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Why Take the Cog Out of Infant Cognition?

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A number of recent commentaries on studies of infant cognition (e.g., Haith, 1998; Haith & Benson, 1998; Munakata, McClelland, Johnson & Siegler, 1997; Smith, 1999) criticized several reports by investigators (e.g., Baillargeon, 1999; Spelke, 1998; Wynn, 1998) who offered a so-called "rich interpretation" of findings gathered from young infants. The fundamental danger associated with such rich interpretations is that some simpler lower level mechanism may be discovered that can account for these same findings. This commentary is directed to three articles that appear in this issue of *Infancy* (Bogartz, Shinskey, & Schilling; Cashon & Cohen; Schilling). All three articles present empirical evidence from 4-, 5½-, and 8-month-old infants that the authors contend is more consistent with a perceptually based interpretation than with the rich interpretation offered by two classic studies of object permanence in young infants (Baillargeon, 1987a; Baillargeon, Spelke, & Wasserman, 1985). I argue that the alternative explanation offered by these three sets of authors is inconsistent with both the original studies and with their own data.

THE ORIGINAL "DRAWBRIDGE" STUDIES

In Baillargeon et al. (1985), 5½-month-old infants were habituated to a screen that rotated 180° (fore and aft), much like a drawbridge. No object (other than the screen) was present during habituation, and the now-standard infant-control habituation procedure was used. Both types of test trials involved the placement of an

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object behind the "hinge" of the drawbridge. For one type of test trial (possible), the drawbridge rotated only 120° because the box prevented any further rotation. For the other type of test trial (impossible), the drawbridge rotated 180°, appearing to adults to pass through the box. Baillargeon et al. reported that infants looked significantly longer to the impossible test trials than to the possible test trials.

Baillargeon et al. (1985) were well-aware of the following alternative explanation for this finding: "Infants might look longer at the impossible than at the possible event, not because they understood the underlying structure of the events, but because they found the 180-degree movement intrinsically more interesting than the 120-degree movement" (p. 196). To address this alternative, they conducted a control experiment in which an additional group of 5½-month-olds were habituated to the same 180° screen rotation, but the two types of test trials positioned the box to the side of the path of the drawbridge. Thus, both types of test trials were possible because the screen could not collide with the box, but the difference in screen rotation (120° vs. 180°) was the same as in the experimental condition. Average looking times were not significantly different in the two types of test trials, indicating that infants were not simply preferring the 180° rotation.

Baillargeon (1987a) extended these findings (with some subtle variations) to younger infants. Again, 4½-month-olds (and some 3½-month-olds) after habituation to the 180° screen rotation without the box present looked significantly longer to the impossible (180° with-box) test trials than to the possible (112° with-box) test trials in the experimental condition, but there was no significant difference in looking times to the 180° and the 112° rotations in the control condition.

THE CURRENT CHALLENGERS

As pointed out by Haith (1998), investigators who espouse a rich interpretation of findings on infant cognition "must fend off every possible *perceptual* interpretation" (italics added, p. 170). Bogartz et al. (this issue), Cashon and Cohen (this issue), and Schilling (this issue) argue that there are perceptual cues that can account for the Baillargeon et al. (1985) and the Baillargeon (1987a) results (henceforth referred to as Baillargeon and colleagues) without recourse to higher level cognitive explanations. Bogartz et al. is reviewed first because the infants were the same age (5½ months) as those in the original Baillargeon et al. (1985) study.

Bogartz et al. (this issue) employed the most complex design of the three articles because they argued that all possible combinations of habituation and test displays should be used to gain a fuller understanding of infant looking patterns. I do not agree with this design strategy when it involves habituation to impossible events because, in my judgment, that gets at a very different question; namely, can brief exposures to events that apparently violate the physical principles of our environment (e.g., magnets, Velcro) overcome the infant's developmental history

with "normal" physical principles? This is an interesting question, but not one that Baillargeon and her colleagues tackled in the drawbridge studies.[1] Thus, I limit my comments on Bogartz et al. to their two possible habituation conditions.

The results of Bogartz et al. (this issue) were quite clear. The infants who were habituated to the 180° display with no box looked longer at the 120° with-box (possible) test display than at the 180° with-box (impossible) test display. This condition fails to replicate Baillargeon et al. (1985). In addition, the infants habituated to the 120° display with the box present looked longer at the 180° with-box (impossible) test display than at the 120° with-box (possible) test display. Taken together, Bogartz et al. argued that the results of these two conditions can be accounted for by the encoding and discrimination of a simple perceptual cue: the novelty of screen rotation. In a follow-up experiment in which the number of habituation trials was fixed at either seven (similar to the mean in Experiment 1) or three, Bogartz et al. obtained evidence for a familiarity effect. That is, the infants habituated for seven trials to the 180° display with no box looked longer at the 120° with-box (possible) test display than at the 180° with-box (impossible) test display. Again, this is a failure to replicate Baillargeon et al. In contrast, the infants habituated for only three trials to the 180° display with no box showed the opposite pattern of looking on the test trials. Bogartz et al. argued that this apparent replication of Baillargeon et al. with fewer habituation trials is actually a familiarity effect to the 180° screen rotation because of poor encoding of the habituation display. In sum, Bogartz et al. failed to replicate Baillargeon et al. and they speculated that this failure to replicate may stem from a disproportionate number of "poor encoders" in the Baillargeon et al. study who showed a familiarity effect for the 180° with-box test display rather than an "impossibility" effect.

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[1]Rivera, Wakeley, and Langer (1999) investigated a speculation raised by Baillargeon (1987a, footnote 4) concerning the role of the habituation trials in the drawbridge experiments. As stated by Baillargeon (1987a), "the main rationale for including these trials was to familiarize the infants with the (presumably unfamiliar) movement of the screen. However, it could be that such familiarization was not necessary and that the infants would have responded in the same way had they received no habituation trials" (p. 658). Rivera et al. reported that, in the absence of any habituation trials, 5½-month-olds looked longer to four test trials of the impossible 180° with-box display than to four test trials of the possible 112° with-box display. However, Rivera et al. also reported that a separate group of unhabituated infants looked longer to the 180° than to the 112° displays when both were possible (i.e., the box was absent). Rivera et al. concluded that "These findings support the hypothesis that infants' longer looking at the apparently impossible 180° rotations is due only to simple perceptual preference for events that display more motion" (p. 433). There are three problems with this conclusion: (a) Showing that Baillargeon's (1987a) speculation about habituation trials was incorrect (i.e., that they may not be necessary) does not prove that the habituation trials are irrelevant; (b) repeated presentations of impossible displays may, via rapid learning, induce infants to interpret subsequent impossible displays as possible. Such a reversal of looking preferences was in fact reported for infants who viewed the 112° with-box display on the first pair of trials (see Figure 2 in Rivera et al.); and (c) in the habituation conditions that most closely matched the Rivera et al. study, neither Bogartz et al. (this issue, Experiment 1) nor Cashon and Cohen (this issue) replicated their results.

Schilling (this issue) extended the second experiment in Bogartz et al. (this issue) by testing 4-month-olds as in Baillargeon (1987a). In addition, infants in the 180° without-box condition were familiarized with either 7 or 12 trials. Again, the results were quite clear. The infants with limited (now defined as 7 trials) familiarization to the 180° without-box display looked significantly longer at the 180° with-box (impossible) test display than at the 112° with-box (possible) test display. This result replicates Baillargeon (1987a). However, infants in each of the other two conditions, familiarized to either 12 trials of the 180° without-box display or 7 trials of the 112° without-box display, looked longer at the possible 112° with-box test display. Thus, Schilling argued that the overall pattern of results strongly suggests that 4-month-olds are responding to the familiarity–novelty of the screen rotation and not to the possibility–impossibility of the test displays.

Finally, Cashon and Cohen (this issue) tested 8-month-olds with 2-D simulations of the 3-D habituation and test displays used by Baillargeon and colleagues. Infants habituated to the 180° without-box display looked longer to the 120° with-box (possible) test trials than to the 180° with-box (impossible) test trials. This result fails to replicate Baillargeon (1987a). However, these same infants did not look differentially at the two possible test displays (120° without box and 180° without box), thereby replicating Baillargeon and colleagues' control experiments.

CLAIMS, COUNTERCLAIMS, AND INCONSISTENCIES

Baillargeon et al. (1985) concluded that

> Our experiments provide evidence that by 5 months of age, infants … [1] understand that an object continues to exist when occluded, and that it exists not as a disembodied image residing somewhere behind the occluder but as a solid, three-dimensional entity occupying a specific spatial location … [2] understand that an object can move only through space not occupied by other objects. (p. 206)

In contrast, Cashon and Cohen (this issue) concluded that "infants responded to perceptual changes from habituation to test (i.e., changes in the amount of rotation and in the presence or absence of the block), rather than to the impossibility of the test events" (p. 442).

In my judgment, the design and interpretation of the experiments reported by Baillargeon and colleagues in their two articles were impeccable. They had a specific hypothesis, they habituated infants using the accepted standard paradigm, they included appropriate control conditions, and they had a clear result that was consistent across four experimental and three control conditions involving a total of 131 infants. The question we are now confronted with, based on Bogartz et al. (this issue), Cashon and Cohen (this issue), and Schilling (this issue), is whether

they could have been wrong. My tentative answer is "Perhaps, but I don't think so." Let me explain why.

First, there are several procedural and design issues that lead me to be skeptical of the strong claims made in Bogartz et al. (this issue), Cashon and Cohen (this issue), and Schilling (this issue) about the relevance of the familiarity–novelty factor in the Baillargeon and colleagues studies.

1. Why do these authors believe that Baillargeon and colleagues' results (longer looking to the impossible 180° with-box test display) can be explained by a familiarity preference? Baillargeon and colleagues used the same habituation criterion that was used by all three of these more recent studies (a 50% decrement across at least two sets of three successive trials). The only differences involved the maximum number of trials presented during habituation (if the criterion was not met) and whether all infants were included in the final sample if they did not meet this criterion. The number of nonhabituators in the experiments reviewed previously was very small (1 out of 43 5½-month-olds in Baillargeon et al., 1985; 3 out of 24 4½-month-olds in Baillargeon, 1987a, Experiment 1; 0 out of 28 3-month-old "fast" habituators in Baillargeon, 1987a, Experiments 2 and 3). Moreover, the average number of trials-to-criterion in these studies differed by less than one trial from Bogartz et al. The only sensible explanation, then, for the familiarity hypothesis is that the stimulus materials (e.g., color of the box, texture on the screen, overall illumination, etc.) were sufficiently different in the Baillargeon and colleagues studies that infants on average met the habituation criterion, yet failed to fully encode the key stimulus events. This seems very implausible. To be clear, I am not arguing that infants always show novelty effects. There is compelling evidence, both in the visual habituation literature as reviewed in these three articles and in the literature on auditory preferences (e.g., Jusczyk & Aslin, 1995; Saffran, Aslin, & Newport, 1996), that both novelty and familiarity effects can be observed. All three studies in this series provide compelling evidence that when the infant-control habituation procedure is not followed, resulting in fewer habituation trials, familiarity effects are obtained. What is not so clear is how relevant these familiarity effects are for an explanation of the discrepancy between these studies and those of Baillargeon and colleagues.

2. One potential perceptual variable that was not equated across the two sets of experiments was the size of the box hidden behind the rotating screen. In Baillargeon and calleagues' studies, the box had a depth of 5 cm or 10 cm, whereas in Bogartz et al. and Schilling, the box consisted of a piece of foam core board with a depth of 1 cm. Thus, it was not really a box at all and had to be supported by fishing line, much like a marionette. Similarly, the box in Cashon and Cohen was presented as a 2-D image on a television display. Thus, even if this box were rendered in 3-D using shading cues (which are not apparent in Figure 1a of Cashon & Cohen), there is binocular information specifying that the box is not a solid object (Cashon & Cohen raise this concern

in their Discussion section). The problem here is that infants may perceive the absence of thickness as information consistent with the collapse of the box when the rotating screen comes into contact with it. As a result, none of the displays would be impossible and the only sources of information remaining for the infants to attend to would be those that are irrelevant to the hypothesis examined in the Baillargeon and colleagues studies. Moreover, Baillargeon (1987b) provided evidence that by 7 months of age, infants are sensitive to the distinction between a hard (solid) and a soft (compressible) object placed behind the rotating screen, with only the solid object eliciting looking times consistent with impossibility.

3. All three articles leave the reader with the clear impression that their data are very similar in overall pattern across stimulus conditions and ages (i.e., preferences for larger rotations, preferences for a change in the box, and differential familiarity–novelty effects). However, there are some clear inconsistencies that are problematic. Bogartz et al. did not find a significant effect of the addition (or removal) of the box between the habituation and the test phases in Experiment 2 (and did not analyze for separate box and rotation effects in Experiment 1). In contrast, Cashon and Cohen found significant effects of both a change in the presence of the box and a change in the amount of screen rotation. Bogartz et al.'s Experiment 2 also revealed that in the 180° habituation conditions (those most similar to Baillargeon and colleagues' studies) there was no effect of a change in screen rotation to 120°. Finally, Cashon and Cohen found among the nonhabituators that looking was longer to the 180° with-box display than to the 180° without-box display. This finding is inconsistent with their (and Bogartz et al.'s and Schilling's) account in which nonhabituators show familiarization effects.

COMMON GROUND OR GROUNDS FOR DISAGREEMENT?

Is there any possibility of reconciling these seemingly contradictory findings, or is this debate among competing viewpoints likely to remain unresolved? Given human nature (showing my nativist tendencies), I do not have high hopes for an immediate reconciliation. However, perhaps some suggestions will help to move us toward resolution.

1. Try not to focus on a single experimental condition (or even a single experiment). Habituation studies are highly variable because looking times are influenced by many factors. Rather, pay attention to patterns of results across stimulus conditions and across experiments. This advice does not help much in this context because both sets of studies are based on many stimulus conditions and many experiments.

2. When attempting to replicate a phenomenon, perform the replication as closely as possible to the original, and then alter key stimulus variables that further test competing hypotheses. The use of "thin" boxes rather than stable 3-D boxes as

the occluded object raises a critical uncertainty about the Bogartz et al., Cashon and Cohen, and Schilling articles in this issue of *Infancy*.

3. Try to design definitive experiments whose results force only one conclusion. Admittedly, this is no easy task, particularly when limited to the use of looking times and the habituation paradigm. Like the three sets of authors, I too approach the interpretation of infant studies with a skeptical eye and prefer simpler, lower level explanations. But, in this context, the familiarity account of Baillargeon and colleagues' findings simply does not wash. Also, there are other problems of interpretation with each of the studies that weaken their claims. I may not be able to answer Haith's (1998) question about "who" put the cog in infant cognition, but, in my judgment, a portion of that cog remains viable until more definitive evidence can be mounted to refute the conclusions reached by Baillargeon et al. (1985) and Baillargeon (1987a).

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