Notes and Comment

Dot displacements can be deceptive: A reply to Hartley

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Hartley (1979) has replied recently to my comments (Wenderoth, 1978) on an experiment and an analysis of his (Hartley, 1978). One of the points I had made was that when Hartley had observers place dots on blank sheets of paper to match the apparent positions of dots within outline frames, these judgments mirrored the appearances of the surrounded dots. That is, if a dot appeared too far to the left within a frame, the subject would indicate this by placing a dot too far to the left on the blank sheet. In my experiments, the method of measurement was different: subjects had to set a surrounded rod to apparent vertical. It needed to be pointed out that in the latter case, rod settings *compensated* for the appearance of a truly vertical rod, rather than *representing* it. Hartley had stated (see Wenderoth, 1978) that the two experiments gave opposite results; I wished merely to note that the results were logically equivalent, rather than opposite, as he wrongly believed.

In his recent reply, Hartley (1979) suggests that when a dot within, say, a square frame is closer to the center than to the sides of the square, the "major axes hypothesis" (Wenderoth, 1978) predicts that the dot will be repelled from the nearer virtual axis of symmetry rather than the real line (side) and so will appear to be further from the square's center than it really is. He claims that the "major axes" account *can* predict this result, because now he accepts that appearances and compensatory settings are directionally opposite. But, he notes, directionally similar errors also occurred in his experiment when the enclosed dot was nearer the frame's side than its center. This, he asserts, is contrary to the "major axes" prediction.

It needs to be reemphasized (see Wenderoth, 1978) that the "major axes" hypothesis refers to the *orientation domain*. That is, the hypothesis asserts that a nonvertical frame will induce an apparent tilt in a vertical rod and that this induced tilt will be in the direction away from whichever frame axis of symmetry is nearest *in orientation*, not position, to the rod. The hypothesis has never referred, or been extended, to the position domain, which is why I concluded earlier (Wenderoth, 1978, p. 574) that "a test

line placed noncentrally in an outline frame would very likely be displaced perceptually [i.e., left or right, in *position*], as were Hartley's dots; but such displacement would not necessarily imply anything about the perceived *orientation* of the line."

The potential complexity of the situation is increased even more when dots are used rather than lines, and I have already alluded to that problem too (Wenderoth, 1978). Consider Hartley's (1979) claim: He asserts that a dot that is nearer the side than the center of a square should be repelled from the side because the side is nearer (i.e., in *position*), and he attributes this (incorrect) prediction to the major-axes hypothesis. Two points render this statement unacceptable. First, as I have already pointed out above, the major-axes hypothesis refers not at all to the position domain. Second, and perhaps more important, it has long been known that curious effects occur when dots lie adjacent to long line segments. For example, Robinson (1972, p. 51) reports a 1954 experiment by Obonai (full details also given in Obonai, 1977, pp. 76-77) in which "subjects compared the distance between a pair of horizontally arranged dots having a vertical line between them with a pair of dots without a line. When the dots were up to 3 cm apart (viewed at 5 metres) and the vertical line was less than 1.5 cm the normal illusion occurred and the filled extent was overestimated. Increase of either measurement brought an inflexion to underestimation which reached a plateau when the vertical line was about 3 cm long. This was so for all intervals between the dots, that is, with a vertical line of 3 cm further increase of the distance between the dots made no difference."

In other words, when a small dot lies adjacent to a long line, the dot is frequently attracted perceptually *towards* the line, an effect which Obonai terms "spatial induction" and which he attributes to retinal irradiation (Obonai, 1977, p. 78). Recently, I have obtained data using dot alignments in a Poggendorff-type display, and these data also are suggestive of dot-to-line attraction effects (Wenderoth, 1980).

I have chosen not to take issue with Hartley on other, minor aspects of his reply. The main point to be emphasized here is that one may not be entitled simply to assert that "there is no a priori reason to suppose that either judgments of dot position or judgments of orientation would not provide an adequate test of theoretical predictions" (Hartley, 1979, p. 331). On the contrary, it is essential to distinguish between the domains of orientation and position. For, as Carpenter and Blakemore (1973) noted, angle

expansion due to lateral inhibition in the position domain would predict that an acute angle appears bowed, with most repulsion occurring near its vertex where the lines are closer in *position*. On the other hand, lateral inhibition in the orientation domain predicts a shift in perceived orientation of the lines, all of a piece, from their true orientations to other, perceived orientations.

In conclusion, then, Hartley would need to demonstrate that dot-to-line attraction effects had not confounded his results before he could conclude anything about theoretical predictions; but even then, it is not clear that any relevant predictions could be derived from the major-axes hypothesis in relation to the perceived position of a single dot. It is beyond the scope of the present note to discuss the other major issue that Hartley addresses, namely whether a hypothesis, which fails correctly to predict results in some circumstances, can nevertheless have considerable heuristic value.

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