

V.—ON THE RELATION OF ACCOMMODATION AND CONVERGENCE TO OUR SENSE OF DEPTH.

By E. T. DIXON.

IN the number of the *Zeitschrift für Physiologie und Psychologie der Sinnesorgane* which was published in May last, Dr Franz Hillebrand published an interesting article under a title of which that above this may be taken as a translation. He commenced by pointing out that no binocular experiments could establish a connection between the convergence of our eyes and our impressions of depth in the visual field, as we can never exclude the possibility of judging the distances of objects, relatively to the point of fixation, by means of 'crossed' and 'uncrossed' images—which is what gives apparent relief to stereoscopic views. But, he argued, inasmuch as there is a 'known physiological association' between accommodation and convergence, if it is shown that we can *not* judge distances monocularly, this will prove that impressions of distance are not produced either by movements of accommodation, or by those of convergence. He therefore set to work to test experimentally whether we can judge distances monocularly, or not. He made two sets of experiments, in the first of which the object whose distance was to be judged was moved to or from the observer while he was watching it, and he was then asked which way it had moved. In the second set the object was rapidly moved out of the field of view and another at a different distance substituted for it, and he had to judge whether the second object was farther or nearer than the first. From the first set of experiments he got almost entirely negative results; that is only when the card was moved very rapidly (so rapidly, as he puts it, that the accommodation could not keep pace with the movement) did any of the observers give more right answers than wrong ones. On this negative result he bases his conclusion that convergence has no effect in producing the impression of depth; and he explains away the fact that in the other form of

experiment his observers were all more or less successful in judging, by attributing their judgments to other sources than the convergence or accommodation of their eyes. It appeared to me that this argument was not quite satisfactory, and that in any case his experiments were worth repeating and extending. For the benefit of those who have not read Dr Hillebrand's paper I will briefly describe the apparatus which I employed to do so, though those who have read that paper will see that it only differs from his apparatus in minor points. I will then give some account of the results I obtained, before proceeding to criticise Dr Hillebrand's conclusions.

The apparatus consists of a rectangular wooden frame, 1·3 metres long, supported at a convenient height above the table on two trestles (see Fig.

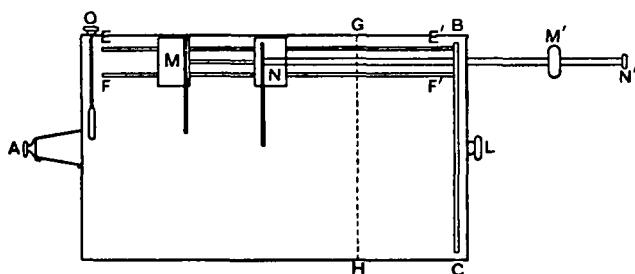


Fig. 1.

1). At one end is an eye-piece (*A*), through which the observer looks, his field of vision being closed by a vertical sheet of opal glass (*BC*), at the other end of the frame. Down one side of the frame runs a pair of railway lines (*EE'*, *FF'*), on which trollies run carrying blackened cards which shut off half the field of view, the vertical edges of the cards being the objects whose distances are to be judged. The sides and top of the apparatus, as far as the dotted line *GH* in the figure, are draped with black cloth, so that the cards may be moved up to one metre distance from the observer's eye and yet be kept practically in darkness, the observer seeing nothing but the uniformly illuminated white surface *BC*, terminated on the left by the straight uniform edge of the card. For the first form of experiment a single trolley is provided, running on wheels, which carries the card so that its vertical edge passes through the centre line of the apparatus, to which the rails are parallel. Thus as the card moves to and fro its edge does not appear to move laterally—the same amount of white background is always visible. The movement is given to the trolley by a wooden rod passing under the glass plate at the end of the instrument, which is graduated in centimetres to read the distance of the card from the observer's eye. The experimenter, who stands behind the glass plate, has also at command a handle *L*, by twisting which a shutter can be moved across the eye-piece, so that he can put the trolley where he likes before letting the observer begin his observation. For the second series of experiments Dr Hillebrand arranged two cards, on separate sets of railway-lines, both of which were pivoted under the eye-piece, and joined by a connecting rod at the other end of the instrument, so that one

card came into view from the right just as the other disappeared on the left of the field of view. This involved a longer interval between the appearances of the two edges in the median line than seemed desirable, and also made comparison more difficult, as in one case the left half of the field of view was the dark one, and in the other the right. To obviate these objections I arranged two trollies on the same set of railway lines (for simplicity these were not provided with wheels, as they do not move during the experiment), the cards being carried in frames on the trollies which allowed of a small movement horizontally across the field of view. The trollies, as before, could be moved to or from the observer by rods (MM' , NN') graduated to read the distance from the observer's eye, but by a simple arrangement the twisting of the two rods together one way or the other, projected one card forward till its edge passed through the axis of the instrument and drew the other back out of sight. To ensure that when the nearer card was withdrawn it actually did pass out of sight (for it might otherwise remain visible, if a little light was reflected off its edge) a shutter was provided in the eye-piece which could be advanced across the field of view (by a screw O) until the edge of the more advanced card was all but hidden.

The chief danger to be guarded against in constructing the apparatus is that distance may be judged by the apparent angular distance between two points. Dr Hillebrand points out that this vitiates all experiments in which the object to be judged is a thread, however fine, such as Wundt employed. In the experiments of the first series this is guarded against by having for the object the edge of a blackened card, which is carefully cut so that there are no unevennesses along the edge whose apparent distances might vary. In the second form of experiment there is no danger from this source, but the edges still have to be carefully cut lest they should be individually recognisable by any irregularity. If the card in the first series did not move exactly down the axis of the machine, and there were any noticeable marks on the background, it would be easy to distinguish between a forwards and a backwards motion. For this reason, and to prevent any comparison of the accommodation required for the card with that for a fixed point, the background is made of opal glass, which shows no marks or unevennesses of surface, and care is taken that it is uniformly illuminated. These arrangements are all Dr Hillebrand's, but I found one other precaution necessary in my form of the machine. I was making experiments with an observer who had before seemed almost incapable of judging, when suddenly, much to my surprise and that of some friends who were helping me, he began to answer right every time—in spite of my reducing the distance between the cards to a very small amount. Presently however he explained that he had discovered a trick. He had found that it took much longer for the cards to change from 'far' to 'near' than *vice versa*. I found the reason of this to be that the rod attached to the trolley M was not stiff enough, but the part between M and N twisted when the handle K was turned, so that the card M always lagged behind N . I substituted for the original wooden rods a quarter-inch square steel bar MM' passing through a brass tube NN' which was slotted down one side to allow the graduations on the steel bar to be read, and also to allow the box-wood ring M' , through which the brass tube passed, to be attached to the end of the steel bar. After making this alteration I feel confident that none of the observers formed their judgments from any other data than changes of accommodation, or of blurring in the image of the edge of the card. With the second form of experiment nothing is easier than to cheat any presumptions that might be formed from the sounds made in adjusting the cards—and I always made some

pretence of adjusting them, even if in the end I brought them back to where they were before. In the first set of experiments the observers closed their ears—and as the answers they gave were almost all negative or as often wrong as right, it is clear that the sound did not help them appreciably.

The methods of using the apparatus were these. In the first set of experiments the experimenter placed the card as he wished before opening the shutter and only opened it while the card was moving, closing it again before it got to the end of its run. Sometimes he might open the shutter while the card was at rest, and in these cases we frequently had answers 'moving away' or 'moving towards me.' In the second set of experiments the experimenter adjusted the cards with the shutter closed, and when ready opened the shutter. The observer then looked, and as soon as he had focussed the edge clearly, he said 'now,' when the experimenter twisted the handle and interchanged the cards. The experimenter noted the distances of the cards and the observer's answer—(thus "70—40 farther," which would be a *wrong* answer). In some cases the observer noted his answers himself, silently, but as this precaution seemed superfluous it was not often employed. But the experimenter never told the observer whether he had been right or wrong till the end of a series of observations—generally 20.

The results of the first set of experiments are easily dismissed; for I found, as Dr Hillebrand did, that most observers did not even make an attempt to answer, and that when they did they were as often wrong as right. I did not even find that when the motion was rapid they answered right, unless it was so sudden as to disarrange the apparatus and make disturbances which might easily give them other clues to judge by. There was however one notable exception. My brother, T. T. Dixon, was able to judge with considerable accuracy if the motion was not very slow; but he described his method of doing so as a trick—he said he fixed his accommodation for a time, and let it go by jumps, and so was enabled to answer¹. In this way on one occasion he got 11 right, two wrong, and five times expressed himself unable to answer. The value of these results seems to me to lie in showing that there is no essential distinction between the first form of experiment and the second, such as Dr Hillebrand seems to assume; and as for several reasons the second form of experiment is much better adapted for giving definite results, I confined myself almost exclusively to the method with the interchangeable cards.

It is not so easy to present the results of the second form of experiment concisely; for the method of tabulating them must depend on the theory which the tables are presented to confirm or refute. If the question to be decided were whether

¹ I may note that my brother is accustomed to rifle shooting at a target, and this may have given him a facility in altering his accommodation at will.

the distance of the card is seen ("*gesehen*") or only inferred ("*geschlossen*") it would be necessary to reject the answers of each observer after the first few, altogether. For even on the supposition that at first the observer "sees" one card to be nearer than the other, as soon as he has had an opportunity of analysing his sensations he will probably notice, or think he notices, wherein the distinction consists, and after that he will seem to be merely "inferring" his answers. But I will leave the further discussion of this point till later, and merely start with the hypothesis that the answers, when preponderantly right, are guided, directly or indirectly, by the difference of accommodation required for the two distances. Accommodation is usually measured in dipters (the reciprocal of the distance of the focal point in metres) and though it does not follow that the muscular effort or movement in the eye is in any way proportional to this, it seems on the whole the best way of tabulating the results to put all answers for distances the difference of whose dipters is the same, together—and this arrangement has the further advantage of being directly comparable with the results given by Dr Hillebrand. He further distinguishes between answers in which the change was from near to far and those in the reverse direction; as Wundt asserted that for monocular vision the latter changes were more easily observable than the former, and I have therefore shown the answers separately in the tables. The answers entered in each column are those where the difference of dipters was not greater than that shown at the head of the column, but was greater than that shown at the head of the next column to the left.

In the first three sets of experiments, *A*, *B* and *C*, the observers had perfectly normal and acute vision. *A* was a friend (Mr Charles Hammick, of Trinity College, Dublin), who was staying with me. He answered almost invariably correctly from far to near, and his right answers preponderated at almost every distance from near to far—his success being far greater than anything recorded by Dr Hillebrand, who does not appear even to have tried with differences of less than half a diopter. Mr Hammick declared at first that the card *looked* nearer or farther, but he could not say what made it look so. After a few trials however he said the way he judged was that the far card looked hazier in outline than the near one. But, being questioned, he admitted that he could see either card equally sharply defined when looking at it steadily¹. I conclude there-

¹ His sight was tested by Dr Rivers, who found that he was not short-sighted.

fore that he judged further when the new card appeared particularly ill defined *for the first moment*, or when it *took a longer time* for the edge to become sharp. The table shows that he only made two mistakes, and once failed to answer out of 33 trials from far to near, and that one of the errors and the failure were with exceedingly minute differences (96—94 c.m. and 90—89 c.m. respectively). In judging from near to far however he made many more mistakes, but he never failed to answer, and he generally answered without hesitation. The next lot of answers, *B*, are those given by my brother, T. T. Dixon. After his success with the moving card I expected he would answer well, but though he has a slight preponderance of right answers, especially judging from far to near, he was very uncertain even with considerable differences, and admitted that many of his answers were little more than guesses. He says at first he answered because the card *looked* nearer or farther, but that his later answers may all be reckoned as "inferences"—not because the cards looked different from what they did at first, but because, as he knows it is changes of accommodation he has to look out for, he bases his answers on the greater or less blurring of the outline of the new card when it first appears. In his case however when the new card appeared much blurred he judged "nearer," which is just the opposite of what Mr Hammick did. However from the very beginning he got an excess (though a small one) of right answers (17 right, 6 wrong, and 7 "no answers" in the first 30 trials). My own observations are recorded in the table *C*. It will be seen that I am distinctly more successful than my brother, and that so far from judging better from far to near I get a larger proportion of right answers from near to far ($2\frac{1}{2}$ to 1 instead of 2 to 1). Possibly owing to the fact that I had thought the question out a good deal before trying any experiments, I never had any impression of "seeing" the distance of the card. But at first I judged by what seemed to me like a feeling of strain in the eye, as if from the effort to contract the lens when the second card was the nearer. But it is more probable that this feeling was imaginary, or due to self-suggestion; and at any rate all I can determine certainly by introspection is that on the occasions when I judge "nearer" it takes a longer time for my accommodation to adjust itself. It might be expected from this that when the differences were too small to be clearly discerned I would generally answer "farther"; and the figures in the table confirm this, as far as they go. If we take the answers of .4 diopter difference and under, it appears that there were 12 near to far and 11 far to near to which I gave replies, and of these replies 15 were "farther"

and only 8 "nearer." I may briefly sum up the results, so far, as follows—

(1) Each of the three observers was able in some degree to judge distances monocularly, but the power of doing so varied greatly in the three cases.

(2) It seems clear that the judgment was directly or indirectly based on the different accommodation required for different distances.

(3) If we can trust the results of introspection it would seem that the actual criterion was in all three cases a difference in the rapidity or ease with which the accommodation adjusted itself (or was adjusted by the observer), and not in any conscious direction of the accommodation by the observer. (If this had been the case it would have been an additional reason for expecting that my brother would answer successfully—at least if his explanation of his success with the moving card is the true one.)

(4) Although the way of interpreting the criterion was different in one case from the other two, all three observers commenced by interpreting it the right way. From this we may infer that the changes which form the criterion in question are noticed, at least sub-consciously, in ordinary life, and associated with the impression of depth, even if they take no part in producing it.

(5) Wundt's observation that changes from far to near are more easily observed than changes from near to far is not confirmed in every case (Dr Hillebrand also found exceptions).

Besides the results *A*, *B* and *C* I made a number of experiments on various observers, which, as far as they went, confirmed the above conclusions, but no one set was sufficiently numerous to be worth tabulating by itself, and no useful result would be attained by mixing them together. But I also made experiments on several observers whose sight was more or less defective. Dr Hillebrand found that his most successful observer was a gentleman (an oculist) who had a cataract in one eye, so that in ordinary life he had to rely on monocular vision alone. I have tried to repeat this experiment, but so far without much success—chiefly because I have not come across a one-eyed observer whose one eye was a good one, as seems to have been the case with Dr Hillebrand's oculist. The observer *D* had a slight congenital cataract in the right eye, so that he could only see clearly, as he would have to do to judge distances at all, with his left. However he was also slightly presbyopic, which, as we shall see presently, would quite explain such success as he attained, the experiments having been made without glasses. The observer *E* had a cataract of some years'

standing in one eye, but it was due to an accident, and not congenital. The vision of the other eye is normal, but apparently not very acute. On one day in particular, after a bad night due to a disordered liver, he said he could not see the cards clearly. His table would have looked better if I had excluded this day's results, but in any case his results are not appreciably better than mine. Table *F* shows a few experiments made one day with an observer who habitually only uses one eye, not however because there is anything radically wrong with the other, but because the other being very astigmatic, and he not having had glasses to correct the astigmatism, the eye has simply fallen into disuse. This at least is how his oculist explained the matter to him. The table shows the curious result that his answers were mostly wrong. In fact it seems clear that he had the criterion by which to make a correct judgment, but that he misinterpreted it. At first sight this would seem to militate against my conclusion (4); but as the sight, even of the eye he habitually used, was not good, I think it more probable that he did not note differences of accommodation at all in ordinary life. This idea was confirmed by the way he answered. At first he said he had no idea which card was the nearer, but when pressed to make some kind of answer, he went right ahead, giving an answer every time with very little hesitation, as if he had arbitrarily fixed on a rule for answering, and just applied it. I could not however make out what his rule was by questioning him.

Tables *G* and *H* refer to Dr W. H. Rivers of St John's College, who is himself a psychologist. His eye is both myopic and astigmatic (3 diopters vertically and 4 horizontally) and moreover his range of accommodation is restricted. This being the case it follows that without glasses he would only be able to see the card (a vertical line) clearly at a quarter of a metre (25 c.m.) distance, which is nearer than the machine allows the card to be placed. Consequently the card always appears blurred, but more or less so according to the distance. This gives him a means of judging distances which a normal eyed person does not possess, and accordingly we find that, without glasses (Table *G*), he gets a very large proportion of right answers, a larger proportion even than is found in Table *A*, though there are also many more cases in which he did not venture on an answer at all. He from the first inferred these answers consciously from the greater or less blurring of the edge of the card, saying he had no accompanying impression of "seeing" the depth. When however he tried to judge with his glasses on, not having thought the matter out, he

was at first completely puzzled, and could hardly venture even a guess. But he and I talked the matter over one day, and it occurred to us that presbyopia ought to confer exactly the same power of judgment as myopia, except that in this case it would be the nearer cards which would appear more blurred. This is undoubtedly what happened in the case of the observer *D*, for he told me that the nearer card always appeared more blurred, and that without his glasses he could not see it clearly at all. Dr Rivers therefore tried again with his glasses on, judging merely by whether the edge looked more or less blurred and answering "farther" in the former case, just as he did without glasses. The result appears in Table *H*,—his answers were nearly all wrong. If his sight had had no other defect than myopia, and his glasses had been correctly adjusted (*i.e.* so that with relaxed accommodation his eyes focussed parallel rays) this would not have been the case—he would then have been able to see either card clearly, and would have had to judge like a person with normal sight. The experiment therefore shows that in addition to myopia his range of accommodation was restricted. We tested this further by paralysing our accommodation with atropine, and then trying to judge as before. My brother and I both noticed at once that we could judge even better than with normal eyes, but that we were judging by quite a different criterion. The difference was so marked that it is unnecessary for a moment to entertain the supposition that we normally judged by the blurring which would result from any failure to accommodate correctly. The results of a few trials on my brother are shown in column *a* of Table II., those of a few on me in column *b*. Columns *c*—*g* refer to observations on Dr Rivers with his eyes under atropine. Having myopia of four diopters for a vertical object, his point of clear vision, without glasses, was 25 c.m. Accordingly, just as with normal eyes, the farther the card was, the more blurred did it look; and, as before, he got a large percentage of "right" answers (column *c*), the only errors being with a difference of about a tenth of a diopter, and the doubtful ones were all under two-tenths. With glasses giving his ordinary correction his point of clear vision would be at an infinite distance; and his accommodation being fixed, the nearer cards would appear more blurred. Accordingly we find that, answering as before, he got only one "right" (.15 diopter difference) and two "no answers" (under 2 diopters difference) out of twenty (column *d*). We also tried with glasses (−1) spherical and (−1) diopter cylindrical (axis vertical). This should have reduced his clear point to 50 c.m., though it appears from the answers to have been about 40 c.m. The

last three columns of Table II. show the results; column *e* the answers between 30 and 35 c.m. at distances which were within the clear point. Column *f* answers at distances, one of which in each case was 40 and the other between 40 and 60 c.m., while column *g* shows answers at distances one at least of which was over 60 and neither under 40. As we anticipated, the answers at short distances were nearly all "wrongs," those at longer distances nearly all "rights." The difference between columns *f* and *g* is not very apparent in the table, but the former answers were all given with much hesitation, while in the latter the differences of blurring were declared to be very marked, except where the difference between the distances was very small. The two errors and the one "no answer" were given at 80 to 82 or *vice versa*, a difference of less than a twentieth of a diopter, though four times he answered rightly even at these distances. The remaining "no answer" was 80—85 (under a tenth of a diopter). To further test the power of judging by different amounts of blurring I put the moveable card into the machine, as in Dr Hillebrand's first series of experiments, and exhibited it to Dr Rivers at various distances, he having glasses giving his normal correction. He recognised the greater blurring at shorter distances and *vice versa* correctly (unless the difference was very small) even though the interval between two observations of the card was necessarily much greater than in the two card form of experiment. I think therefore we may add one more to my list of inferences from the experiments.

(6) An absence, or defect, of accommodating power may enable a person to judge distance monocularly, but this is *not* the way people with normal eyes judge.

But though these experiments seem to me sufficient to show that there is a distinction observable between a change of accommodation from far to near and one from near to far, which is quite independent of convergence, and which enables judgments to be formed, even if only by conscious inference; this does not prove that we are incapable of feeling changes in the convergence of our eyes, nor that our impressions of depth are uninfluenced by such feelings. It is an obvious, and I think quite sufficient, explanation of the failure of most observers to judge in the first set of experiments, to say the change of accommodation or convergence was not noticed merely because it was too gradual; just as a father will not notice the growth of his own children, but be surprised at the growth of other people's. But in any case the negative results of the first set of experiments would not tell against feelings of convergence unless we could be sure that the

"bekannte physiologische Association zwischen Accommodation und Konvergenz" not only holds as a general rule, but is operative under the special conditions of the experiment. Now under ordinary conditions it is well known, and it is acknowledged by Dr Hillebrand, that the association is only a loose one. Wundt drew attention to this fact, referring to observations of Volkmann, Donders and Czermack; and all Dr Hillebrand had to reply was that their experiments were inconclusive, and were conducted under artificial conditions. He could hardly have contended that the conditions of his experiments were other than artificial; but as the question is interesting in itself I thought it worth while myself to make some further experiments upon it.

I constructed a piece of apparatus which (in spite of the protests of my classical friends) I call the "Accommodometer." It consists of a longi-

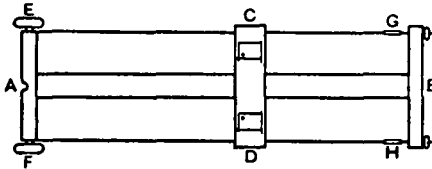


Fig. 2.

tudinal slide (*AB* Fig. 2), about a metre in length, with a notch at one end in which the observer can rest the bridge of his nose, and which is supported on pillars at a suitable height. The longitudinal slide carries a cross-slide, on which move two wooden blocks (*C*, *D*) carrying vertical pins, which are the objects to be observed. By means of an endless cord passing over two hand-wheels (*E*, *F*) at the eye end of the instrument, the observer can move the pins towards or away from each other on the cross slide, or he can move the cross slide bodily towards or away from himself, without having to move his head. The longitudinal slide is graduated in centimetres, to read the distance of the pins from his eyes, and the cross slide in millimetres to read the distance between the pins, both graduations being arranged so that the observer can read them without moving. The course of the endless cord is from an attachment under the block *C*, round a pulley under the end *C* of the cross slide, round the hand wheel *E*, to a pulley *G* at the far end of the instrument (which is attached to a screw by which the cord can be tightened), back to the pulley under *C*, to an attachment under the block *D*, round a pulley under the end *D*, to a second tightening pulley *H*, to the hand-wheel *F*, back to the pulley under *D*, and to the attachment to the block *C* again. The effect of this arrangement is that if both hand-wheels are turned at the same rate in the same direction the cross slide travels in or out, but the pins remain stationary upon it. If however they are turned at the same rate in opposite directions, the pins move to or from each other while the cross slide remains fixed. By a combination of the two motions any desired movement may be given to the pins, with the exception that (if there is no slipping of the cord) the two pins always remain at equal distances on either side of the axis of the instrument.

Now suppose the observer to look at a point in the axis of the instrument, but nearer or farther than the cross slide. He will in general see four pins, but by altering his convergence, or by moving the pins, he can make two of the four images coalesce into a "middle pin," which is seen by both eyes. If now this "middle pin" is seen sharply defined, his accommodation is adjusted for the actual distance of the pins, but his convergence is adjusted for the point of intersection of his optic axes; which, if his interocular distance is known, can be readily calculated from the readings of the scales. To test the association between convergence and accommodation I put the cross slide successively at 20, 30,...up to 100 c.m. distance, and having got a "middle pin" either by convergence (squinting) or divergence (stereoscopically) I see how far I can separate the pins while the "middle pin" remains sharply defined. If there were no association between convergence and accommodation I should be able to see the middle pin sharply defined as long as I could unite the images at all. If on the other hand the association were absolute, the pin would always look more or less blurred. As a matter of fact I find that at medium distances I can keep the middle pin clearly defined up to a certain point, as I separate the pins, but after that, though I can unite the images, the middle pin is blurred, evidently from a failure of accommodation; as if the accommodation were tied to the convergence by a slack rope, which was hauled taut when the pins had been separated a certain distance. To give some notion of the slackness of the rope in my case I have given the average of the results of the last 10 measurements I made at various distances in Table III. If we call the interocular distance of the observer a , the distance between the pins b , their distance from his eyes d , we may measure the difference between the actual angle of convergence (or divergence) and that normally associated with the distance for which the eyes are accommodated by $\frac{b}{a}$, or $\Delta\theta$ say. The absolute angle of convergence θ , measured in the same approximate way, would be $= \frac{a \pm b}{d}$, the lower sign applying to stereoscopic combination, the upper to squinting. When combining stereoscopically if b is greater than a , θ is negative, i.e. we get absolute divergence of the eyes. I get this result at long ranges (it is indicated in the tables by a line over the figure, thus $\overline{001}$) but my friend Mr Hammick got it at all ranges. As the results in the table are averages of ten trials they do not show the greatest angle of divergence

I got, which was $\overline{026}$ or about $1\frac{1}{2}$ degrees. Mr Hammick never attained to this amount of divergence, though doubtless that was only due to want of practice. These experiments by divergence at long distances do not however throw any light on the connection between accommodation and convergence, as I found that at distances of 70 c.m. and over, whenever I could unite the pins by divergence at all I could see the middle pin clearly. For shorter ranges in the divergence column, and for all ranges in the convergence column, the figures given in the table refer to the greatest distance between the pins for which the "middle pin" remained *sharply defined*. The figures for convergence at 70 and 80 c.m. are really too small, as on some days I was able to converge beyond the limit of the cross slide (300 m.m.). At 90 and 100 c.m. I could, nearly always do so, and sometimes considerably over, so I have merely entered (in brackets) the numbers corresponding to 300 m.m. for comparison. It appears from the table that the variation in the angle of convergence which I can make without altering my accommodation, on either side of the normal, decreases steadily with the distance. In the columns headed *DD*, which give the difference of diopters corresponding to the distances for which the convergence and accommodation are adjusted, $\left(\frac{b}{ad}\right)$, the same de-

crease is visible, though it is not so marked. And though the results obtained on different days differed a good deal in their absolute magnitudes the same decrease was always visible. The table shows that even at 100 c.m. distance I could make over six diopters difference in my convergence without affecting my accommodation; and though in Dr Hillebrand's experiments the observers had no motive for consciously influencing their convergence, it is clear that, if I may be taken as a fair sample, the *bekannte Association* affords hardly even a *prima facie* reason for supposing that the convergence followed the accommodation in those experiments. Of course it is true that the results in the table were not attained without effort and practice. Even after I had got over the preliminary difficulty of keeping the images combined into a "middle pin" I did not at first get such large figures as those given. But if the convergence strayed from the accommodation in Dr Hillebrand's experiments by even as much as one diopter it could not afford the means of judging successfully, and it would therefore be natural to suppose that any information which might be given by a sense of convergence would be neglected.

I have not been able as yet to make any such systematic

experiments with any other observers, but the few trials I have made are enough to show that there are considerable variations in the strength of the association. The great difficulty in getting observations made is that few people are able without practice to combine the images sufficiently steadily to make measurements, or even to say definitely whether the "middle pin" is sharply defined or not. Mr Hammick could only succeed by divergence, but he declared that he always saw the middle pin sharply defined, even with a difference of diopters of 4.2 (at 25 c.m.). By convergence however he was unable to keep the images combined, even when he succeeded in combining them at all. Dr Venn kindly made some experiments one day, and he easily kept the images combined, and perfectly defined even with considerable differences between his accommodation and convergence. But as he is presbyopic, and used glasses, the observations were not useful for the purpose of comparison with Dr Hillebrand's experiments.

But the really important point to settle, if it can be settled, is whether the convergence actually follows the accommodation when judging distances monocularly,—for if it does not, it might be that wrong answers were given in just those cases where the convergence went wrong, and in this case we should have a powerful argument *in favour* of sensations of convergence. I tried to test this by direct observation. I fitted a pair of parallel mirrors, inclined at 45° to the vertical, the one close beside the eye-piece of Dr Hillebrand's apparatus in front of the observer's second eye (which he was requested to keep open), and the other above it, so as to reflect the light from the eye over the top of the instrument, parallel to its axis. The experimenter, or another observer, was thus enabled to look directly into the un-used eye, and he was in a position to detect very small movements in it. I made on the whole over three hundred observations in this way (see Table IV.). I have only recorded observations I made myself, as there were only a few others to record, and the personal equation would prevent their being comparable with mine. The cases enumerated in the columns headed "Right (agreed with fact)" are those in which I noted (or thought I noted) a movement of convergence if the second card was the nearer, or *vice versa*; i.e. in which the movement was what would have occurred in binocular vision. These cases are subdivided again according as the observer answered right or wrong or not at all. Observations *F* and *G* are put separately as I have shown reason for believing that they were affected by an error of interpretation. In the totals at the bottom I have first added in these observations as they

stand, and then as they would be if we interchanged right and wrong answers. It will be seen that in the later case the grand total shows the same sort of proportion as the total of the first four sets of observations.

In so far as any value can be attached to these observations, they seem to show that in the great majority of cases the convergence *did* follow the accommodation (agree with fact) whenever it was noticeable. The first four sets of observations further give 76 cases where the answer agreed with the convergence, to 54 where it did not, whereas the proportion of right answers to wrong is 79 to 51. The table therefore as it stands affords no presumption that the movements of convergence had anything to do with the answers. But after making control experiments I have come to the conclusion that very little value can be attached to the observations. I tried observing one eye of a person looking with both eyes at points at different distances from him—but it was impossible to reproduce the exact conditions of the monocular experiments, or even to get conditions equally favourable for observation. Sometimes I seemed able to detect the movements with tolerable certainty, but on others I judged as often wrongly as rightly. The explanation of this probably is that what I noted were the quickest or most sudden movements, and that a slow movement of considerable amplitude might escape me. For on several occasions where my answer proved to be wrong I could swear that the movement I noted actually took place. My brother, whose eye it was that I watched, said he was aware sometimes of oscillations in the movement of his eye before he fixated the second card exactly. It may be that under the conditions of the monocular experiments these oscillations would be less likely to occur, the movement being more automatic. But I am afraid that until some superior method of observation is devised it will be impossible to say with certainty how far the convergence follows the accommodation in these monocular experiments; and it is therefore impossible to reach any conclusion about feelings of convergence through them.

But interesting as these experiments of Dr Hillebrand's are in any case from the point of view of the experimental psychologist, what lent them their peculiar fascination was the far reaching significance he attached to them. He regards them as destructive of any argument in favour of muscle sensations drawn from the supposition that we can judge distance by convergence of our eyes. He says (p. 148 of his paper) "As it appears in contradiction to these suppositions that, for example, a feeling of convergence is entirely non-existent, or at least does not operate as an associating link in judging distances in space

(let alone that it should itself take part in an intuitable spatial determination) the foundation of all these constructions is cut away." I have already pointed out that this conclusion is entirely based on the negative results of his first set of experiments; for the fact that he was able to give another explanation, however plausible, of the successes in his second set, cannot be held to prove that sensations of convergence are 'non-existent.' I should like to criticise these conclusions on more fundamental grounds also, but as this paper is intended rather as a record of observations, than a criticism of opinions, I postpone a general discussion of the results to a more suitable occasion.

TABLE I.

Difference of Diopters			.05	.1	.15	.2	.25	.3	.4	.5	.75	1	1.5	2	2.5	Total
A	{ near to far	right	5	4	3	0	5	1	1	6	9	3	1	1		39
		wrong	1	3	1	2	0	1	2	1	3	0	0	0		14
		no answer	0	0	0	0	0	0	0	0	0	0	0	0		0
	{ far to near	right	3	4	2	2	3	1	3	2	8	0	2	0		30
		wrong	1	0	0	1	0	0	0	0	0	0	0	0		2
		no answer	1	0	0	0	0	0	0	0	0	0	0	0		1
Total			11	11	6	5	8	3	6	9	20	3	3	1		86
B	{ near to far	right				0	0	0	5	2	5	7	7	8		34
		wrong				0	2	0	3	5	7	5	5	3		30
		no answer				0	1	0	3	0	4	3	2	0		13
	{ far to near	right				3	0	1	1	6	10	9	9	4		43
		wrong				0	0	0	0	1	3	1	5	3		13
		no answer				0	0	0	1	1	0	0	1	0		3
Total						3	3	1	13	15	29	25	29	18		136
C	{ near to far	right			0	0	2	2	4	4	15	15	20	2	1	65
		wrong			0	1	1	1	1	2	5	4	7	2	1	26
		no answer			0	0	1	0	1	2	5	3	0	0	0	12
	{ far to near	right			0	0	1	1	2	5	15	19	20	6	3	72
		wrong			1	1	1	2	2	3	13	9	4	0	0	36
		no answer			0	0	0	1	0	0	1	2	0	0	0	4
Total					1	2	6	7	10	16	55	52	51	10	5	215
D	{ near to far	right				6	1		1	4	3			1	1	17
		wrong				2	1		0	1	1			0	0	5
		no answer				0	0		0	1	0			0	0	1
	{ far to near	right				5	1		0	5	1			1	0	13
		wrong				3	1		0	0	1			0	0	5
		no answer				0	1		0	0	0			0	0	1
Total						16	5		1	11	6			2	1	42

E	near to far	right					5		3	10		9	5	32
		wrong					5		1	7		4	1	18
	far to near	no answer				1		0	0		1	0	2	
		right				4		2	8		6	3	23	
		wrong				3		4	8		2	1	18	
		no answer				2		0	2		1	0	5	
Total							20		10	35		23	10	98
F	near to far	right						0	0	5	1		0	6
		wrong						0	2	7	0		9	18
	far to near	no answer					0	0	0	0		0	0	
		right					1	1	0	0		3	5	
		wrong					1	1	1	2		6	11	
		no answer				0	0	0	0		0	0		
Total							2	4	13	3		18	40	
G	near to far	right	3	2	0	1	6	5	11	1	9	0		38
		wrong	0	0	0	0	4	1	1	0	2	1		9
	far to near	no answer	0	1	1	0	2	1	3	1	3	0		12
		right	1	3	0	1	6	3	15	2	8	2		41
		wrong	1	0	1	1	0	0	3	0	1	1		8
		no answer	0	0	1	1	5	1	3	1	1	0	13	
Total			5	6	3	4	23	11	36	5	24	4		121
H	near to far	right					0		0					0
		wrong					5		13					18
	far to near	no answer					0		2					2
		right					1		2					3
		wrong					4		6					10
		no answer				0		4					4	
Total							10		27					37

TABLE II.

(Eyes under Atropine.)

		a	b	c	d	e	f	g
near to far	right	6	11	9	1	0	5	10
	wrong	2	1	1	9	5	1	1
	no answer	3	2	2	2	2	2	1
far to near	right	6	8	5	0	1	4	11
	wrong	1	1	1	8	7	0	1
	no answer	2	7	2	0	1	1	1
Total		20	30	20	20	16	13	25

TABLE III.

Distance from eye (c. m.)	by Convergence			by Divergence			
	<i>b</i>	$\Delta\theta$	<i>DD</i>	<i>b</i>	$\Delta\theta$	θ	<i>DD</i>
20	115	·58	9·7	81	·15	·15	2·6
30	139	·44	7·4	45	·15	·05	2·5
40	158	·39	6·6	52	·13	·02	2·2
50	184	·37	6·1	57	·12	·005	1·9
60	213	·36	5·9	61	·10	·001	1·7
70	243 (?)	·35	5·8	64	·09	·006	1·5
80	273 (?)	·34	5·6	68	·08	·010	1·4
90	[300	·33	5·6]	71	·08	·012	1·3
100	[300	·30	5·0]	74	·07	·013	1·2

TABLE IV.

Convergence				Right (agreed with fact)				Wrong (disagreed with fact)				None observed				Total
Answers				right	wrong	none	Total	right	wrong	none	Total	right	wrong	none	Total	
B				14	10	0	24	8	6	0	14	28	20	4	52	90
D				12	6	1	19	1	1	1	3	8	2	0	10	32
E				32	25	3	60	2	2	1	5	14	6	2	22	87
G				8	0	2	10	2	1	0	3	30	5	5	40	53
Total				66	41	6	113	13	10	2	25	80	33	11	124	262
F				5	18	0	23	3	0	0	3	2	5	0	7	33
H				1	3	1	5	0	10	2	12	1	16	3	20	37
Total				6	21	1	28	3	10	2	15	3	21	3	27	70
Grand total				72	62	7	141	16	20	4	40	83	54	14	151	332
The same, interchanging right and wrong answers in lines F and H.				87	47	7	141	23	13	4	40	101	36	14	151	332