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Book Review Reply

On Cross Ratios and Motion Perception: A Reply to Niall

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Niall's (1987) review of my book *Perception with an eye for motion* (Cutting, 1986) is, I think, a wonderful critique. Few authors can hope to receive such a thorough and discerning review. Niall makes a number of points, some with regard to the history of ideas and some about the nature of perceptual theory to which I might respond, but the bulk of his comments is directed at my four chapters (6 through 9) on cross ratios and the perception of moving planar stimuli. I disagree with him on certain issues, but I acknowledge an important error.

Rigidity and planarity. Niall claims, correctly, that I do not explicitly make a rigidity assumption about the perception of planar objects in motion. He implies further, I think incorrectly, that this leads me into the following conceptual morass: One cannot use the cross ratio to prove rigidity without first assuming rigidity. My response, in the spirit of the theoretical framework of *directed perception* (my chapters 14 and 15), is that the visual system must simultaneously entertain many possibilities, rigid and nonrigid alike (see also Hochberg, 1986). Assuming an invariance hypothesis, an unchanging cross ratio is pretty solid evidence for the presence of a rigid set of four points on a line—or, as in my cases, four parallel lines on a plane.

Although not explicitly stated in my book, I have no problem with a Helmholtzian view that entertains the following deduction: if a moving object is rigid, and if a cross ratio measured on its elements is (within measurement error) unchanging, and if there are no nonrigid interpretations (simultaneously calculated) that are more plausible, then one will perceive the object as planar and rigid. This might be called an unconscious inference, but notice that it is written as a deduction, not an induction (see also Cutting, 1987).

Niall, however, thinks that even this is unacceptable. He suggests that cross ratios cannot be used to specify coplanarity because they can also be calculated on a circle. Whereas this is true, I claim it not relevant. Cross ratios on circles, despite a connection to cross ratios on lines (Schwerdtfeger, 1979), are *not projective*; they are part of a circles' analytic geometry and, I claim, a different beast with the same name.

Cross ratio syzygy. Niall notes that my discussion of cross ratios ignores the fundamental syzygy, in which the signed direction of segments is preserved and at least

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one segment will have a negative sign. My rationale for ignoring sign, although mathematically odd, seemed psychophysically justifiable.

First, pilot studies and inspection of many experimental trials suggested that the particular *value* of the (canonical) cross ratio did not matter in the detection of nonrigidity, only the *change* in its value. Nonetheless, I would argue that if the visual system registers a change in the cross ratio it must first do something akin to "calculating" its value (say .750), then monitor it throughout a trial. Second, I claim that there must be a region of values around it (say, between .725 and .775) within which no change can be discerned. If so, and my results are consistent with these ideas, both the value of the cross ratio and the direction of difference in the change of the cross ratio do not matter.

What are the consequences of ignoring sign? First, with respect to invariance, none: For example, using Niall's notation, $[x_{ij} \cdot x_{jk}]/[x_{ik} \cdot x_{ij}]$ and $[x_{ij} \cdot x_{jk}]/[x_{ik} \cdot x_{ij}]$ are both invariant under rotation and translation. Reversing the sign of one segment, although it changes the sign of the ratio, does not change its invariance. If one cross ratio is invariant, they all are—and they were in my experiments. Second, if I ignore sign, then the syzygy is experimentally unimportant. This may appear to do an injustice, but that injustice is mathematical esthetics, without importance to the empirical claim.

Arithmetic errors. Most importantly, and stemming from symmetries in the syzygy, Niall provides corrections to my Table 6.1 that are well-founded and welcome. The values in my book are, unfortunately, incorrect; this is a grievous error. An important question arises: What are the consequences of these errors? Aside from my embarrassment, I think they actually clean up the situation a bit. For all cross ratios now, experimental manipulations that shift an interior element (line j or k) cause two to three times the change in the cross ratio of equal-sized shifts of exterior elements (i or l). Thus, to change one cross ratio is to change them all in general proportion. More pragmatically, this means that some problematic sections in my book (pp. 87 and 89, and 123) could be eliminated. With Niall's sleuthing, I now claim that my experiments 1 through 8 test all cross ratios about equally well.

Whether my book is as deeply flawed as Niall suggests is, of course, not for me to say. What I can say is that it is a technical, experimental monograph written for students of visual perception that has now received a thorough and critical eye.

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