The major-axis effect: Some comments on Hartley's experiment and analysis

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Hartley's (1978) recent paper invites comment for two reasons. First, it contains misleading, if not incorrect, assertions. Second, it fails to refer to publications containing data and analysis which are highly relevant to the issues addressed by Hartley.

The points to be made here require a brief summary of the effects under consideration. In a series of experiments, I measured errors in vertical settings of a thin line in the presence and absence of various background shapes. The test line generally was placed and pivoted centrally in, and coaxially with, the background-inducing figure (IF), and these IFs varied in both shape (e.g., outline square, cross, octagon, etc.) and in lateral orientation. Much of the data so obtained seemed to point to the relevance of the IF's axes of bilateral symmetry to the prediction of vertical setting errors. For example, consider the results obtained when the IF was a square outline frame which enclosed the test line. When the square was tilted by a small amount (e.g., 10° clockwise), the test rod *appeared* tilted in the opposite direction and so had to be *set* in the same direction as the square's tilt in order that the line should appear vertical. That is, the test line was set in the same direction as the 10° clockwise sides of the square. However, at greater IF tilts (e.g., 40° clockwise) errors were in the other direction: vertical settings now erred in the direction of the counterclockwise corners of the square rather than in the direction of the clockwise sides of the square. A simple way to *describe* this result is in terms of the square's axes of bilateral symmetry, shown in Figure 1.

Here, the axes of symmetry are numbered 1 to 4. For a 10° clockwise tilt of the square, Axis 1 is the closest to and clockwise of gravitational vertical; for a 40° tilt, Axis 2 is closest and counterclockwise. A description which therefore fits the above data is that the test line is set in the direction of the IF axis of bilateral symmetry which is closest to gravitational vertical. This is the so-called "major-axes hypothesis" put forward by Beh, Wenderoth, and Purcell (1971) and further supported by Beh and Wenderoth (1972) and Wenderoth (1973).

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Figure 1

As Hartley correctly points out, Wenderoth (1973), and incidentally, Wenderoth (1977) and Wenderoth and Beh (1977), suggested that the *explanation* of this major-axis effect might be found in neural interactions between orientation-selective cortical channels. That is, the axes of bilateral symmetry of many figures coincide with angle bisectors or midlines between contours. Thus, lateral inhibition propagated by the neural channels which are excited by the real lines of the IF might be expected to summate along virtual vectors like angle bisectors or midlines. Hartley described experiments designed to discriminate between (what he considered to be) these two opposing hypotheses: the bilateral symmetry hypothesis and the neural interaction hypothesis.

Before taking up any of the details of Hartley's paper, it is essential to clarify the nature of a logical error he made. Hartley showed observers a dot located somewhere within a shape, such as a square or a pair of opposite sides of a square. The task was to reproduce the apparent position of that dot on a second, blank sheet of paper. The general finding was that the reproduced position always erred towards the real lines of the IF and away from the nearest axis of bilateral symmetry. So, for example, a dot inside a square but left-of-center, was reproduced too far to the left on the test sheet. Hartley concluded as follows: "Studies requiring the subject to set a rod to the gravitational vertical find that the settings

deviate toward a virtual line when that is the nearest feature. The present study found that reproduced dots tended to be displaced *away* from virtual lines and toward physically present lines. Thus the possibility remains that judgments of the vertical in the context of a surrounding figure are affected by the axes of bilateral symmetry of the figure, while other judgments in the same context are affected by loci of neural interaction."

Here, Hartley implies that the vertical setting and dot setting results are opposite in direction; but he is wrong. In setting a rod to vertical, the observer sets the rod to *compensate* for the perceived effect, but in reproducing a dot's position, the observer places the dot to *represent* the appearance of the dot. In both cases, the result can be described by saying that the stimulus (line or dot) is displaced perceptually away from the axis of symmetry. It is merely that one method of measurement results in the observer's reproducing the appearance while the other requires the observer to offset that appearance. Hence, Hartley is simply incorrect when he states that "the bilateral symmetry hypothesis predicts that the apparent location of a point . . . should be displaced toward a real line, a figural contour, or an implicit line, an axis of bilateral symmetry . . . whichever is closer to the point" (p. 537). The correct prediction is just the opposite, and his data totally confirm it.

Let me now turn to the central issues. First, Hartley states that most of the figures used by us to investigate the major-axis effect are such that "the loci of presumed heightened neural interaction and the axes of bilateral symmetry have been identical . . . the present experiment investigates figures in which the loci . . . are not the same in order to separate their effects on perceptual judgments" (p. 537). Apparently, Hartley is unaware that the two such figures he chose (as well as others in the same class) have been used in my experiments and for the same reason: Experiments should be designed to permit falsification of a theory, not only confirmation. Results obtained with a pair of parallel sides of a square were discussed by Wenderoth (1977) and Wenderoth and Beh (1977), and the former also mentions data obtained with a three-sided amputation of a square frame.

In considering these data, it is essential to stress that I, unlike Hartley, do not consider that the major-axes hypothesis and the neural interaction hypothesis are hypotheses of equivalent status which are alternative explanations of the same data set. This point can best be made by quoting from Wenderoth (1977), who, after noting the *broad* agreement of *most* of the data with the major-axes hypothesis, stated: "While comparison of these 'predictions' with the angular functions . . . indicates an impressive degree of agreement, there are asymmetries in the functions which

are not predicted by the hypothesis . . . It follows therefore that the geometrical deviation from the vertical of the major axis nearest vertical is not sufficient to account entirely for illusion directions and magnitudes" (Wenderoth, 1977, p. 107-108). And later: "The aim of 'psychophysical' hypotheses such as the major axes hypothesis is to specify IF properties which are highly correlated with, and can therefore predict, the shapes of angular functions induced by those IFs. It is conceivable that the major axes hypothesis has failed to predict some angular functions because there are no simple geometrical properties of an IF which can be used to predict illusion functions. A possible reason is that orientation effects might correlate not with *geometrical* IF tilt but with *judged* IF tilt, as Gibson suggested originally . . . [and] there may be no simple, single geometrical or physical correlate of judged IF tilt. . . . It may be necessary . . . to take into account the types and combinations of figural axes at each orientation, the relative salience of axis deviations from vertical and horizontal, and any other factors which affect judged or perceived tilt" (p. 119-124). And finally: "In the last section the possibility of a correlation between judged IF tilts and the shapes of an illusion angular functions was discussed but there was no intention to suggest a deterministic relationship. It is likely, in fact, that a large part of the explanation of orientation illusions will be found in mechanisms of inhibitory (and perhaps excitatory) interactions between orientation-selective channels in the visual system" (p. 124).

In short, the "neural interaction hypothesis" is seen as an explanatory device, whereas the "major-axes hypothesis" attempts merely to specify correlates of perceived figure tilt which might allow prediction of illusion functions, without considering the mechanisms underlying those illusions. Thus, Wenderoth and Beh (1977) described the major-axes hypothesis, and more complex variants of it, as attempting to *define* the judged orientation of an inducing figure relative to a test figure.

Two other points can be made briefly. The fact that three-sided IFs, such as those used by Hartley, induce orientation illusion functions similar to those induced by a complete square (Wenderoth, 1977) indicates the inadequacy of the original formulation of the major-axes hypothesis, in particular its restrictive definition of "major axes" in terms of axes of bilateral symmetry. Indeed, recognition of this fact in part led to the discussion which has been quoted above. More recently, the illusion data have been more elegantly encompassed by considering, as the generic IF, a set of n intersecting gratings, of given spatial frequency, intersecting at angles of $180^\circ/n$ and Wenderoth and Beh (1977) and Wenderoth (1977) show how some data which cannot be accounted for

by the original major-axes hypothesis [e.g., the illusion function induced by the three-sided amputation of a square and the data reported by Wenderoth and Curthoys (1974), to which Hartley refers] are consistent with this more recent formulation. Indeed, Wenderoth (1977, p. 136) concluded that "the conceptualization of the RFI [rod-and-frame illusion] as a variant of the illusion produced by intersecting orthogonal gratings also suggested that it, and other intersecting grating illusions, might be explicable in terms of lateral inhibitory mechanisms. The neural basis for 'major axes' could then be inhibition, in the orientation domain, which is propagated by actual contours of the stimulus (mainline axes) or weaker inhibition which summates at orientations defined by angle bisectors."

Another point relates to comparisons between tasks which require orientation judgments and those which require dot alignments or dot positioning. Heywood and Chessell (1977) had subjects reproduce the distance between the endpoints of angle arms using dot markers. They obtained results which were not reconcilable with those obtained by others using methods of orientation matching with similar figures. We, too, have shown that dot alignment tasks do not reflect merely the perceived orientation of isolated lines or abutting lines (Wenderoth, Beh, & White, 1978a, 1978b; Wenderoth, White, & Beh, 1978). Thus, even had Hartley *not* been logically in error when he concluded that vertical judgments and dot position judgments gave opposite results, the different results would not necessarily have cast doubt on any theory of orientation judgments, for as has been shown in another context, opposite results can occur using two different matching methods which had hitherto been assumed to measure the same perceptual variable (Emerson, Wenderoth, Curthoys, & Edmonds, 1975). Furthermore, a test line placed

noncentrally in an outline frame would very likely be displaced perceptually, as were Hartley's dots; but such displacement would not necessarily imply anything about the perceived *orientation* of the line.

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