EXPERIMENTS IN SPACE PERCEPTION. (II.)

BY DR. JAMES H. HYSLOP,

Columbia College.

There is another experiment which throws farther light upon the question of magnitude and its perception as discussed in the previous article. It is illustrated in Fig. 4. Draw the circles *A* and *B* and *C* and *D* on separate cards, so that one of them can be held above the other and at a greater distance from the eyes. Also draw *C* and *D* farther from the median line or plane *MN*, so that when placed at a certain distance farther from the eyes than *A* and *B*, the planes *LY* and *OY* may pass respectively through the centres of *C* and *A* and *D* and *B*, and terminate in the retinas of the eyes after crossing, *E* being the point of fixation. This enables the same degree of convergence to combine *C* and *D* at the same time that *A* and *B* are combined. When this is done the effect is as follows. The central circle, which is the fusion of *C* and *D*, appears to be much smaller than that of *A* and *B*. Now as the retinal image cast by *C* and *D*, they being farther from the eyes, is smaller than that of *A* and *B* (the circles being of the same size), we might say that the phenomenon is due wholly to that fact. But as previous experiments have shown, if the centres of *C* and *D* are at the points *H* and *I* respectively, the central circle *does not* seem any smaller than the central image of *A* and *B*. Measurement shows the fact. This cannot be determined easily by introspection, because under the circumstances the fusion of *C* and *D* does not take place at the same time as that of *A* and *B*. But the comparison by memory, which is that of two successive seconds or moments, makes them appear the same. However, as this is exposed to error, we can rely upon the measurement mentioned above, and that, as we have seen, makes the central circle the same in magnitude for all degrees of convergence. Now if the dimin-

ished magnitude of *C* and *D* under convergence, when their

FIG. 4.

centres are at *L* and *O*, were due wholly to the diminution of the retinal image owing to their greater distance from the

eyes compared with *A* and *B*, then the same diminution ought to take place when they are at the points *H* and *I* respectively, because these points are as far from the eyes as *L* and *O*, and the retinal images must be the same.

But before referring to the possible explanation of this result we should indicate also a probable reason for the constancy of the apparent magnitude of the central circle in all degrees of convergence, a fact which would appear anomalous if we suspected that the diminution of magnitude in the first place was due to the degree of convergence. But if we note that the angle of convergence *FEG* for the simultaneous combination of *C* and *D*, and *A* and *B* is greater than that for the fusion of *C* and *D* at the points *H* and *I* respectively, and then remember at the same time both that the retinal impressions of *C* and *D* are smaller in proportion to their distance from the eyes than those of *A* and *B*, and that by hypothesis associative accommodation may coincide with the lessened convergence for *H* and *I*, we can imagine that this proportional decrease of convergence and retinal magnitude might exactly compensate for the difference of distance and for the enlarging influence of ciliary expansion or relaxation, associated with diminished adjustment. That is to say, other things being equal, decrease of convergence is, by hypothesis, connected with increase of retinal magnitude due to ciliary relaxation, habit and association having made the two functions tolerably cohesive, and hence with the approach or recession of the circles the changes of convergence might be accompanied by constancy in the apparent magnitude of the circles. But, of course, the difference between the magnitude of the fused images of *C* and *D* at *L* and *O*, and that of *A* and *B* at *F* and *G*, can be accounted for by the difference of retinal magnitude, all other things being equal in the case. This is proved by drawing circles on another card at *L* and *O* just large enough to cast the same retinal images as *A* and *B*. This can be calculated quite accurately. If then we combine them at the same time as we combine *A* and *B*, we shall find the apparent magnitude of both central circles *exactly the same*. This would seem to justify the conclusion that, retinal magnitude remaining the same, the effect varies with convergence and combination, and

it remains only to determine whether the modification of magnitude is due to convergent or to ciliary influences, or to both of them together.

That merely retinal magnitude, so far as it is determined by the distance of the circles from the eyes, has nothing to do with the effect is amply proved by placing the larger circles, or the card containing them, at *J* and *K*, and the smaller circles at *H* and *I*. If under these conditions we combine, it will be found that the magnitude of the central circles may still be different, but the reverse of what they were in the first instance. The larger circles under fusion appear smaller than the combination of the smaller. The trouble here, however, is that the combination for both sets cannot be effected at the same time, and we have to compare a memory impression in one case with a real impression in the other. But the lapse of time is too brief to attach any weight to this objection, and besides the difference of apparent magnitude is too great in the case to make illusion possible, as every one who can perform the experiment will see. Moreover there are two objective proofs of the fact which make it unnecessary to rely upon subjective impressions as evidence. In the first place, measurement, such as has already been described, shows so marked a difference of magnitude that illusion cannot be supposed without assuming that a man cannot tell the difference between a twenty-five- and a fifty-cent piece. In the second place, we have already learned that the magnitude of the central circle in any case remains constant with the approach or recession of the figures from the eyes. In this way we can make a more or less direct comparison of images at the same time, and the result confirms all that has been described. Hence retinal magnitude may be different, so far as distance is concerned, and the effect the same, or the retinal magnitude of one image smaller than another and yet its apparent visual magnitude greater than the other. We might infer from this that the whole effect of modified magnitude was due to convergence alone, because, in spite of the smaller circles at the greater distance from the eyes, the combination of the larger circles at *J* and *K* appears smaller than that of the others.

The convergence for the larger circles is greater than that for the smaller and the magnitude is correspondingly diminished.

But there is a fatal objection to this hasty inference. We have not eliminated possible ciliary influences and the modification of the lens. By hypothesis ciliary and convergent functions are so cohesive as to act in harmony, and here, noting that the convergence is less for the remoter and smaller circles, ciliary contraction would be less, so that the effect coincides with the possible variation in the associated action of the optical lens to modify the retinal magnitude independently of distance. If this be so, we should find an actual change of retinal magnitude to compensate for the supposed differences due to distance from the eyes, and construction of the circles. But it would be a change which we should have to account for by the associative cohesion of ciliary with convergent influences, and not by reflex accommodation to distance. Hence, though the experiment may show that the effect is independent of ordinary retinal magnitude, it does not seem to eliminate that factor from the problem, because associative ciliary action may influence changes of magnitude which we may think to have eliminated.

But there are facts which strongly contradict the supposition of ciliary influence in this case as before. The first of these is one that we have already mentioned; namely, the fact that no such modification of magnitude accompanies monocular perception when one eye is covered or closed and artificial convergence effected. Under the conditions described, whatever appearances of change take place, they are purely proportional, though we should be obliged to suppose associated ciliary action along with the changes of adjustment, if that action ever takes places at all when convergence is effected. But in this case and in spite of convergence and the supposed ciliary changes the differences of magnitude are not observed. But a second experiment is still more conclusive. It is represented in the circles of Fig. 5. I draw three circles *A*, *B*, and *C*, *A* and *B* being farther apart than *B* and *C*, but in the same plane, so that combination of *A* and *B* will require greater convergence than that of *B* and *C*. Now since these circles are of the same size and lie in the same plane their retinal magnitude

will be the same. But if I cross the eyes until *B* and *C* combine, I observe two effects: first, the apparent fusion of *A* and *B*; and second, a greater diminution of magnitude in the apparent fusion of *A* and *B* than that of *B* and *C*. If I combine by negative convergence, enlargement takes place and the fusion of *A* and *B* appears larger than the apparently simultaneous fusion of *B* and *C*. Now we cannot suppose that the differences here remarked are due to differences of retinal magnitude without supposing that two very different degrees of accommodation occur simultaneously with a given degree of convergence. This must be absurd upon any theory of neural mechanics. The same absurdity is manifest if we suppose dif-

FIG. 5.

ferent degrees of simultaneous convergent tension, and with it different degrees of accommodation. The only other theory of the case that can commend itself is that of a muscle continuum which has no resemblance or analogies whatever with the ordinary theory of muscular tension, based upon the conception of mechanics. Such a view, however, only retains the motor theory in name and not in reality, and we can only wonder at the self-deception and disingenuousness that led to its adoption and the retention of all implications belonging to the muscular sensation-theory. Not that we mean to present any *a priori* objection to the general conception of such a theory, but only that it should be called a doctrine of a muscle continuum while imagining that motor implications were involved and sensory denied. However this may be, it is clear that ciliary influence and its presumably consequent modification of retinal magnitude are not available to account for the alteration of apparent magnitude in the objects in the case mentioned without supposing different degrees of accommodation simultaneously, and that, I judge, no one is willing to do. But if we

thus exclude ciliary action from the effect in Fig. 5 we have no reason to suppose that it will explain the same phenomenon in Fig. 4, and we find that the only definite generalization to be formed in the case is that apparent magnitude is somehow connected with the degree of convergence necessary to produce fusion, whether we choose to regard the influence as motor or sensory. A more complete generalization may be forthcoming in the sequel of the present discussion, after we have examined some of our experiments in the perception of distance.

There is a very pretty, and perhaps conclusive, experiment showing a modification of apparent magnitude when we *know* retinal magnitude has not been altered, whatever connection we assign between accommodation and convergence. It is in after-images, and one effect in it is quite uniform. For instance, I have often tried the experiment with pictures hanging on the wall. The attempt was to cast the after-image upon some other part of the wall nearer or remoter than the picture itself. What I have uniformly noticed is a diminution of magnitude or increase of it according as I look at the nearer or remoter point, after gazing at the picture for a moment. That is, if I throw the after-image on the ceiling, which may be farther off than the picture, as it was in the experiment here described, it appeared larger than the object; if I throw it on the wall near me, fixing my eyes upon the wall, the figure seems smaller than the original. It is to be noted also, that where I can hold the after-image for some time, which happens whenever certain neural conditions favor extreme sensibility, the image seems located at the point of fixation. When the after-image lasts but for a second this translocation is not so evident or clear, as the change of adjustment seems to suppress it. But by quick changes of positive or negative convergence I can even then notice a corresponding difference of locality and magnitude. However, when the image persists for fifteen or twenty seconds, or sometimes for nearly a minute, both the localization and modified visual magnitude are perfectly distinct. Now here is a case where we cannot plead the alteration of retinal magnitude, due to the coexistence of a certain fixed degree of accommodation with a given degree of convergence. Even if

this occurs in such cases it cannot alter the retinal magnitude of the impression, and yet we find the apparent magnitude varying with the degree of convergence while retinal magnitude remains absolutely constant. It will be apparent, therefore, that if we thus exclude the influence of ciliary contraction and expansion from the effect, and with it all fixed connection between retinal and apparent magnitude, in this important case, there will be no reason to suppose them in other cases where it is not so easy to eliminate them, and we are left only with the connection between convergence and apparent magnitude, as the only uniformity as yet unanalyzed.

There is one other interesting fact in connection with this experiment which bears upon the perception of solidity, and which should be noted before passing to that question. It is, that in spite of the influence of binocular changes of adjustment to translocate the after-image to the point of fixation, it does not alter its dimensional relations to the plane of vision. Thus, if the picture hang obliquely on the wall, say thirty degrees, more or less, and I look at it while lying on a bed or lounge and then look at the wall vertically near me, besides the diminished magnitude and translocation I notice that the image *does not lie in the plane of the wall, but in the same position relatively to the plane of vision as in its real position*. The top of the picture, which is some thirty degrees from the wall, now seems resting on the wall and the bottom of it seems much nearer. The angle at which it, the after-image, intersects the line of vision remains the same as before, and in the same relative position. At a certain point on the ceiling the image will seem to lie in its plane. This is when the plane of the ceiling and of vision coincide. In all other positions the plane of the after-image and the ceiling intersect. I get the same effects from whatever position or attitude I look at the picture, and the same is true of any object from which I can succeed in getting persistent after-images. Here is a case where solidity, or the third dimension, seems independent of motor functions, even though we explain translocation and modified magnitude by them. They may modify localization without modifying the dimensional relations of the object.

But before taking up the subject of distance it will be inter-

esting to use the experiments already described to explain a phenomenon which has generally been attributed to memory and association. We may also find in it a confirmation of the suspicion that ciliary influence or accommodation may not be involved in the modification of retinal and therefore of apparent magnitude, at least to the extent required by the illustrations described. I allude to the phenomenon of the judgment of size at great distances. Physiologists and psychologists were once puzzled by the fact that an object, say a man, a horse, a wagon, or a house always seems as large at the distance of a thousand feet or more as at a distance of five hundred feet, though it was assumed that the retinal magnitude is quite different in each case. Supposing that real magnitude depends on retinal magnitude, this apparent constancy of our impression about size without regard to distance seemed to be quite an anomaly, and hence association with past experience was the resort to explain the phenomenon. But whatever influence may be attributed to association, it certainly cannot explain the constancy of apparent magnitude with the varying degrees of convergence in the experiments above described, while this latter fact throws much light on the normal impressions of mankind as containing natural and functional elements not admitted by the associational theory. It represents exactly the strong feeling we have in ordinary experience that the phenomenon is less analyzable those we find connected with association. But not to urge the force of a feeling merely, this actual constancy of apparent magnitude with the alterations of retinal magnitude and convergence, unless we assume that changes of accommodation overcome or compensate for the former, is evidence of functional arrangements for the judgment of real magnitude. But a fact in connection with the whole phenomenon of such judgments reflects upon the hypothesis of ciliary influence as affecting a modification of retinal magnitude sufficient to compensate for the effect of actual distance in diminishing it or increasing it, as the case may be. It is that beyond the parallel position of the eyes all changes of accommodation, if any, must be so independent of convergence as to cast a doubt upon the fixity of their relation within those limits. If that connection is not a firm and inva-

riable one, there ought to be some variation in the perception of magnitude with any given degree of convergence, which I do not discover. In my own case, however, it is certain that this connection between accommodation and convergence is not a fixed one, while the relation between convergence and apparent magnitude is fixed. Thus for any degree of positive convergence the perception of outline is as clear as in the normal position of the eyes. *But this is true only for the fusion of similar images.* Before the combination of the images they are slightly blurred or indistinct, and if they are different in nature, say a black and a red pencil, they are blurred even in fusion. Again, in negative convergence, as remarked earlier in the discussion, the blurring is constant until the plane on which the figures are placed is located about eighteen or twenty inches from the eyes. For this and all greater distances the images are clear. I notice in all this blurring, which is undoubtedly caused by defective accommodation, that the apparent magnitude is not affected in any way to reflect ciliary influences, and as the accommodation does not assume a definite degree until fusion of similar impressions occurs, while localization and magnitude are proportioned to convergence, there is every reason to discount the associative agency of ciliary influences and to suppose other structural functions, perhaps sensory, to account for the phenomena described, unless motor influences in connection with convergence can be invoked. Further confirmation of the unimportance, if not the absence, of association, and of the independence of convergence and accommodation, is found in the fact alluded to that blurring occurs if I close one eye and then converge while looking at an object, which shows that there is no absolute fixity of relation between the two functions as long as distinctness occurs only when fusion is effected. But if accommodation varied uniformly with convergence there should be equal clearness for all degrees of it, which is not the case except when fusion takes place. Hence I must conclude against the probability that ciliary influence determines either magnitude or localization, while the facts point to other structural functions than the purely motor to account for the effect and to explain the normal judgment of magnitude when the retinal image varies

with distance. That is to say, some other function than association and accommodation is necessary to account for the apparent constancy of magnitude in spite of distance and diminished size in the retinal image.

I come next to some experiments on the perception of distance or solidity, or, perhaps better, localization in the third dimension. They will confirm the conclusion just mentioned, and throw some light on the general function of space perception. The first illustration will involve, presumptively at least, nothing but binocular processes. If I draw four circles as represented in Fig. 6, with the centres of the

FIG. 6.

two smaller circles farther from the median plane than those of the larger, and then cross the eyes until fusion occurs, I see the usual frustum of a cylindrical cone, with the smaller base nearer the eyes. If I focus the eyes beyond the paper, negative convergence, I get the same effect, except that the relative position of the basis is reversed. The larger end now appears nearer the eyes, and the smaller end farther away. The same alteration of magnitude, in both circles, occurs as it has already been described. But these are not the phenomena to which I call special attention. The fact of apparent solidity is perfectly familiar to all observers. But I have referred to it as preliminary to the mention of certain other incidents not so generally considered, some of which I have not seen in the accounts of any other experimenter. I shall not dwell upon the relation of the circles to the median plane as a determinant of this localization in the frustum, but only upon the facts that the frustum itself seems located at the point or points of real or apparent fusion, and that the length of the frustum varies

with the distance of the paper from the eyes, and with the construction of the figures.

In the first place, if I employ positive convergence the localization is at the point of fusion or fixation between the paper and the eyes; if negative convergence it is beyond the paper at the point of fixation. These facts I determine by placing a pencil or some pointed instrument where the figure appears to be. I do not use the point to effect convergence, but after this is done I can point instantly, as in normal vision, to the places where the two bases of the frustum appear, and these are invariably as described. To do this accurately for negative convergence the circles should be drawn upon a piece of glass, and when so done I have no difficulty in indicating the true apparent position. The same is true of all objects or figures combined by binocular adjustment. A screen, for instance, always appears distinctly to be at the point of fixation nearer or beyond the real place according as the convergence is positive or negative, and my eyes can wander all over it without disturbing the illusion any more than most persons can modify the position of objects at will in normal vision. This is so universally true that I could formulate as a law that all fused images are located in the horopter, no matter what their real position in space or their distance from the eyes. At least this is true in my own experience, though subject to qualification from the disturbance of monocular functions, which varies with the nature of the figures or objects combined, being less where the resemblance or identity is great, and greater where differences of surface are marked or distinct.

I have been able to obtain an approximate measure for the length of the frustum of a cone which I see. For negative convergence this is impossible unless the circles are drawn upon glass, and even then there is the difficulty of attaining complete accuracy, because putting the pointer at the right place is not easy for the reason that it is too far away to be sure either of its exact coincidence with the visible line of the figure, or of the absence of double images. But it is quite accurate enough for practical purposes. Then there is also a limit to which even the attempt to measure the frustum can

be made. But nevertheless the measurement can be tolerably accurate for a short distance in negative convergence, and quite perfectly so for the whole of positive convergence. This is effected in the following manner. I take a sharpened instrument, like a pencil, and place it at the point where I see one base of the frustum. This is marked by another person, and I then indicate in the same way the position of the other base. The distance between the two points is then measured. It can be quite accurate, but I have not attempted to get the most accurate results possible, but only such as give something better than my subjective judgment, and the figures will show very suggestive differences under the varying conditions. I hope that those who are devoting themselves to accurate measurements in such cases may take the matter up and investigate it more thoroughly than I have either the time or the instruments to accomplish.

Taking the circles of Fig. 6, which are drawn respectively from fifty- and ten cent silver pieces, and combining them, at a distance one foot the length of the frustum measures $\frac{1}{2}$ an inch, at two feet it is $\frac{2}{3}$ of an inch, at three feet 1 inch, and at four feet $1\frac{1}{2}$ inches. Beyond that distance the measurement was not attempted because I did not have at hand a pointer long enough to indicate the position of the figure. But experiment shows that the length of the frustum increases with the distance of the circles from the eyes, and the above figures will give some conception of what the general description means. There is only one anomaly to mention, and it is that my subjective judgment of the length of the frustum is uniformly greater than the actual measurement makes it. For instance, at the distance of one foot I should guess the length to be at least an inch, while the measurement puts it at $\frac{1}{2}$ an inch. But the liability to this illusion is very easily explained. In normal vision the judgment of definite distance in the third dimension is never so accurate or reliable as that of plane dimension. I find by actual trial that the error is greater for median than for horizontal distances. Still it is a noteworthy fact that the measured length is much less than the apparent length.

The results of negative convergence were more difficult to

obtain and are less accurate, though they show approximate figures. The main difficulty came from the rough means employed to measure the length of the frustum, and were the differences between the lengths for various distances very slight I could place no reliance upon the result. But these are great enough to justify consideration. Another difficulty came from the blurring when the distance from the eyes was less than nine inches. Beyond this the blurring was so slight that the only obstacle to comparative accuracy was the apparent distance of the frustum from the eyes, which was about four feet. However, at a distance of three inches from the eyes the frustum, which was localized at a much greater distance, measured 4 inches; at six inches it measured 7 inches; at nine inches it measured 12 inches; and at twelve inches it measured 18 inches in length. Beyond this distance no measurements were made because of the difficulties attending it.

The length of the frustum varies with the position of the inner circle in the larger circles, being longer as its centre is farther from the centre of the larger. The reason for this is apparent as being proportioned to the amount of convergence to produce fusion. But it is not so easy to account for the increasing length of the frustum in proportion to the distance from the eyes of the plane on which the circles are drawn. If we could say that it was the association of a given degree of convergence with a given retinal magnitude we might rest content; but so far is this from being the case that a variation in the relative position of the centres of the circles always affects the apparent length of the frustum. More especially the difference between the results of negative and those of positive convergence, which is greater than the difference between the separate acts of convergence, at once eliminates the notion that association will account for the effect. For instance, the difference between the point of fixation in negative convergence at three inches distance from the eyes of the figures is about the same as that of positive convergence at three feet distance, and yet the length of the frustum in the former case was 4 inches and in the latter 1 inch, although to get any results at all by negative convergence I had to draw the circles on the glass much nearer the median line than they

are on the paper as represented in Fig. 4. Association, therefore, however much it may contribute to the effect,—and this, I suspect, is not much, if anything,—will not explain the phenomenon. But when it comes to offering anything better than association I fear that I cannot indicate anything more than the peculiar visual machinery for judging of magnitude and distance according to a very complex set of relations—the relation between the retinal image fixated and the degree of convergence, on the one hand, and between the same image and all other objects, homonymous and heteronymous, on the other. We merely know as a fact in normal vision that two circles of different sizes and at different distances, though producing exactly the same sized retinal image, the larger being the more remote, will be correctly judged as to real magnitude and distance, often under circumstances where experience and association can hardly account for the whole of the effect. Horopteral relations and their accompanying binocular functions, whatever they are, probably contribute largely to the result. That purely binocular influences may do something will be proved when we come to remark the results of rivalry between them and monocular agencies. But this name is so vague a conception that it can count for nothing except to exclude the efficiency of association to explain the result. Hence I shall not pretend as yet to have reached an adequate explanation of the phenomenon described, but must be content with stating the facts and remarking the difficulties in the way of accepting the associational theory.

There is a very pretty confirmation of this result and conclusion by a variation of the experiment. It might be said that the lengthening and shortening of the figure may be affected less by the changes of convergence than by the actual alteration of the distance of the circles. But in reply it can be shown that the same result can be obtained without altering the distance of the figures, and while they remain in the same plane. If we draw the two sets of circles on two pieces of paper, each near an edge, so that they can be moved toward or from each other, increasing or decreasing their distance from the median line at will, we shall find that the length of the frustum changes with the degree of convergence neces-

sary to produce fusion. Thus as we move the circles farther apart while increasing the convergence to retain fusion, the frustum shortens while its magnitude diminishes. On the other hand, as they approach each other and fusion is sustained the frustum lengthens and the magnitude increases, and all this while the figures occupy the same plane. The only difference is that when the circles retain the same distances from each other but are moved along the median line, that is, to and from the eyes, the magnitude remains the same while the length of the frustum changes. In this case, however, as described both undergo a modification, the contrast between the two different effects showing that one modification does not determine the other. This makes it all the more remarkable, therefore, that when lying in the same plane the third dimension should vary with the degree of convergence except that the modification seems to coincide exactly with the law established in the first case; namely, that localization in the third dimension represents varying relations of distance with the degree of adjustment necessary to produce fusion. It is hard to see that the slight changes of binocular parallax can account for the result, especially as that would seem to diminish with diminished convergence, while the length of the frustum increases under these conditions. But when the circles continue in the same plane binocular parallax increases with their increased distance from each other while the length of the frustum diminishes, and when the parallax decreases with their approach to each other the frustum lengthens. Besides there are two distinct reasons for wholly excluding parallax from the effect. The first is that the figures are in a plane, and the second is that the fused images are exactly alike, the parallax occurring between the monocular and the binocular circles, and the former taking no part in the combination. We seem left, therefore, wholly to the degree of convergence for explaining the phenomenon, retinal conditions apparently being excluded from consideration unless we can take into account something like Hering's theory of space perception. But with this view I am not yet satisfied, and if other experiments seem to exclude the muscular tension

of convergence from the effect we can only fall back upon functions which are as yet imperfectly analyzed.

There are several more interesting experiments bearing upon the question whether the perception of distance is a motor phenomenon. If I draw two circles intersecting each other, as in Fig. 7, and combine them either by positive or by negative, I obtain the following result, with the variations of magnitude already described. I see three intersecting circles, with the central one located at the point of fixation, but the two monocular circles do not appear in the same plane. They can be made to alternate with changes of attention, but without change of convergence between remoter and nearer positions than the central circle. Now they can be located beyond, and again within, the point of fixation. But the translocation in this case is monocular instead of binocular; that is, the images translocated are monocular, while the experiment described some years ago in *Mind* (vol. XIII., p. 512) represented the translocation of a binocular image with a variation of attention. In the present case, however, the binocular localization remains fixed, and the monocular images are translocated without change of convergence, though the effect is not due to the shifting of attention from the binocular to the monocular circles, but to a process like the inversion of mathematical perspective in geometrical figures. I can alternate their positions by the effort to think them within or

FIG. 7.

beyond the horopter. But this effort has most decided limitations. I cannot locate them thus where I please. They can occupy but either of two positions. The one beyond the horopter is considerably farther from the central circle than the one within it. This coincides singularly enough with the

tendency of divergent movements or negative convergence to increase the space representation with the degree of its condition, a fact which would hardly occur if I could determine arbitrarily the distance from the central circle at which the monocular circles should appear. But as this is a fixed position, determined by the relation of the monocular circles to corresponding points, and the function of localization, acting without any apparent change of convergence or separation of images, and as the translocation represents points which do not correspond in motor or muscular terms to the amount of tension or movement which is necessary to account for the distance between them in the median plane, and which must be the slightest in this case, if it exists at all, there seems no reason to suppose that muscular or motor efforts cause the result. There are some features about the phenomenon which look like motor influences, and which I cannot discuss at present. In a more exhaustive description I should mention the particulars, and would feel bound even to indicate and analyze them here but for several facts which depreciate their importance. *Firstly*, there is no proper proportion whatever between their localization and the utmost possible amount of muscular or motor change which might escape visual detection. A fine point in the centre of the two circles remains fused during the translocation, which represents degrees of convergence that would widely separate them. *Secondly*, I am able to translocate one of the circles to a point within, while the other remains beyond, the horopter, though this effect is not easily retained for a long period. *Thirdly*, I have been able with two sets of such circles, one immediately below the other, to get opposite effects at the same time, though only for a second or two, the result being very difficult in this case. But these facts, with the alternation of position of the two circles without the slightest apparent alteration of convergence, at least propose difficulties for the motor-tension theory unless it is made a sensorial continuum, which, as we have seen, is a retention of the muscular theory only in name. However, as already remarked, there are features about this experiment which seem to confirm the theory. I shall not emphasize its negative import, but call attention to the manner in which it refutes the

supposition of a fixed and definite connection between accommodation and convergence with a corresponding effect upon retinal and apparent magnitude. In the first place, apparent magnitude is modified with the translocation, while convergence remains unaltered. If this were due to modified accommodation, it is not a fixed degree with any given degree of convergence, and the force of supposing it such, in experiments above described, to account for alterations of magnitude, is wholly lost, and the whole result falls to the responsibilities of convergence if that be accepted as a cause in the case. In the second place, the translocation occurs without any modification of retinal magnitude, though apparent magnitude corresponds to the apparent locus of the circles. The only conclusion from this is that accommodation cannot be resorted to for an explanation of modified magnitudes, while there are also difficulties in supposing that convergence can produce the effect.

But if there are objections to the argument from the experiment just described, there is one more which makes it quite difficult to suppose that localization in the third dimension is due to muscular tension, and I shall describe it briefly. On the walls of one room in my apartment is a wall-paper of yellowish hue and blue squares formed by lines crossing it. If I cross my eyes so as to combine symmetrical portions of the surface, the diminution of magnitude and translocation of the whole surface of the wall is distinctly apparent. But it is interesting to note two anomalies. First, while all the lines or squares which are combined seem to be located in the horopter, if there be a mark or spot in any square that is not also found in the corresponding square combined with it and symmetrically located so as to fall on corresponding points simultaneously with the line of the squares, instead of being seen in the plane of the squares, that is, in the horopter, it is seen beyond it. That is to say, monocularly it seems to be localized where it really is, while binocular influences seem to affect only *similar figures and lines that fall upon corresponding points or involve the attempt at fusion*. Secondly, if a particle of dust be floating between the wall and the eyes (no reflections on my housekeeping) but beyond the horopter, represented

in the convergence mentioned, or the plane in which the squares appear to lie, as long as I watch the squares by keeping the attention closely fixed upon them, the particle of floating dust seems to be floating beyond them; that is, beyond the horopter where it really is. But the moment that I turn the attention to the dust, without in the least altering the adjustment, and although the squares do not change their apparent magnitude nor their locality, the dust seems to be floating nearer the eyes than the squares; that is, within the horopter. This experiment is the same in character as the one referred to above as having been described in *Mind*. I have confirmed it also in another way. If I take two circles, as in Fig. 1, and draw a short vertical line at any point in one of them, preferably in the line of the vertical diameter, and then combine the circles by convergence, this short line does not seem to be located in the horopter, or plane of the central circle. It appears beyond it, although by the process described in Fig. 7 it can be translocated to a point within the horopter. But the one fact to be noted is that muscular tension seems not to affect the locus of any figure which does not at the same time represent sensory fusion.

A very interesting experiment illustrating what has just been described can be performed with an ordinary stereoscopic picture and the stereoscope. Place a mark, scar, or indenture on *one* of the pictures at any point in it, and then combine by means of the stereoscope. While the real perspective is thus developed as usual, the spot put on the picture does not seem to be localized with the points of the picture on which it is placed. It seems much nearer and so to lie in an entirely distinct plane. I have had others perform the experiment and uniformly with the same result as myself. If muscular tension had anything to do with the matter we should naturally suppose that this spot would be equally affected with all others and more especially with those in immediate contiguity with it. But such is not the case, and localization seems wholly to correspond to the fusion of identical points in the impression rather than to muscular tension. In normal conditions these two factors generally, if not always, coincide, and there is no opportunity to discover exceptions. But in the

experiment just described the muscular tension cannot be said to coincide both with the localization of the picture and the mark in it which seems nearer, and we are left rather to suppose that localization is rather connected with the tendency or actuality of fusion apart from muscular conditions.

But I shall not go so far as to exclude them from the problem altogether. It is too complicated, and so many facts consist with it that it is best to allow them a contributory effect in some cases, a determining effect in others, while appearing to be wholly absent in still others. But what functions we must appeal to to supplement the defects of the motor theory I am not yet prepared to say definitely. My experiments, so far as their effects justify any generalizations at all, may be summarized in two tentative suppositions, looking to a central explanation of both distance and magnitude, independent both of peripheral conditions and motor impulses. First, apparent magnitude is not determined by retinal magnitude, but by a certain relation between this and the point of fixation or degree of convergence. Second, localization seems to depend much more on fusion than upon motor tension of the eyes. Both these suppositions appear to make the functions of space perception central, sensory or not as you please, but not proportional to peripheral conditions, nor dependent upon what is ordinarily understood by muscular sensations. Whether such an hypothesis can be sustained or not I am not prepared to assert dogmatically, as the question is still an open one with me. But my experience makes it preferable for the present.