INFANCY, *1*(4), 479–490

THEMATIC COLLECTION: RESPONSE

Reply to Baillargeon, Aslin, and Munakata

Richard S. Bogartz
Department of Psychology
University of Massachusetts, Amherst

Cara H. Cashon and Leslie B. Cohen
Department of Psychology
University of Texas at Austin

Thomas H. Schilling
Department of Behavioral Sciences
Fitchburg State College

Jeanne L. Shinskey
Department of Psychology
University of Denver

Our thematic collection relates to the nature of young infants' representation in specific situations involving occluded objects. Piaget (1954) concluded that the infant has no representations at this age. Most now agree that conclusion was unwarranted, but researchers differ as to what, if any, representations exist of occluded objects (Baillargeon, 1993, 1995; Bogartz, Shinskey, & Speaker, 1997; Haith, 1988; Leslie, Xu, Tremoulet, & Scholl, 1998; Meltzoff & Moore, 1998).

Obviously, issues concerning the nature of infant representation must be decided experimentally. It is therefore important to know how much confidence can

Requests for reprints should be sent to Richard S. Bogartz, Department of Psychology, University of Massachusetts, Amherst, MA 01003. E-mail: bogartz@psych.umass.edu

be placed in the existing studies, especially those supporting more extreme positions. This exchange regarding the larger theoretical issues takes place in the context of assessing the nature and importance of evidence from the drawbridge experiments in general and the frequently cited Baillargeon, Spelke, and Wasserman (1985) study in particular. Here, we respond to the remarks made by Baillargeon (this issue), Aslin (this issue), and Munakata (this issue).

BAILLARGEON'S COMMENTS

Habituation and Impossibility

Baillargeon (this issue) suggested that habituation to the impossible event confuses the infants, but no reason is given why they should be confused. It is also unclear in what way they should respond differently if they are confused. For example, would Baillargeon predict that they should look longer or shorter? In any case, the data do not support her suggestion. Neither Bogartz, Shinskey, and Schilling (this issue) nor Cashon and Cohen (this issue) found a difference on the habituation trials as a function of the impossibility when there should have been longer (or perhaps shorter) looking due to confusion.

An apparent inconsistency is that Baillargeon (this issue) claimed that infants should be confused, distracted, and so on by the impossible event during habituation but claims no such confusion or distraction on the test trials, only surprise. She gives no justification for why impossible events should confuse or distract infants at the beginning of an experiment, but only surprise them after habituation. Baillargeon (this issue) also suggested that habituation trials may not be needed. But, if they are not needed, then habituation trials in the Event Set × Event Set design would be equivalent to test trials, and these trials show no difference between possible and impossible events.

The Event Set × Event Set Method

Baillargeon (this issue) is correct that Oakes (1994) did use an Event Set × Event Set design in her study of infants' causal perception. Oakes went beyond merely asking whether infants distinguish between different categories because theoretical questions were embedded in her use of the design. But, the Event Set × Event Set design used by Oakes is not the Event Set × Event Set method (e.g., Bogartz et al., 1997). This new method entails mathematical modeling that enables different models to be explicitly pitted against one another in the study of infant perception and cognition. Specific hypotheses can be incorporated in the form of specific parameters and tests concerning these parameters can be per-

formed. Thus, the method goes well beyond simply using the same events for both habituation and test.

Consistency of Results

Schilling's (this issue) finding with 4-month-olds of a familiarity preference after 7 familiarization trials and a novelty preference after 12 familiarization trials is consistent with the Bogartz et al. (this issue) finding with 5½-month-olds of a familiarity preference after 3 trials and a novelty preference after 7 trials. Recall from Hunter and Ames (1988) that younger infants require more trials than older infants before they shift from a familiarity preference to a novelty preference. Also, potentially consistent with this pattern is the finding by Baillargeon, Kotovsky, and Needham (1995) that still older infants at 6½ months old prefer the impossible 180° rotation after just one habituation trial but not after multiple habituation trials.

Using Habituation Trials

Baillargeon (this issue) claims that habituation trials are not essential. She admits presenting limited exposure or familiarization trials in more recent studies. However, doing this may actually make it more likely that infants will respond to familiarity. It is not surprising that she says long habituation phases can be "counterproductive." This very well could increase the likelihood that infants would show a novelty preference.

Habituators Versus Nonhabituators

Baillargeon (this issue) is surprised that Cashon and Cohen (this issue) gave their infants up to 20 trials because "20 is a staggering number to use with older infants" (p. 457). Twenty habituation trials is not at all a staggering number of trials to use in a habituation study. Numerous studies have used that maximum. Furthermore, apparently Baillargeon's setting of the maximum number of habituation trials at 9 or 14 was not conservative enough—roughly 30% of her infants did not meet her habituation criterion (Baillargeon, 1987a, Experiments 2 and 3). Cashon and Cohen wanted to ensure that as many infants as possible would fully habituate. The most basic assumption of the experiment (i.e., after habituation, infants should prefer novelty) depended on it. This is also the reason why Cashon and Cohen separated the nonhabituators from the habituators.

To return to the issue of consistency, the fact that Cashon and Cohen (this issue) found a familiarity effect with 8-month-old infants after up to 20 trials should not

be seen as an inconsistency. Whether infants have been shown fewer familiarization trials (Baillargeon et al., 1995; Bogartz et al., this issue; Schilling, this issue) or whether they never reached the habituation criterion, or did reach the criterion but showed a preference for the familiar test trial (Cashon & Cohen, this issue), the conclusion is the same. Infants who have not been fully habituated, and as a result have a preference for the familiar rotation, appear to show a preference for the impossible test event.

Use of Animated Events by Cashon and Cohen

Many of Baillargeon's (this issue) criticisms seem irrelevant given that the nonhabituators in Cashon and Cohen's (this issue) study tended to replicate her major findings. Baillargeon (this study) suggested that computer-animated events may have been impoverished perceptually, so that the infants could not interpret them. Apparently, some animated events are appropriate given that Baillargeon (this issue) is willing to include as supportive evidence results from Lécuyer, Abgueguen, and Lemarié (1992), who also used animated events. We have not seen those events, but it would be interesting to compare how realistic the events used by Lécuyer were with those used by Cashon and Cohen. We believe the Cashon and Cohen stimuli, which are on file as QuickTime movies at http://www.infancyarchives.com, were quite realistic. However, readers can decide for themselves. Also, despite Baillargeon's (this issue) objections, Cashon and Cohen did in fact get results that were highly consistent with the other studies in this thematic collection that used real objects.

Spurious Claims

Space limitations do not permit detailed refutation of several incorrect statements by Baillargeon (this issue) concerning the Bogartz et al. (this issue) article and what its authors believe. We discuss here just a few.

Baillargeon (this issue) claimed that Bogartz et al. (this issue) "apparently believe that the experiments of Kellman and Spelke (1983), Baillargeon et al. (1985), and Baillargeon and Graber (1987) are the primary experiments to suggest that young infants can represent occluded objects" (pp. 447–448). What Bogartz et al. (this issue) actually said was "Some of the classic experiments (Baillargeon & Graber, 1987; Baillargeon et al., 1985; Kellman & Spelke, 1983) that have been widely assumed to demonstrate young infant representation of the occluded object have been recently receiving experimental attention and consideration of alternative interpretations" (p. 426). Clearly, we referred to those three experiments as some of the support and not as the primary experiments. They simply happen to be

the ones addressed first. One must begin somewhere. Baillargeon (this issue) had no basis for her statement that Bogartz et al. (this issue) fail to appreciate that there are many studies suggesting that young infants can represent occluded objects.

Baillargeon (this issue) claimed that Bogartz et al. (this issue) "believe that their experiments contain replications of the experimental and control conditions in Baillargeon et al. (1985)" (p. 450). We actually said that the experimental result was replicated (not the procedure or the control group) in the sense that the infants with a familiarity preference looked longer at the impossible event, as in Baillargeon et al. (1985). In context, our remarks are unambiguous and correct.

In general, Baillargeon (this issue) mischaracterizes Bogartz et al. (this issue) as questioning object permanence in young infants based on a limited awareness of the literature. Actually, Bogartz et al. (this issue) believed that their article was not the appropriate venue for the detailed treatment of numerous studies. We refer Baillargeon to Meltzoff and Moore (1998), who also consider Baillargeon and Graber (1987) and Baillargeon et al. (1985) to be important exemplars of literature taken to support object permanence, and who, after reviewing a variety of studies, conclude that object permanence does not occur in young infants.

ASLIN'S COMMENTS

Aslin (this issue) says that the design and interpretation of the experiments reported by Baillargeon et al. (1985) were impeccable. We believe that is a bit of an overstatement. Here we look more closely at aspects of the Baillargeon et al. design, analysis, and results.

Experimental Design

Even if one were to accept the adequacy of the control experiment, and assume it controlled for the fact that impossibility is confounded with familiarity and confounded with the greater amount of motion (Rivera, Wakeley, & Langer, 1999), the design is still seriously flawed. The amount of screen rotation time it takes for the same amount of the novel block to be exposed is also confounded with impossibility. On the impossible event, the block is occluded for 10 sec per cycle and visible for 2 sec. On the possible event, the block is occluded for 6 sec and visible for 2 sec. Their infants look for about 25 sec at the impossible event and 14.33 sec at the possible event. Thus, on the impossible trials, the infants, on average, look for about 2.08 screen rotation cycles, seeing the block for 4.16 sec; on the possible trials the infants look about 1.83 cycles, seeing the block for about 3.66 sec. The difference is about .25 cycles. The infants see the revealed novel block for about 4 sec in each condition. The obtained difference in looking time could have been due to the lon-

ger looking required on the impossible event trials to see the same amount of novel block as could be seen with shorter looking on the possible event trials. Naturally, this difference would not show up in the control experiment because the novel block was always visible on both types of trials.

Baillargeon (1987a) did not replicate the motion timing used in Baillargeon et al. (1985). Instead, a different confound was introduced. On the impossible events, the screen was in motion during eight-tenths of each rotation cycle, but on the possible events it was in motion on only five-ninths of each cycle.

The Control Experiment

It is not clear that the control experiment was run at the same time as the principal experiment. Infants were not assigned at random to one or the other of the two experiments. Procedures may have subtly changed, the observers may have been different, and their criteria may have shifted. We have no measures of comparability between the two experiments.

The control experiment is supposed to control for the simple effects of different amounts of screen rotation in the possible and impossible events, and therefore control against a screen rotation preference. We suggest that it did not. In Baillargeon et al. (1985), the block, always visible to the side of the rotating screen, may have distracted infants from the screen producing the lack of difference to the two rotations. Therefore, a better control condition for the effects of different amounts of screen rotation would be to test infants without a distracting block. In fact, in the control conditions of Baillargeon (1987a), the block was not present. However, another problem is created by that control. All infants were first habituated to the longer rotation and then tested on the longer rotation and the shorter rotation. Any initial preference for the longer rotation would be countered by the fact that the infants were habituated to the longer rotation. A more appropriate control would have been to counterbalance the two rotation events during habituation or even simpler, to test for a rotation preference without any prior habituation. Rivera et al. (1999) did this in their second experiment and found a preference for looking at the 180° rotation.

Statistical Analysis

In each study, repeated measure error terms were inappropriately pooled without justification or prior test of homogeneity (Bogartz, 2000; Winer, Brown, & Michels, 1991). Baillargeon et al. (1985) performed the crucial test using a pooled error term with 83 *df* when the appropriate error term would have had 20 *df*. In Experiment 1 of Baillargeon (1987a), an *F* that should have had 1 *df* and 22 *df* is re-

ported as having 1 *df* and 126 *df* and in Experiment 2, $F(1, 189)$ should have been $F(1, 35)$. A spurious increase in the error term degree of freedom inflates the size of the F ratio by decreasing the denominator mean square while decreasing the size of the F needed for significance.

Rivera et al. (1999) indicated that they "did not include infants who did not complete all eight test trials so as to avoid having to analyze the data using a missing-data algorithm and potentially inflating the degrees of freedom" (p. 428). In our opinion, this is what Baillargeon should have done.

Baillargeon (this issue) makes much of the failure by Bogartz et al. (this issue) to give multiple trials on the test events. But, it is that very trials factor and its interaction with events that provides the extra spurious degree of freedom. In Baillargeon (1987a, 1987b), she changed from three test trials to four. This made incomplete data more likely, which would result in pooling of the error terms and also of further increasing the number of degrees of freedom that would be incorrectly added.

The Results

Are the results as clean and neat as Aslin (this issue) and Baillargeon (this issue) suggest? Rivera et al. (1999), in discussing the results of their Experiment 1, noted that

> The order effect (i.e., infants looked longer overall at the impossible 180° event only when it was presented first) found, for example, by Baillargeon (1987a, 1987b) and Baillargeon et al. (1985) was robustly found in our data as well … what is being found is not a general preference. It is particular to one order, that is, a preference for 180° rotations when they come before 112° rotations. (p. 431)

But note that the order effect in Baillargeon et al. (1985) was only for the control condition, not the experimental. In discussing their Experiment 2, Rivera et al. stated,

> As in Experiment 1, we found a significant order effect. Specifically, only infants who saw the 180° rotation first looked longer overall at the 180° rotations. Baillargeon et al. (1985) reported a similar finding in their control study … Therefore we cannot accept Baillargeon's (1987a) dismissal of the order effect as 'theoretically uninteresting.' (p. 433)

Because order was balanced in Baillargeon (1987a, 1987b), only half of their infants in the experimental conditions showed the claimed effect. In effect, the result with the impossible event occurring on the first trial could not even be replicated by Baillargeon (1987a, 1987b) using her own procedures, if the possible event occurred first—a fragile result indeed.

Aslin (this issue) ignored one third of the design in Bogartz et al.'s (this issue) first experiment and one fourth of the design in Cashon and Cohen's (this issue) experiment (the infants who were habituated to the impossible event) by begging the theoretical question at issue. Aslin claimed that habituating infants to impossible events asks the question "Can brief exposures to events that apparently violate the physical principles of our environment … overcome the infant's developmental history with 'normal' physical principles?" (p. 464). He believes this does not relate to Baillargeon's question in the drawbridge studies. But, of course, one of the major issues to be tested is whether the infants are at all sensitive to these violations of what adults take to be physical principles.

Bogartz et al. (this issue) and Cashon and Cohen (this issue) found two critical results by habituating the infants (between participants) to all of the stimulus events used in Baillargeon et al. (1985) and not just to one of them. First, the infants took no longer to habituate to the impossible event than to the possible event. If the impossible event is violating a physical principle known to the infant, the infant should be surprised, look longer, and take longer to habituate. This does not occur. Baillargeon (this issue) suggested that habituation trials may not even be needed. But, if they are not needed, then, as we mentioned earlier, the habituation trials may be viewed as test trials and these test trials do not reveal longer looking to the impossible event.

Second, Bogartz et al. (this issue) found symmetry in the table of means for the test trial looking times. This result, also found repeatedly, shows that the mean looking time to Event A following habituation to Event B is the same as the mean looking time to Event B following habituation to Event A, regardless of which, if either, of the events is possible or impossible. The result strongly suggests that the looking time concerns a symmetrical relation between the stimulus event in perception during the test trial and the stimulus event placed in memory during the habituation trials. This result has been noticed before (Rose, Jankowski, & Senior, 1997). The result is implied by the comparison theories used by Bogartz et al. (this issue), Cashon and Cohen (this issue), Schilling (this issue), and Meltzoff and Moore (1998). There is no obvious line of reasoning from the Baillargeon (this issue) or Aslin (this issue) position that would imply this reliable result.

MUNAKATA'S COMMENTS

Munakata (this issue), although acknowledging the importance of our studies, suggested that we failed to address a critical control condition, failed to replicate the original findings, and mischaracterized conceptual accounts. We have already discussed the control condition at length. We respectfully disagree with Munakata in that we believe that in each of the articles, we replicated the critical finding of longer looking at the impossible screen rotation. We agree that we did not replicate the

exact experiments performed by Baillargeon et al. (1985), but we disagree as to how crucial it is that we do so. See the discussion of replication that follows.

Pretesting With the To-Be-Hidden Block

Munakata (this issue) claims that in the original studies infants "first had to look at the block for 2 sec before the event started," says that none of our studies appeared to apply this criterion, and then suggests why this may be critical (p. 474). We believe that Munakata is in error so far as the Baillargeon et al. (1985) study is concerned. Baillargeon et al. indicated no such 2-sec look at the block in their description of the possible or impossible events. They did indicate that "After the habituation phase, the yellow box was introduced into the front alley. Infants were given two 3-sec pretest trials to call their attention to the presence of the box." Bogartz et al. (this issue) indicated that

> When the computer signaled the end of the last habituation trial, the curtain dropped and two pretest trials were presented. The pretest trials are included because they were included in the experiment by Baillargeon et al. (1985). In the pretest trials, the curtain was raised to reveal the red block standing alone. The screen lay flat against the floor of the alley with the block clearly visible behind it. The curtain was dropped when the infant had been judged to look at the block for 3 cumulative sec. (pp. 410–411)

Munakata's claim that the infants may use the time before the drawbridge starts moving to form predictions that allow them to recognize the impossibility of events is an interesting conjecture, but it does not relate to a methodological difference between the Baillargeon et al. study and the Bogartz et al. (this issue) study.

Revisiting Baillargeon's Control Condition

Munakata (this issue) argued that perceptual processing accounts cannot readily explain the findings of the Baillargeon et al. (1985) control condition. We disagree. There is evidence in Cashon and Cohen's (this issue) nonhabituators' analysis that although infants did not look significantly differently at the two control condition test events, there was a significant preference for the two longer rotating test events over the two shorter rotating test events. In fact, Figure 4 of Cashon and Cohen shows that, descriptively, the mean looking time at the 180 no block is longer than the mean looking time at the 120 no block test event. In their original analysis as reported in the article, this comparison was not significant. However, when they compared the 180 no block mean to the 120 no block mean in a simple F test, the difference approached significance, $F(1, 9) = 3.979$, $p < .08$. That trend plus the failure to

obtain a significant Block × Rotation effect all indicates that the nonhabituating infants are showing a familiarity preference for the 180° rotation. Baillargeon et al. (1985) failed to statistically analyze the data for a main effect of rotation across experimental and control conditions. Perhaps if she had, she too would have found an overall preference for the longer rotation.

Conceptual Versus Perceptual Accounts

Finally, Munakata (this issue) suggested, without documentation, that a weakness of our studies is that they mischaracterize conceptual accounts as positing only conceptual factors, and suggested that proponents of conceptual accounts are in fact concerned with the importance of familiarity. We disagree. First of all, in the Event Set × Event Set method we set forth a model that allowed for both a conceptual factor and for factors involving relations between the habituation events and the test events. In the largest model we considered, both types of factors could have played a part. We did not claim that if both factors played a part, this would disprove the conceptual account that allowed for only the conceptual factor, nor did we believe this to be true. What we did say was that if the parameter representing the role of the conceptual factor of impossibility did not contribute to description of the looking times, this would be evidence against the conceptual account, and this is what we found.

Our criticism of some approaches is not that familiarity is ignored but rather that an automatic novelty preference is assumed for all participants so far as the test events are concerned relative to the habituation or familiarization event. Roder, Bushnell, and Sasseville (this issue) show unmistakably that infants do not shift immediately into a novelty preference after exposure to an event but instead that this is a gradual process and begins with a familiarity preference, supporting our work. We believe that the Event Set × Event Set method provides an important correction to this bias by allowing for novelty–familiarity preference to be assessed for each infant rather than taken for granted to always be novelty preference. Alternatively, one can use a strict habituation criterion and separate out nonhabituators from habituators as Cashon and Cohen (this issue) did.

A FINAL WORD ON REPLICATION AND PARSIMONY

Baillargeon (this issue) asserts that our three articles fail to replicate her initial studies. In all three articles, we reported results that replicate the main finding in her studies, albeit under certain circumstances. The key concern for the familiarity hypothesis is under what conditions we replicated her findings. Under conditions indicating a familiarity effect (either limited exposure or nonhabituators), we did rep-

licate her findings. Under conditions suggesting a novelty effect, we did not replicate. These results from all three laboratories undercut Baillargeon's (1987a, 1987b; Baillargeon et al., 1985) basic assumption in these studies that if infants did not know the event was impossible, they would prefer to look at the possible test event because it was more novel. We found in all three laboratories that this assumption is questionable. Were infants in her studies fully habituated and thus "should" have had a preference for novelty? We believe we have demonstrated that infants looked longer at the impossible event, not because it was impossible, but because they preferred the familiar rotation.

The reviewers make much of the procedural replication. Replication was impossible with respect to the Baillargeon et al. (1985) and Baillargeon (1987a, 1987b) studies given that they varied among themselves. We did not set out to replicate the procedures exactly. We felt that our manipulations fell into a reasonable range in which the claimed phenomena should occur. We find the suggested reasons why our procedures may have resulted in nonconceptual approaches by the infants to be flimsy at best. Replication of the results was unlikely in the light of the fragility of the results. Baillargeon (1987a, 1987b) herself could not replicate the result when she changed the order of the events on the test trials to possible first.

Baillargeon (this issue) interpreted numerous studies as supporting the object permanence inference and suggested that this body of work must all be engaged at once if the inference is to be doubted. Our approach has been more circumscribed. We agree that the most satisfying conclusion will be when all of these studies are well understood. We believe that considering one area at a time is the only practical way to go about the empirical side of the matter. We share this approach with Rivera et al. (1999), who also first approached the drawbridge work and then the arithmetic work. (Baillargeon, this issue, correctly anticipated a failure to replicate the arithmetic work. See Wakeley, Rivera, & Langer, in press.) For a broader, more encompassing approach that also rejects the object permanence claim, see Meltzoff and Moore (1998).

Baillargeon (this issue) argued that a "single coherent explanation of large bodies of findings" (p. 448) is more parsimonious than separate post hoc explanations for each individual experiment. We agree. But, just as the specific perceptual explanations for each study vary across studies (as the objects and events differ), so too does the specific conceptual knowledge that is supposedly demonstrated. There are different conceptual explanations just as there are different perceptual explanations. So, why is an explanation of different findings that is based on conceptual processing any more parsimonious than an explanation based on lower level perceptual processing? In fact, given either perceptual explanations or conceptual explanations for a set of experiments, a lower level perceptual explanation would appear to be more parsimonious. The perceptual explanation is clearly more parsimonious if one includes in the set of studies to be explained the vast literature on infant habituation, memory, attention, and perception.

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