


In my initial commentary (Anderson, 2007a), I focused on the psychophysical data that provided evidence that reliable contours were neither necessary nor sufficient to predict contour interpolation and on evidence against the Kellman and colleagues’ identity hypothesis. In their replies, Kellman, Garrigan, Shipley, and Keane (2007a) and Albert (2007a) asserted that certain phenomena contradicted my views, most notably crystalline displays and crossing interpolation displays. I therefore focused my rejoinder (Anderson, 2007b) on demonstrating that the relevance of these phenomena in challenging my views is incorrect and articulated the theoretical principles that are missing from Kellman and colleagues’ model (e.g., Kellman, Garrigan, & Shipley, 2005; Kellman & Shipley, 1991) that are needed to understand such phenomena. In their postscript, Kellman, Garrigan, Shipley, and Keane (2007b) claimed that my position has changed and that some of my arguments have been abandoned. This is simply incorrect. Nothing in their replies significantly challenges my previous critiques of their model. Data now demonstrate that relatability criteria can be violated and completion nonetheless occurs (see Anderson, 2007b); that completion does not occur simply from the presence of relatable contours (as their promiscuous contour interpolation process asserts); and that relative depth imposes asymmetric constraints on how surfaces and contours are processed, which in turn underlie differences in how modal and amodal completion and/or continuation occur. Despite repeated assertions by Kellman et al., I have never claimed that luminance constraints block modal completion; rather, they merely weaken it, a fact that was discussed repeatedly since my first theoretical article on the topic (Anderson & Julesz, 1995; see also Anderson, 1997, 2003, 2007a, 2007b). On this issue, in his postscript, Albert (2007b) incorrectly asserted that my theoretical views do not predict that contours in glass (or crystalline) displays should be weaker. He failed to appreciate that my theory uses both binocular and monocular information and that both contribute to the construction of illusory surfaces and contours. In luminance conditions that do not support transparency, binocular and monocular information are in conflict (only binocular information signals their presence), which readily accounts for the weakened appearance of these contours. In their postscript, Kellman et al. (2007b) now concede that luminance constraints weaken contour completion and that this is consistent with their views, despite their claim in their previous reply (Kellman et al., 2007a) that contour completion was unimpeded in their crystalline displays. It is difficult to understand how to reconcile these inconsistencies.

Much of the defense of Kellman et al.’s position in their postscript is articulated in the form of arguments by assertion. Kellman et al. (2007b) assert that quasi-modal completion occurs, they assert that the fat–thin paradigm has been demonstrated to be sensitive to interpolation, and they assert that they have demonstrated that modal contours are amodally interpolated under occluding surfaces. I have argued that all of these assertions are false. For example, their claim about the existence of quasi-modal completion rests on two kinds of data: their phenomenology and performance on the fat–thin paradigm. The data that they provided from Gutman and Kellman (2005) showed that performance using outline stimuli is worse when observers do not receive any trials with filled Kanizsa figures. However, when such trials are intermingled, performance on the outline figures is elevated to the level of the filled figures. How can such effects be understood on the basis of interpolation? Kellman and colleagues would have to claim that the interpolation of contours in outline figures was somehow enhanced by exposure to filled figures to uphold their claim that this method taps into (unseen) interpolation processes. A simpler explanation is that once observers see the illusory figure in the filled inducer conditions, they realize that the task can be performed by imposing a mental (square) template on all figures with relatable contours (i.e., with figures that can be treated as deformations of a square template). Such strategies would obviously not work with misaligned or rotated inducers, so these control conditions do not provide any means of distinguishing between a template model of this kind or the interpolation model favored by Kellman and colleagues. In addressing the failure to observe modal contours emerging from behind occluders, Kellman

Postscript: Filling-In Models of Completion

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Received September 21, 2006
Revision received January 8, 2007
Accepted January 15, 2007
et al. argued that this failure arises because my displays contained two occluders along the interpolated contour path, whereas their quasi-modal displays contained only one. I believe that this explanation is incorrect. In Figure P1, I have constructed a variant of the figure I presented previously (in Anderson, 2007b), but in this example, only one of the stereoscopic occlusion junctions has been covered with an occluding disk (and, hence, is a quasi-modal display). When fused, a clear illusory bar appears to form at the (unoccluded) bottom of the figure, fades in clarity, and eventually disappears. Critically, however, no completion is observed to emerge from underneath the (single) occluding disk at the top of the image. Thus, Kellman et al.’s explanation that previously reported failures arose from the presence of multiple occluding surfaces appears untenable. Indeed, this demonstration provides phenomenological evidence that quasi-modal completion does not occur in the manner that they have claimed.

Both Kellman et al. (2007b) and Albert (2007b) continued to differ with me on the nature of the arguments involving the role of asymmetries in assessing models of modal and amodal completion (or continuation). First, Kellman et al., like Albert, asserted that because similar asymmetries can be observed with transparent and opaque versions of our cross display, this means that the effect does not have anything to do with modal and amodal completion. In my rejoinder (Anderson, 2007b), I articulated why this simple argument is wrong: The asymmetry is attributed to a difference in the way near and far depth signals are interpolated (that I have attributed arise from differences in the frequencies of unpaired features), which includes but is not restricted to modal and amodal completion. Kellman et al. (2007b) incorrectly claimed that

Anderson’s (2007b) backup position is a new theory that opacity and transparency are differences not in kind, but in degree. Such a claim may be reasonable, but it comes at a high price: This claim is incompatible with the idea of distinct modal and amodal completion processes (they would be matters of degree of a unified process). (p. 504)

First, this is not a new theory: It is the one that I have been developing for over a decade (Anderson, 1997, 2003, 2007a, 2007b; Anderson & Julesz, 1995). Second, the unification does not in any way embrace any version of Kellman and colleagues’ identity hypothesis. Indeed, the contrast depth asymmetry principles and the transmittance anchoring principle are the core principles of this theory, and they express asymmetric constraints on the way depth, lightness, and opacity are assigned to near and far surfaces and contours. This point continues to be a source of disagreement with Albert’s discussion of our serrated-edge demonstration. Here, too, it was the shift in relative depth that was critical, not the modal or amodal status per se. We agree on the border ownership explanation he favors, so the border ownership interpretation does not in any way contradict my arguments regarding this phenomenon.

Space constraints preclude a full response to all of the points of disagreement that remain, so a few concluding comments must suffice. Kellman et al. (2007b) questioned whether Singh’s (2004) data can be used to draw inferences about shape differences in modal versus amodal completion. There are at least four problems with their arguments. First, their attempt to dismiss Singh’s data on the basis of variability is ill-founded; Monte Carlo simulations with samples from a normal distribution of size 140 (the number of data points in Kellman et al.’s, 2007b, histograms in Figure P3A) show that between 4% and 11% of the sample points fall within 1 standard error of the mean. This is simply what one expects from the definition of standard error. Kellman et al.’s histograms in Figure P3A also mix together multiple sources of variability, including individual differences, within-subject variability, and two levels of turning angle with very different cell means; hence, their plots in their Figure P3B present the data in a manner that makes it essentially impossible to tease apart these different sources of variability. Figure P2 shows the data from individual subjects in a standard way that separates the influence of individual variability, within-subject variance, and turning angle. Second, as is also evident from Figure P2, Singh’s modal–amodal difference is robust across subjects; it is statistically significant in the reported direction for 10/14 and 9/14 of individual subjects, respectively, in Experiments 1 and 2 of his article. Third, Kellman et al. claimed that subjects use a second process—recognition from partial information (RPI)—to guide their judgments, and they used this to question whether observers are actually performing interpolation judgments. They argued that “as predicted, RPI acted predominantly in amodal cases” (Kellman et al., 2007b, p. 505). This is not what their model claims; they have explicitly asserted

![Figure P1](image-url)

**Figure P1.** A quasi-modal display demonstrating that the failure to observe contour completion emerging from under an occluding surface (here, a small disk) is not due to the number of occluding surfaces present. When fused (cross-fusers should fuse the left two images, divergers should fuse the right two), an illusory bar appears to form at the bottom junction that fades in clarity with distance from the occluder. No illusory figure appears to emerge from under the occluding disk at the top of the figure.
that RPI occurs only in the amodal case, which is contradicted by Singh’s data. Fourth, even if all of the responses that Kellman et al. attributed to RPI are removed (i.e., all corner responses), the modal–amodal difference remains statistically reliable in both experiments ($p = .01$ and .035 for Singh’s Experiments 1 and 2, respectively), directly contradicting their assertion that the reported results arise from contamination by such responses.\footnote{These arguments constitute the core source of disagreement that Singh has with Kellman et al.’s reanalysis of his data. More detailed information can be obtained by contacting Manish Singh directly at manish@ruccs.rutgers.edu.}

Anderson argues that “it is highly unlikely that the identical responses observed by Murray et al. (2004) have anything to do with contour interpolation processes” (p. 512). The basis of his argument is functional magnetic resonance imaging work by Stanley and Rubin (2003) on LOC [lateral occipital complex], in which similar activation of LOC was found in response to illusory contour stimuli and variants that disrupted clear contour perception by rounding tangent discontinuities. (p. 507)

This is only one aspect of my argument; the other is that these higher brain areas do not appear to have the kind of localized receptive field structure that are needed to support the kind of contour interpolation processes that Kellman et al. contended are occurring in these regions. It thus seems implausible that such brain regions can be the neural substrate for the kinds of computations hypothesized in Kellman and colleagues’ model.

\begin{figure}[h]
\centering
\includegraphics[width=\textwidth]{figure_p2}
\caption{Plots of individual subjects’ data from individual subjects in Singh’s (2004) Experiment 1. These plots reveal that the modal–amodal difference is robust across subjects; indeed, separate analyses of individual subjects’ data show that this effect is statistically significant in the reported direction for 10/14 and 9/14 of individual subjects, respectively, in Experiments 1 and 2.}
\end{figure}

References


