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LETTER TO THE EDITORS

CONFUSION PREVAILS!

A REPLY TO KERTESZ AND SULLIVAN

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Kertesz and Sullivan's (1976) reply to our article (Kaufman and Arditi, 1976) is an example of the usefulness of give and take among members of the scientific community. We now know the precise instructions given by Kertesz and his collaborators to their subjects in the many experiments they have reported. This information was not previously available. We also know that our assumptions about the criteria of fusion ostensibly employed by their subjects was not far from the mark. Also, "The Fusion Illusion" seems to have led Kertesz and Sullivan to perform several interesting experiments which enable us to see the problem of the nature of so-called fusion more clearly. Thus has scientific inquiry been furthered by the present controversy.

Unfortunately, Kertesz and Sullivan have not yet proven that the concept of fusion as they define it should prevail. For one thing, it is now clear for the first time that they had preselected subjects using as a criterion the apparent motion which sometimes occurs when one looks at cyclofusional stimuli through alternately occluded eyes. It would be worthwhile to study the excluded subjects if only to discover if the occurrence of apparent movement really does predict performance in tests for central cyclofusion. This selection procedure needs validation. Readers of the papers should have been informed of this selection procedure prior to the present communication.

Apart from the fact that Kertesz and his associates selected subjects, they also required their subjects to utilize the multiple criteria we described in our paper, i.e. singleness, straightness and horizontality. The violation of any one of these criteria should result in a judgement of the presence of disparity. It therefore is hard to understand the force of their criticism that we deal with these criteria separately. When the method of signal detection theory is applied, any one of these criteria could be employed. We are happy to note that in their Experiment 1 Kertesz and Sullivan confirm the results of our Experiment 1, i.e. some subjects do detect differences between the crossed and uncrossed lines even with nominally subthreshold stimuli.

However, there is one unfortunate problem with the experiment reported by Kertesz and Sullivan. In actual fact they do not really seem to have employed the method of limits. The account of the experiment in their letter indicates that the threshold stimulus was the one which was perceived as just fused as the magnitude of disparity was increased. Measurements were not made when the disparity was decreased. In the method of limits such increasing and decreasing trials are both needed and their average gives the threshold. As is well known, without such counterbalanced trials the data are biased. In this case, for example, the common anticipation error could have occurred. The presence of an anticipation error might be the reason why Kertesz and Sullivan had to discard the results of six of their 10 observers because these observers could not discriminate the existence of a disparity in nominally threshold stimuli when using a forced choice procedure. It would have been useful if Kertesz and Sullivan had reported on the data obtained with these six subjects. Their failure to do so makes it difficult to evaluate their experiment. This is particularly disturbing since by the hypothesis that observers use the early portion of a trial in which to detect disparity prior to the occurrence of fusion, these subjects should have been more sensitive in the signal detection portion of the experiment. Thus, the majority of the subjects could not use the cue which Kertesz and Sullivan maintain underlies our results. Why could six of ten subjects not use the "initial appearance" cue? The experiment must be suspect because even without this cue observers are almost always more sensitive to small differences in a forced choice decision experiment than they are in adjustment-type experiments.

Experiments 2 and 3 reported by Kertesz and Sullivan are ingenious and interesting. Unfortunately, these experiments do not provide convincing evidence for the existence of central cyclofusion either. There are several reasons for this. First, when a stimulus of one disparity is exposed for 10 sec and then is switched abruptly to a stimulus of another disparity, there is a strong apparent movement effect. Where the adaptation disparity is $\theta/2$ and the test disparity is θ, the magnitude and direction of the resulting apparent movement is identical to that which occurs when the test disparity is zero. (And the "initial appearance cue" is also present.) This identity of apparent movement is a cue which could confuse the subject, thereby reducing his ability to discriminate the disparate from non-disparate stimuli. Similarly, when the adapting disparity is θ, as in Experiment 3, and when the test disparity is also θ, then there is no apparent movement. However, when the test disparity is zero there is a strong apparent movement. If the subject bases his decision on the existence of apparent movement it is easy to see how he might confuse the existence of movement with the presence of disparity and the absence of movement with the absence of disparity, thereby giving the bizarre negative d' results they obtained. It would be interesting to know if the subjects were given feedback as in our experiment.

This is not the only basis for criticizing these experiments. It is possible, for example, that local retinal adaptation produced during the adaptation period could alter sensitivity to either or both the disparate and non-disparate stimuli, depending upon the disparity of the adaptation stimulus. Our own unpublished data suggest that sensitivity to disparity is affected by line luminance. Similar effects could be produced by local retinal adaptation. Hence, without appropriate controls, e.g. randomly varying line luminance from trial to trial, we cannot tell if local adaptation resulted in a differential diminution of the effective intensities of the stimuli, thereby decreasing sensitivity to disparity after exposure to the $\theta/2$ stimulus and adding a spurious brightness difference cue after exposure to the θ stimulus. It is worth noting that in their Experiment 3 Kertesz and Sullivan had an initial appearance cue as well.

We also believe that Experiments 2 and 3 are rather indirect tests of the initial appearance hypothesis and depend upon a number of assumptions about the nature of the fusional process. A proper interpretation of these experiments depends upon the model of fusional processes assumed by Kertesz and Sullivan. It is possible to perform simpler experiments to more directly test the initial appearance hypothesis without making quite so many assumptions. We are now preparing to perform these experiments.

In one experiment we are slowly increasing the luminance of the stimulus from a level at which the lines cannot be seen to one at which they are fully visible for a period of time. During the early phase of exposure the subject must of necessity be unable to distinguish disparate from non-disparate stimuli, thus avoiding a brief period in which disparity is purportedly detectable after the onset of the exposure. Our preliminary data, with one subject, indicates that he is just as sensitive to the existence of the disparity as he is when given a flash exposure such as those we have used previously. We are also planning other experiments in which exposure time is manipulated and also experiments in which adaptation stimuli are used but with controls for apparent movement artifacts.

We do not have the space in this brief communication to review all of the reservations we have about this line of research. However, we cannot let the present opportunity pass without a small comment on the importance Kertesz and Sullivan ascribe to the presence of a fixation point. In this connection it is necessary to be clear that Kertesz and his associates did not measure vertical eye movements. Even with a fixation point, it is possible for the eyes to spontaneously execute vertically disjunctive movements. Such movements would cause the intersection of the tilted lines to slide horizontally across the field. These movements would also cause the vertical disparity at one end of the display to become smaller as that at the opposite end becomes larger. Thus, in prolonged exposures, such as those used in Kertesz' earlier work, eye movements can in fact play a role in producing confusion between disparate and non-disparate stimuli. Moreover, these experiments do not exclude hypothetical effects of fusion other than the pure form of cyclofusion that Kertesz and Sullivan believe they are dealing with.

With regard to the "fusional effort" experiments (Sullivan and Kertesz, 1975) in which a non-disparate probe is perceived as disparate when a background is fused, it should be noted that cyclorotational eye movements were not measured during these studies in which powerful stimuli to motor fusion were employed. Also, vertical disjunctive movements could produce apparent doubleness of such stimuli.

These same fusional effort experiments are also the basis for the criticisms of our experiment in which background features of the environments were visible to the subject. Of what use is fusion if fixated and fused features of the natural environment inhibit fusion of other features which do not lie on the horopter? Clearly, this implicit proposition is at variance with the classic view of fusion in which half-images lying on both sides of the horopter may be simultaneously fused or seen as single. Kertesz must make his alternative theory explicit if the criticism based on visible external features is to be taken seriously.

With regard to their criticism of our Experiment 4, it is true that Kertesz and Optican (1974) never asked their subjects to report on the boundary between the two concentric regions of the display. Nevertheless they did provide us with a figure which shows this boundary to be missing during so-called fusion. We believe that it is of some importance that the effect of fusion on this boundary be dealt with. Our own reservations about the meaning of our Experiment 4 are dealt with in our paper (Kaufman and Arditi, 1976). They are not entirely at variance with those of Kertesz and Sullivan.

One question which continually recurs concerns the adaptive purpose, if any, of central cyclofusion. We have tried to imagine situations in nature which would require the evolution of a central mechanism which would compensate by as much as 13° for the lack of ability of the eyes to counter-rotate. Such a central mechanism must be complicated. Our own failure to define the biological usefulness of such a mechanism is a major reason for the doubts we have expressed concerning central cyclofusion.

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