the "fusion-free position." Chavasse prefers the term "dissociated position," which has the advantage that it does not assume that fusion has been completely suspended. It is desirable to indicate the ideal in the choice of a term, and for that reason the term "fusion-free position" is to be preferred. It is to this position that the eyes tend

FIG. I
The factors determining the various positions of rest
and activity of the eyes

	Anatomical and Non-nervous Physiological Factors	Minimal Balanced Tonus of Muscles	Postural and Fixation Reflexes	Fusion Reflex	Accommoda- tion-Conver- gence Link (Normal or Abnormal)
Anatomical Position of Rest	X	O	O	O	O
Physiological Position of Rest	X	X	O	O	O
Fusion-free Position	X	X	X	O	(1) O
Functional Primary Direction = Binocular Fixation at Infinity	X	X	X	X	(2) O
Binocular Fixation at Nearer than Infinity	X	X	X	X	X

X=Present O=Absent

Note.—The diagram applies only to the emmetrope; in the case of ametropes, if uncorrected or if only corrected immediately before determining these positions, cells (1) and (2) should read X instead of O.

in the application of tests for heterophoria, provided always that the test does not itself stimulate muscular activity. It is implicit in the term "fusion-free position" that fusion be completely suspended, and it is obviously essential that the method of suspending fusion be specified in detail before the results obtained are comparable with the results of other investigators. The relations between the various positions of rest and activity are summarised in the diagram of Fig. I.

The physiological position of rest and the fusion-free position will reproduce more or less of any existing asymmetry of the two eyes which exists in the anatomical position of rest. It is, perhaps, reasonable to believe that such asymmetry may influence the determination of a "master eye." Crider (1935) has shown that there is a close correlation between the master eye as determined by sighting tests, and the eye that shows the less deviation behind a cover, maintains convergence better, and shows a more rapid recovery when fusion is again possible. Dolman's work showed that roughly 75 per cent. of a series of cases tested with the Maddox rod-screen test exhibited a larger deviation when the master eye fixed the spot of light (Dolman 1920a).

Section IV

From the previous section it has become clear that there are two positions of rest to one of which the eyes may tend in the course of any one of the multitudinous tests for heterophoria which have been devised. These are the physiological position of rest and the fusion-free position, and some tests will tend to elicit the one, some the other. Moreover, unless care be taken, there will be a tendency for the one position to slide towards the other. For instance, a test giving the fusion-free position when the visual axis of the fixing eye is in the primary position in relation to the head may give a result tending towards the physiological position of rest if the patient can get the fixing eye, still maintaining fixation, into the position which feels to him most easy, *i.e.*, into the position of minimal and balanced tonus. It follows that any test must be carried out with care to the ancillary factors which may influence the result, but it is difficult to agree entirely with the first part of Dunnington's dictum "the test employed is not so important as the manner in which it is done. Any muscle test to be of value must be done carefully" (Dunnington, 1931).

It is necessary to consider, in the light of the reflexes which govern the attainment of binocular vision, the possibility of falsifying the measurements. These possibilities will differ according to the position of rest which it is desired to obtain. If the physiological position of rest is desired, it is clear that each eye must be

measured separately, and the deviation of each visual axis must therefore be referred to some direction common to the two. The head must, therefore, be kept fixed during the whole of the testing of the two eyes (Howe, 1906, 1914). But the method employed to ensure this fixation must not be such as will, by creating a sense of strain, bring into play the postural reflexes from the neck muscles. For the same reason the head must be kept straight, not tilted in any direction nor twisted. The fixation point or, more accurately, the point which indicates the projection in space of the macula must also be freely movable for, if not, the fixation reflex is immediately brought into play. It is not surprising that no practicable method has yet been devised for determining the physiological position of rest: of which statement the corollary, that it is not the physiological position of rest which is shown by any existing test, is more important. Donders, by his method of using a spark, the patient being dark-adapted and differentiating the images by a red glass over one eye, has travelled furthest along the road to solving this problem. It should not be beyond the wit of man to devise a practical realisation of this, subject to certain precautions, theoretically perfect method.

Any method of measuring heterophoria—and this applies to all tests, whatever the position of rest which they tend to give—may be falsified if the two images do not both fall on the maculae of the respective eyes which perceive them. For if the image of any object, and in particular the image of the object to which attention is directed, falls off the macula, the fixation reflex is immediately called into play, and the more vigorously if the object stands out more clearly from the background. And this is no fusion reflex, but purely the result of the primordial desire to see, as clearly as possible, the most important object in the field of vision. It is a subconscious result, but it will be seen later that it may occur consciously in the case of such tests as the Maddox rod, if used with a tangent scale. It was to the effect of this reflex that Jackson (1893*b*) referred when, of tests involving diplopia produced by a prism, he spoke of the presence of a sensation of strain, continuing, “such diplopia we expect to correct by an effort directed to the ocular musculature, an effort that in childhood may be voluntary and conscious, but that after the establishment of binocular vision is maintained as a reflex.” There is the additional indirect effect of the fixation reflex in all diplopia tests which arises from the fact that a vertical diplopia is induced in order to measure a horizontal deviation. Bearing in mind Márquez’ diagram of the various actions of each muscle (Márquez, 1935), and van der Hoeve’s demonstration that in any movement the third muscle is necessary because the subsidiary forces of the paired muscles do not cancel out (van der Hoeve, 1932), it is easily seen that the

fixation reflex, in the presence of a vertical diplopia, may give rise to a horizontal deviation which is not truly present; in which direction the deviation will occur it may be impossible to say.

Turning to the other position of rest, the fusion-free position, it is clear that the movements tending to disturb the measurement are of two types, fusion movements and non-fusion movements. It is more convenient to deal with the latter first, and in this type there fall to be considered esophoria, exophoria, and hyperphoria, all relatively to the position of rest.

A relative esophoria is both easy to produce, and of frequent occurrence, and the factors causing its appearance have been noted by many writers. There is, first, the presence of a voluntary effort of convergence. Stevens has called attention to this importance of voluntary effort (in Norris and Oliver, Vol II, p. 170). Landolt says "but any object of observation . . . influences the direction of the two eyes even when it is seen only by one of them, and that not only by the effort of accommodation which the vision of this object demands, but even by the distance at which the individual supposes it to be, whether correctly or fallaciously" (in Norris and Oliver, Vol. IV, p. 144). Maddox (1886) puts the same idea more concisely as "the mental concept of nearness." Cass (in a private communication) speaks of the "reflex for the distance away of the object." It appears, therefore, that any method of suspending fusion, if it gives a false conception of the distance of the object, will give a measurement which errs, generally towards esophoria in the case of tests in the distance. The effects of the fixation reflex and the accommodation-convergence reflex in producing a relative esophoria have already been discussed.

The possibility, next, of producing a relative exophoria during muscle-balance tests, resolves itself, apart from the difficulties already examined, into the question of divergence as an active movement. This has already been discussed, and requires no further comment.

Artificial hyperphoria produced by the imposition of tests may, with the general remarks already made, and one further possibility (to be considered later) applying only to prism-diplopia tests, be dismissed with Hansen Grut's comment on active divergence, which is equally applicable here, "because the habit and practice of such a movement is altogether absent" (Hansen Grut, 1890).

The presence or absence of fusion movements, in contradistinction to the previous type, is a real clue to the efficiency of a test for heterophoria. If the visual acuity of the two eyes be high and substantially equal, relatively small differences, whether of form or colour, of the images seen by the two eyes may be suffi-

cient to ensure that the stimulus to fusion is effectively small. With Stevens' view one is forced violently to disagree, though "with the greatest respect"; he says "even the greatest contrasts of form or of character of the images impressed upon the two retinas (*sic*) do not cause a suppression of the fusion impulse" (Stevens, 1906). As the disparity between the visual acuity of the two eyes increases, so the stimulus to fusion of the two images will increase. Thus, an anisometrope, fixing a spot of light with a myopic eye, and having the image seen by his other, emmetropic, eye altered by Stevens' stenopaic lens, will receive on each retina a circle of diffusion, and the stimulus to fusion may be enormous. Similarly, a high degree of astigmatism in the eye which fixes a spot of light may so distort the image seen by that eye that there is a marked stimulus to fusion between it and the altered image seen by the eye behind a Maddox rod or a stenopaic lens.

Reverting to prism-diplopia tests, whether the image be displaced, or displaced and doubled, or displaced and quadrupled, the stimulus to fusion remains. Pasetti (1911) says of this, "nelli quali gli occhi sono dissociati da prismi non é interamente tolto lo stimolo a unire le due immagini, ma solo la possibilità di far questo" (in which the eyes are dissociated by prisms, the stimulus to unite the two images is not removed entirely, but only the possibility of doing so). Jackson (1893*b*) points out that, with a double prism but little stronger than the ocular muscles can overcome, the middle light will almost always be found to approach the upper or lower one even if there is no hyperphoria. In the case of Folinea's quadruple prism, a factor comes in which can by no means be neglected, but has apparently not been examined by any writer, and that is the mental desire for symmetry. The pattern suggested by five spots of light, of which four are already placed at the corners of a square, is so geometrical and tidy that the tendency to try to place the fifth spot in the centre of the square is the natural failing of the normal mind. The same objection applies, but in less degree, to any test which doubles the images, and this mental desire for symmetry is not absent in a test which merely displaces one image in a horizontal or vertical direction. Duane (1895) gave a hint of this difficulty in his statement " the impulse towards bringing two dissimilar images into line is almost as strong as when the images are alike." So also Pasetti, who says " mentre l'immagine nitida, solo simmetricamente spostata, dei due punti luminosi, cadendo su meridiani corrispondenti della retina tende, sotto la guida della sensazione, a far mettere ancora i due punti su una stessa retta" (while the bright image, only displaced symmetrically, of the two luminous points, falling on corresponding meridians of the retina, tends under the stimulus of perception to restore the two points to their

previous opposition). Zentmayer did not approve greatly of prism-diplopia tests, "as it had always seemed to him that the displacement of the image on the retina, which the phorometer produces, introduced a complicating factor" (see Duane, 1918), although he had not identified the factor to which he referred. Savage's method (Savage, 1891) of using two double prisms with the axis of one vertical and the other horizontal is liable to bring in the same difficulty of desire for symmetry, while to Roger's cone (mentioned by Maddox, 1912a) the same criticism applies.

Much has been written of the "fusion area" of the retina, and some writers have gone so far as to imply that all and any images falling within this fusion area will automatically stimulate fusion. Pasetti (1911) says, of Worth's lights, "Essendo le luci abbastanza tra loro, non vi è alcuno stimolo alla fusione" (The lights being sufficiently distant from one another, there is no stimulus to fusion). As recently as 1923 there occurs the statement, "It is generally conceded that, in order to obviate the tendency to fusion, the false image should be such and in such position that it will fall outside the fusion area, which, according to Savage, extends vertically 3° upward and downward, 8° nasalward, and 25° temporally" (Georg, Teren, and Lowell, 1923). Again Pasetti (1911) quoting Savage, says, "non estima affatto buono il cilindro di Maddox, perche dice che la striscia luminosa non cadendo tutta fuori dell' area di fusione, da sempre un eccitamento alla medesima" (he does not consider the Maddox rod really satisfactory, because he says that the luminous streak, not falling entirely outside the area of fusion, always gives a stimulus to the same, i.e., to fusion). Burian (1939) has investigated the effect of peripheral objects in stimulating fusion; his ingeniously devised apparatus leads him to conclude that there appears to be definite failure of central fusion when peripheral objects are so placed as to require relative deviation of the visual axes in order to superimpose their images, and he was able to obtain these fusion movements with an 0.5° object situated 12° from the fixation point. Thirty-four years before this, Stevens had pointed the way with his remark, "the images which fall on spatially non-corresponding points therefore serve an essential purpose. They serve as finders in the field of space" (Stevens, 1906).

The conclusion to be drawn from the above views is in strong support of the principle that has been discussed earlier in reviewing the effect of the fixation reflex. It is, briefly, that no test is free of suspicion in which both images do not fall on the maculae, when the eyes are in the fusion-free position.

One further suggested source of factitious error remains. Stevens (1891) states:—"I do not find there is any exception to the rule that, any prism for inducing diplopia or lens for causing

contrast in the size or form of the two images, when held close to the eye, will admit of a range of adjustments under a variety of circumstances which is extremely liable to lead to error." Baxter (1894), speaking of the phorometer, expressed the opposite view: "In measuring the strength of the ocular muscles, it is well to place the slide as near the eyes as convenient, as by removing the prism from before the eyes, the individual strength of the muscles is diminished, but the relative strength is not affected." Stevens put forward his views often on this point, and in his text-book he refers to the "strong tendency on the part of the eyes to neutralise the effect" (of a prism-induced diplopia) "by adjustments" when any instrument is held near the eye (Stevens, 1906). It is difficult to see which reflex comes into play with an instrument held near the eye which does not also play a part when the instrument is held further away, unless it be the postural reflex from the neck muscles due to the strain of an unwanted position. Care in the application of the test can avoid this, and Stevens' criticism then becomes void, having served its purpose of directing attention to the possibility.

Section V

Some notice has already been given to the difficulties of wearing glasses for tests for heterophoria. There has been a divergence of opinion in the past, even on this subject, although the consensus now is clearly in favour of doing so. Thus there can be found "the correction of the refractive anomalies by glasses during an examination of this kind is not only in the vast majority of cases unnecessary, but positively detrimental, as the glasses themselves often lead to error and their absence does not, as a rule, influence the muscular conditions" (Stevens, 1893). Probably the error introduced was largely due to incorrect centring of the lenses, of which Risley made great point (see Duane, 1918), and it is now well recognised.

Certainly it can now be said, bearing in mind the effect of wearing glasses for the first time, that no estimate of heterophoria can be accurate until the patient has been rendered effectively emmetropic, until, in fact, the normal accommodation-convergence ratio has been restored as well as the normal refraction.

Much has been written on the advantages of a cycloplegic during muscle-balance tests. Most of the American writers are in favour of its use; thus Howe (1914) refers "... to such useless experiments as others will make in future, unless they recognise the effect of the intra-ocular muscles in tests which are supposed to involve only the extra-ocular muscles." Risley emphasises the importance of a cycloplegic (see Duane, 1918), and at a meeting in

1900 Howe spoke of the value of a cycloplegic in removing the difference between the various tests (Howe, 1901). On the other hand, Marx (1908) is of the opinion that one need pay no attention in practice to the effect of the tension or relaxation of the intrinsic eye muscles in causing variations in the heterophoria. Guaita and Bardelli (1899) wrote of cocaine in tests for heterophoria, and begged several questions in declaring that one of its actions was "to develop the full *dégré* of heterophoria." Pasetti (1911) was a great believer in its value, whereas van de Heydt (1918) ruled against any cycloplegic.

Since, as pointed out before, the clinical aim of the diagnosis of heterophoria is to restore fully the normal binocular function, it is clear that a cycloplegic is at a disadvantage. Another point should not be overlooked, to which Bichelon's remark, though not of this, "*les classiques les passent sous silence*" (Bichelon, 1926) applies, and that is, that a cycloplegic does not remove the stimulus to accommodation, but only the motor outcome of that stimulus. The stimulus to accommodation remaining, either the patient may become conscious of its helplessness, and there will then be no corresponding convergence, or the reflex may produce a convergence as excessive as the accommodation is deficient. For this reason, too, a preliminary period of restoration to emmetropia is preferable to a cycloplegic in the estimation of ocular muscle-balance.

Unfortunately, the list of difficulties in the way of the measurement of a heterophoria is not exhausted by the fallacies that have been considered. Many writers have noted the extreme persistence, in time, of the fusion impulse. The full deviation does not appear immediately the ocular stimulus to fusion is removed. A clue to the explanation of the difficulty may be sought in this fashion. In a paper on the movement of after-images, Rexroad (1928) states "An object which gives rise to a definite pattern of visual stimuli would give rise to a definite pattern of tension in the recti and bodily musculature and other associated stimuli would become capable of eliciting this tension pattern so that the individual would seem to see the object in its absence." Now this is merely a conditioned reflex with a sensory, not a motor, outcome. The unconditioned reflex is the fusion reflex, requiring stimuli from both retinæ to attain fusion, while this reflex requires a stimulus only from one retina; the result is a partial persistence of the "fusion posture" in the dissociated eye. It has been said earlier that the fusion reflex is binocular on the afferent side, and the explanation suggested above involves the separation of a new, and younger, "monocular fusion reflex" conditioned upon the older, relatively unconditioned, "binocular" fusion reflex. An alternative explanation may be proposed, involving the introduction of

"memory" to override the tendency to disappearance of the fusion posture. The physiological explanation is always to be preferred to the psychological, but both are, of necessity, tentative. Whatever be the explanation, there is no doubt of the fact: and it was this fact which led Marlow to introduce his prolonged occlusion method. Bielschowsky (1938) has set out the whole difficulty: "A very important fact must be emphasized; namely, that the fusion innervation produced by adequate fusion stimuli does not relax immediately after stimulation has ceased. It decreases very slowly and a residue, manifesting itself as a corresponding phoria by suspending fusion, can be ascertained even after fusion stimuli producing the antagonistic innervation, by which the eyes are brought into the normal position, have been applied." Correspondingly only a part of the heterophoria appears when binocular vision is momentarily suspended in testing. Koyle (1905) says "a certain amount of latent heterophoria . . . will always become manifest if the phorometer is left long enough in position before the patient's eyes," but this is, of course, too sweeping since it excludes the possibility of orthophoria. "The only hint is to let the eye be covered long enough for the deviation to develop" (Maddox, 1929). The problem is what period of time constitutes "long enough." For clinical purposes the rule general in all scientific work holds, namely, to wait until two successive measurements are the same. Against this rule, there must be set the difficulty of binocular instability or "*incertitude dans la collaboration du regard des deux yeux*" (Cantonnet, 1930), for the cases are well known in which the patient seems constantly to be "changing his mind." However, the variations from this cause are small, and experience can usually minimise the error.

Of the variability of the deviation much has been written, and the variations are the source of a great deal of trouble. Care in the manner of applying the tests can reduce the variations and, fortunately, they are large only in the lateral deviations where the importance of exact measurement is much less. But a recognition of the sources of error, as discussed earlier, will materially assist the task of reducing them.

The effect of a previous forced position of the eyes in altering the estimate of a heterophoria has been considered by Bielschowsky (1938) who says that after wearing prisms for a time "as a rule, it takes fifteen to thirty minutes for orthophoria to return." Marx (1908) made a study of the effect of prisms, and found that the forced position of the eyes tended to remain so that the results were biased in the direction of the earlier forced position. He also showed that the effect of relaxing the accommodation beforehand was to produce an increase of exophoria. Furuya (1938) was unable to demonstrate any connection between variations in heterophoria

and fatigue, but Reijnders, in some investigations on aviators, found that oxygen-lack, fatigue, illness, and alcoholic abuse had each an appreciable effect on the heterophoria (Reijnders, 1922). In this work, the curious fact emerged that oxygen-lack biased the measurement towards esophoria, whereas the other factors caused an increase in any existing heterophoria. Howe (1912) showed that fatigue of the ocular muscles produced a "staircase" type of curve relating force of contraction to number of contractions performed. Strebel (1921) showed that emotion interfering with attention caused marked variation in the heterophoria. Maddox (1921) said "I have definitely proved my own slight esophoria to be increased by gastric irritation," and Thorne (1930) was more specific, "the extent to which this urge" (for fusion) "can be lowered varies in most individuals and in the same individual from day to day." The variability is also referred to by Benjamin (1928), Hansell (1891), Javal (see Berry, 1890), etc.

In face of all these possible causes of variation, it seems hopeless to try to establish the exact worth of any given measurement. Stevens held that "the manifest heterophoria is usually only a portion of the absolute heterophoria" (in Norris and Oliver, Vol. II, p. 181). Ballantyne (1922) is more dogmatic:—"It is certain that the measurements of degrees of heterophoria which we obtain in our routine clinical tests have no absolute value"; and again, Duane (1927) says "It has no definite assignable amount, and the maximum values of it . . . cannot be regarded as its true measurement." It seems from the embattled authorities that there is little chance of extracting any reliable information from a given measurement. But this is a pessimistic view and, with care to the auxiliary factors, the variations can be sufficiently reduced to make each measurement near enough to the mean of all to be of clinical value, and probably also of theoretical value.

Section VI

Apart from the clinical purpose of measurements of heterophoria, their value in another direction is of great importance. All activities which require stereopsis are handicapped by the existence of a marked heterophoria. Outstandingly in aviation is this the case where the effect of fatigue in rendering a heterophoria manifest, *i.e.*, in converting a heterophoria into a heterotropia, occurs at the time when stereopsis is required at its highest pitch of accuracy, namely, when landing an aircraft. That stereopsis, sufficient for flying purposes, is not wholly dependent on binocular vision is well shown by the monocular pilot, but the importance of heterophoria in aviation arises from the two facts that the prospective pilot with a large heterophoria is much less easily able to

acquire a judgment of distance in the third dimension, and that the temporary loss of binocular vision leaves him at a greater disadvantage than the monocular pilot because of the absence of training in monocular stereopsis. Livingston (1932) writes of many instances "which show how an unconscious alteration in ocular muscle balance may produce gross errors in visual judgment." Clements (1924) describes how failure to land correctly was found to be "mainly due to faulty judgment of speed and distance which was associated with lack of ocular muscle balance and with poor binocular vision." It is not universally agreed that heterophoria is responsible for these "gross errors." Thus, Brailey (1919*b*) was by no means satisfied that the theory and the results were not both fallacious, in spite of the statistical evidence and he refers to some work of his with Constantine which showed that faults of muscle-balance were equally common in good and bad pilots. Reijnders' work showed that there was a close correlation between stereopsis as tested by binocular parallax methods, and heterophoria: the fault in stereopsis, was shown to be usually that theoretically expected from the type of heterophoria (Reijnders, 1922). Livingston (1934), however, says that the correlation between landing difficulties and poor results in binocular vision tests is much higher for near vision than for distant vision tests, but the inescapable conclusion is to wonder whether the distant vision tests were efficient and in particular whether they were able to pick out all the cases of a large heterophoria. It will be shown later that the "red-green" test, for which on many occasions Livingston has signified his preference (*e.g.*, Livingston, 1934, 1939, 1940), will give, in a number of cases, only a small heterophoria when gross errors are revealed by other tests. It is obviously desirable, for research purposes, to establish tests which determine a single clinical property of the patient (such as the basic heterophoria) in preference to tests which assess the resultant of several factors acting together (such as is given by the "cover test for near"). Only by the isolation of the ocular properties which may affect stereopsis or flying fitness or both is it possible to determine which properties do affect these faculties. Recently, the complaint was made that the results of physical fitness tests bore little correlation with the practical assessment of flying aptitude in pupils (Whittingham, 1940). The remedy lies in the isolation of the individual factors, so that to each may be given its due weight.

It is interesting to note the only two claims which have been made for the red-green test. The Air Ministry handbook of medical flying tests says of it "any degree of latent squint will be revealed by a deviation of the coloured lights (Air Publication 130). This will be shown later to err so far in the direction of

"wishful thinking" as to secure for the other claim a charitable reception. Clements (1926) describes the red-green test as giving "an indication of uniform delivery of centrifugal impulses received," to which the late Lord Chief Justice's comment "now this means, if indeed it means anything" is applicable at least as fairly as to its original target. While progress in knowledge is bound to create, and is materially helped by, controversy, it is desirable that the controversy should be carried out in language designed to elucidate, rather than to conceal, the meaning of the statements made.

Section VII

The causation of heterophoria, in non-pathological cases, has already been substantially covered in preceding sections. It is generally regarded as being normal and a "constant accompaniment of binocular single vision" (Fox, 1923). It is the resultant of anatomical and physiological factors, both nervous and non-nervous. Responsibility for a large variety of pathological conditions has been ascribed to heterophoria by Smith (1907). A suggestion was also put forward by Broome (1919), which is mainly interesting for the curiosity of its inversion of cause and effect. He concludes "heterophoria is a comparatively rare condition depending usually on interference with the ease of obtaining coincidence of the images of the two eyes" (and occasionally, he adds, on muscle or nerve defects) "and represents a tendency to failure of binocular vision."

With the rarity of heterophoria most writers disagree, whatever the test which was used for the discovery of the condition. The figures given differ widely. Stilling (1885) says that orthophoria is the exception, Hansen Grut (1890) that esophoria is relatively rare. Various results for orthophoria are given in Fig. II in the form of a table. If the figures for hyperphoria are taken, there is also a marked variation. Marlow (1936) quotes Hansell and Reber's figures as 20 per cent. of 700 cases showing 0.5° hyperphoria or more, and by prolonged occlusion raises the figure in his own series of 700 cases to 84 per cent. By prolonged occlusion of each eye in turn in 82 cases he nearly gets a "highest possible score," finding hyperphoria of the same degree in 98 per cent. of cases! (*ibid*).

Turning to near vision tests, Young (1926a) found exophoria at 13 inches of 8 prism-dioptres (about 4.6°) or more in 18.45 per cent. of 905 cases, while Sheard (1920) found that 70 per cent. of normal hypermetropes (having less than 2 prism-dioptres esophoria or exophoria for distance) showed between 2 and 7 prism-dioptres of exophoria at 13 inches.

There is clearly room for more work of a statistical nature on these points, but such work will be largely rendered nugatory by any failure to standardise the conditions, and the tests, on the lines already discussed.

FIG. II
Proportion of orthophoria found by various investigators.

Authority	No. of Cases	Test(s)	Limits		Percentage
			Maximum Esophoria	Maximum Exophoria	
Thorne 1930	500	Maddox Rod with Screen	2°	1°	51·2
Marlow 1930	700	?	0	0	22
Bissell 1892	70	Maddox Rod and Prism Diplopia	0	0	25·7
Marx 1908	70	Maddox Rod and Prism Diplopia	0	0	12
			1°	1°	42
Reijnders 1922	?	?	0	0	38·4
Clements 1929	277	Red-Green	0	0	39·3
Marlow 1915	?	?	0	0	31·5
Martin 1937	2000	?	0	0	26
Thomson 1924 (figures of Lempp and Rabinowitsch)	797	Maddox Rod	0	0	25·25

Section VIII

It is convenient at this stage to summarise, of necessity dogmatically, the conclusions reached in the previous sections.

(1) There are four positions of rest of the eyes; each is the result of additional factors operating upon its predecessor.

(2) The anatomical position of rest is the resultant of all anatomical and non-nervous physiological factors and, in the absence of secondary pathological changes, it is, invariably, divergence.

(3) In the physiological position of rest is added the effect of minimal, balanced, tonus of all the extra-ocular muscles. It is closely akin to the anatomical position of rest.

(4) The physiological position of rest is changed into the fusion-free position by the addition of the effects of the postural and fixation reflexes.

(5) The functional primary direction is visual parallelism.

(6) According to the test employed, and the care given to the ancillary conditions, the position of rest revealed by any test for heterophoria may be the fusion-free position, or some position intermediate between this and the physiological position of rest, or any perversion of either of these. The physiological position of rest is not obtained by any practicable test yet devised.

(7) Basic heterophoria is the latent tendency of the eyes in vision at infinity to deviate from the functional primary direction, in the presence of actual or effective emmetropia.

(8) The criteria of efficiency of a test for heterophoria depend on the position of rest which it is desired to obtain. Since, for most practical purposes, the fusion-free position is required, the fusion reflex is to be excluded, and the postural and fixation reflexes are to be stimulated in such ways only as will not cause a false deviation of the visual axes.

(9) There are many means of so stimulating the postural or fixation reflexes or both as to cause a deviation which vitiates the measurement of a heterophoria. The majority of tests are open to damaging criticism on these grounds.

(10) The wearing of glasses for a test is insufficient, for they must have been worn previously for long enough to have re-established a normal accommodation-convergence ratio. With this precaution the use of a cycloplegic is unnecessary.

(11) The deviation must be given time to develop; generally speaking, enough time has been given when two successive measurements are the same.

(12) The range of variations of the deviation can, with care, be made small enough to render a single measurement significant.

It is now possible to examine the details of the ideal test for determining heterophoria. The preliminaries have already been discussed. The patient must be, or have been rendered by glasses, emmetropic, and must have been in that state for long enough to possess a normal accommodation-convergence ratio. He must be in a normal state of physical fitness, since fatigue, illness, etc., will materially affect the result, and the test itself must involve neither apparatus so overwhelming as to preclude effective relaxation nor conditions so dull as to blanket his attention. In fact, the ideal is a patient in a state of physical and mental ease.

For all tests which are to give the fusion-free position, the exact fixation of head and body is unnecessary, but both require to be substantially in the primary position, that is, the natural, erect and "eyes front," posture. The application of the test must not disturb either head or body and even if not disturbing them, must not create a sense of strain in the maintenance of the posture. These standards demand, in fact, an instrument no more upsetting to the patient than a trial frame, and as freely movable. The fixation point must be capable of securing fixation in any direction of gaze, the head and body remaining in the primary position. This point is of great importance for, whereas in looking to the left the head and body turn to the same side, the testing of a heterophoria in a direction of gaze to the left involves turning the head to the opposite side, an unusual combination of movements.

The test employed must be such as does not stimulate the fixation reflex. However, this is not to say that it may not leave the image on the macula, but only that, if the image were suddenly presented to an eye already in the fusion-free position, it must not incite any movement of the eye away from that position. Nor must it create any false impression of the distance away of the fixation object, whence it follows that the fixation object must be sufficiently clearly seen, at least by one eye, as to enable the patient to orient his perception of it in relation to himself. Maddox (1920) says "I have always advised the room to be only partially darkened," and, that it is "best used in a room . . . with enough light to secure accommodation" (Maddox, 1921). Had he said "orientation" or "space-perception" the advice would be true to-day, for accommodation is to be avoided, in all distant vision tests. The background of the fixation object must be free of all marks, lines, figures, etc. (Stevens, 1906, *et al.*) which could confuse the patient's understanding of the test and, more important, which might sufficiently resemble either visual image to cause an impulse to fusion. Finally, the apparatus used must be such as will enable the conditions of the test to be reproduced exactly by other observers, or by the same observer on subsequent occasions. The only possible variable must, in fact, be the quantity to be

measured, for there is no place for Relativity in the measurement of heterophoria.

Of the means of measuring a deviation produced under the conditions laid down above, two are available. In the first, which involves the use of prisms, the false image is so transferred as to fall on the retinal meridian corresponding to that in the other eye on which the true image falls, and the patient thus sees what he would do if he were orthophoric. The second means requires the patient to read the deviation on a scale, and of this there are two varieties, depending on whether the scale is to be read by the eye which is seeing the fixation object, or by the other eye. In a test where, for example, an eye receives a distorted image, the other eye is required both to fix and to read a scale. Should a heterophoria exist, the part of the scale on which the distorted image falls cannot have its image received on the macula of the fixing eye, on which the image of the fixation-point already falls. The important section of the scale is therefore seen by a part of the retina with a lower visual acuity than the macula, and the direction of attention to this part of the scale immediately brings into play the fixation reflex, with the consequent vitiation of the measurement. To other tests, where the one eye fixes a single point, and the patient is asked where this point appears to be on the scale seen by the other eye, the same objection does not apply, for here the important part of the scale falls on the macula of the non-fixing eye. Practical experience, confirmed by statistical investigation, bears out this apparently theoretical criticism of the first variety.

To be concluded.

ANGIOMATOSIS RETINAE

**With a report of four cases in one family
involving six eyes**

BY

L. STAZ

JOHANNESBURG, SOUTH AFRICA

THE first clear description of this condition was given by Fuchs in 1882.

D. J. Wood (1892), later of Cape Town, exhibited a coloured drawing of Mr. Tweedy's case to the Ophthalmological Society of the United Kingdom in 1891. The drawing is reproduced in Vol. XII of the Transactions of the Ophthalmological Society. It is of the right eye, and shows very peculiar enlargement of some of the

NOTES

North of England Ophthalmological Society AT the Annual General Meeting of the North of England Ophthalmological Society held at Sheffield on March 29, 1941, the following officers were elected:—*President*, H. Haward Bywater (Liverpool); *Vice-President*, C. M. Geddie (Blackburn); *Members of Council*, John Foster (Leeds) and A. B. Nutt (Sheffield); *Treasurer*, W. H. Kiep (Bradford); *Secretary*, Percival J. Hay (Sheffield).

* * * *

Glasgow University, Department of Ophthalmology SIX meetings will be held in the Tennent Memorial Institute, 38, Church Street, on Wednesday evenings, April 16 to May 21, the hour of meeting being 8 p.m.

The proceedings will be quite informal—an introductory Demonstration or Paper to be followed by tea and discussion.

The speakers and subjects will be as follows:—April 16, Professor A. J. Ballantyne—"Some questions concerning the retinal vessels and retinal haemorrhages"; April 23, Dr. John Marshall—"Some unsolved problems regarding endophthalmitis"; April 30, Professor A. Loewenstein—"The rôle of lipoids in ocular pathology"; May 7, Dr. W. J. B. Riddell—"Medicine, Linkage and Ophthalmology"; May 14, Dr. A. Garrow—"Ocular palsies"; May 21, Dr. J. A. Conway—"Eye injuries in the recent air raids."

An invitation is extended to Ophthalmic Surgeons and to others interested in ophthalmic problems.

* * * *

Corrigendum In Mr. Nigel Cridland's paper in our last issue on p. 141, third line from the bottom of the page, "Nothing" should read Nothung. On p. 159, line 9, "van der Heydt" should read "von der Heydt." On p. 162, line 36, "proporties" should of course be "properties." On p. 162, last line but two, quotation marks should be added after the word "lights." We regret these errors in proof-reading.

FUTURE ARRANGEMENTS

1941

May 1-3, 1941.—Irish Ophthalmological Society, Annual Meeting in Dublin.