

genital achromasy *may be* a defect of the cortical centers; this has long been known to be very frequently the sole cause in cases of acquired achromasy (see Förster's case, among many others, *Arch. f. Ophthalm.*, 36 (194)), and since retina and cortical cells are both essential links in the chain of causation in question, and since it is quite certain that there are separate centers for color-vision, there is every reason to suppose that mal-development, as well as disease, may sometimes cause the latter to fall out of function. But, on the other hand, the undeniable cases in which total lack of color-vision is accompanied by total blindness of the fovea, even though they are not exhaustive of all cases, still point as strongly as before to the cones of the retina as being, in these instances, the source of the defect. What is rendered certain by the case of Raehlmann is that there is nothing forced in the referring of cases of a different sort from these to a different seat of the physiological lesion, namely, the cortex.

There is now, therefore, nothing that stands in the way of believing that: (1) *The rods are the organ for nothing but achromatic vision*; (2) *Color is mediated by the cones only*; as I maintained, following upon Max Schultze and Parinaud, in 1892.

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### THE ILLUSION OF DEFLECTED THREADS.

Professor Pierce has reported in the September number of this REVIEW some new and interesting observations on a thread illusion which I described in May, 1898. He has also presented an explanation of the illusion which he regards as simpler and more direct than that originally offered. One almost despairs of ever finding an explanation of any optical illusion that shall command universal acceptance. But it is doubtless the duty of those who are engaged in the investigation of these illusions to compare notes as often and as fully as possible in order to reach some clearness as to the grounds of their differences, even if they find it impossible to accept each other's positions.

The illusion under discussion has as its essential feature the apparent turning of certain parts of two threads so that these parts do not seem to be continuations of the threads to which they belong, but seem to extend away from the threads into the third dimension. The threads which form this illusion lie at different depths in front of the observer, and the upper one passes over the lower one at an acute angle. The points at which the two threads seem to cross are, under these condi-

tions, different for the two eyes. It is the middle part of each thread, or the part between the two crossing points, that is apparently deflected. In some cases, and it should be noted that it is in some cases only, the image of the middle part of the upper thread fuses, not with the other image from the same thread, but with one of the images from the lower thread. That is, instead of the two images from one thread holding together throughout their whole course, they fuse only outside the monocular crossing points. In the middle parts, that is, between the monocular crossing points, they break away from each other and then they may fuse, as was pointed out in the original article, with images from the other thread, even though that other thread is at a different level. It may also be added that this unusual form of fusion may take place even when the two threads are of different colors, so as to give the images very decidedly different characters.

Now the essential difference between Professor Pierce's explanation and mine is, that I lay greatest stress on the *separation* of an image from its true companion, while Professor Pierce lays greatest emphasis on the *fusion* of the images after separation. He assumes that separation of the images is easily possible, and that the moment a new mode of fusion offers itself as a possibility it may be adopted as easily as any other possible mode of fusion. Professor Pierce finds in the presence of a possible new mode of fusion a sufficient explanation of why images which come from a single thread and have been fused through a part of their course, should in the middle part of their course be separated from each other and fused with images from an entirely different thread. In taking this view he discusses only cases in which the point of fixation is chosen half way between the two threads, thus rendering easy the assumption that the images from both threads were double from the outset. His problem, as he sees it, is therefore merely one of finding possible modes of fusion.

The objections that offer themselves to Professor Pierce's view are as follows: First, the deflection often takes place in only one image of one thread. The first case of the illusion discussed in the original paper<sup>1</sup> (and I have confirmed its validity since by a large number of observations with many subjects) is that in which the point of fixation is on one of the threads (not half way between the two threads). When this point on one thread is fixated, the two images of the fixated thread fuse with each other as usual. One of the images, and usually only one of the images, of the other thread will be deflected. These statements can be verified by using colored threads, so that the different

<sup>1</sup> PSYCHOLOGICAL REVIEW, Vol. V., p. 288.

images can be distinguished. This fact shows clearly that deflection is prior to any recombination, and, so far from being dependent on the recombination, is the source of the conditions which make a new and unusual mode of combination possible.

The second objection is this, the mode of fusion to which Professor Pierce refers in his explanation is not the only mode of fusion possible with the images that are given. There is a perfectly regular mode of fusion between the images belonging to the same thread which would result in a perception of each thread as a continuous whole lying at a different level from the other thread. This usual mode of fusion takes place outside of the monocular crossing points. It takes place there so readily and completely that it is very difficult to perceive the double images. Now if the two modes of fusion which are possible, namely, the usual form of continuous fusion and the one that actually takes place, were both equally possible, and if one is chosen to the exclusion of the other, particularly if that one is, to say the least, unusual; then the explanation is not complete until we have been told why this particular choice is made. The statement that a certain choice is made is merely the descriptive preliminary to the more fundamental question of why it was made. As a mere possibility the chances are no more in favor of one mode of fusion than of the other. But in the illusion in hand complete normal, or usual, fusion never takes place in portions of the thread lying between the monocular crossing points. Something has happened to prevent it. What is that something? Certainly it is not enough to say that another mode of fusion has presented itself as possible. As a matter of fact, another mode of fusion has appeared which is not merely possible, but *necessary*. And this necessity Professor Pierce has not explained.

Finally, to deal with two particular points which Professor Pierce dwells upon with great emphasis. The first is that the apparent deflection appears even when the monocular crossing points are covered up. The conditions that result from this covering up of the monocular crossing points are essentially different from those which appear in the ordinary illusion. In the ordinary case of the illusion there is a mixture of what may be called monocular and binocular modes of interpretation. The result is that we see both the separate threads at different depths and also the oblique, deflected threads. When the monocular crossing points are covered, the middle parts of the threads are cut off from the end parts. The result is that the binocular mode of interpretation is very much weakened. So much weakened, indeed,

that *one can never see both the deflections and the threads in their real positions, but only the deflections*. Now the seeing of the deflections *only*, means that the two images that enter a single eye are treated exactly as if they came from a single plane. They might, for example, have been lines drawn on a single plane card. But this treatment of images which really come from different depths as if they came from a single plane, is what the original article attempted to point out as the essential characteristic of monocular vision in general, and of this part of the threads in particular. In covering up the monocular crossing points and isolating the middle sectors of the threads, Professor Pierce has, therefore, shown what becomes of the illusion when the binocular factors are cut out of the original conditions and the monocular factors alone are left. He has thus contributed a very interesting and striking confirmation of the general position that images which fall in a single eye are treated exactly as if they came from a single plane. What Professor Pierce has failed to see is, that the particular case of the illusion which he emphasizes is a particular and limited case, not the original form.

The second point with which I wish to deal is one on which I have no doubt that Professor Pierce will be in fullest agreement with me, but one which it is perhaps well to bring out for the sake of the general discussion of the whole subject of illusions. Professor Pierce writes in one place, 'nothing but the universal laws of tri-dimensional vision are here in operation.' And again he writes, "In view of these various considerations I cannot refrain from the conclusion that Professor Judd's illusion presents no new visual principle \* \* \* I am convinced that we have before us in this illusion only a particularly interesting case of what is eternally happening whenever we open our eyes to see."

The article describing the illusion was very misleading if it gave the impression that its author supposed that the facts presented led to the discovery of any new principle. I take it that the value of any illusion is to be found in the fact that the old 'universal laws of tri-dimensional vision' are absolutely reliable and unchangeable. What we try to do is to see how unusual appearances can be reduced to these same universal principles. Now in the illusion under discussion the appearance is unusual, while the principle of vision may be safely assumed to be the same as usual. The unusual appearance must be due to an unusual combination of conditions. That unusual combination of conditions is looked for in the fact that a part of the field is seen chiefly as one eye would look at it without the help of

the other, while the rest of the field is seen by both eyes in joint action, just as it usually is seen under ordinary conditions.

When Professor Pierce says of the result that it is 'what is eternally happening whenever we open our eyes to see,' I should be in most hearty accord with his statement if it referred to only the general principles of vision involved. Certainly visual perception must here, as elsewhere, follow the long and fully established mode of this form of mental activity. But if Professor Pierce means that under the ordinary conditions of visual perception, that is, under the conditions which are constantly recurring when we open our eyes to see, images from one object may fuse for a certain distance and then suddenly break off to fuse with other images in the field of vision and finally come back to the original mode of fusion—if, in other words, he means that the conditions of this particular thread deflections are everywhere common, then I shall have to take exception to his conclusions. The distinction between uniformity of principles and invariability of typical conditions is worth keeping clearly in mind—for, if I mistake not, the value of the whole study of illusions depends on this distinction.

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### THE SPACE-THRESHOLD BY THE PSEUDOSCOPIC METHOD.

Recently my attention was called to an article by Professor Stratton which appeared in your issue of November, 1898, and which I found contained a serious error. Professor Stratton came to the remarkable conclusion that binocular relief was still perceptible even when the images which produced it had a separateness of less than 24", or in other words that our sense of binocular perspective was more delicate than our visual acuity. It seems to me that if this is true it must overthrow our ideas as to the rods and cones being the ultimate visual units. A glance, however, at his method of reaching this result reveals the source of his error. He has compared pseudoscopic with stereoscopic vision without allowing for the fact that this is practically equivalent to doubling his interocular distance. His results therefore should be multiplied by two, which gives 48", and this, according to Professor Stratton's own statement, closely agrees with the results obtained by other observers.

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