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Reply to Commentary

Kind matters: A reply to Samuelson and Perone

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In the article that is a subject of Samuelson and Perone's commentary, we reported the results of a study in which 15-month-olds were presented with novel target objects that possessed a nonobvious property—a novel sound. We found that infants attempted to elicit the novel sound on test objects that matched the target object in shape, more so than on test objects that matched the target in color or texture. Critically, this pattern of behavior emerged only in a condition in which none of the test objects could actually produce a sound. We concluded that this selective generalization to shape-matched objects reflects infants' expectation that objects similar in shape share nonobvious properties. In their commentary, Samuelson and Perone (S&P) challenged our interpretation on both empirical and theoretical grounds. In what follows, we respond to these challenges by first addressing incorrect representations of our findings and then discussing the theoretical challenges presented in their commentary.

A full consideration of the data

S&P present a reinterpretation of the presumed findings reported by us (G&D), arguing that our interpretation of what children represented in the task was incorrect. Based on their account of our findings and prior work by Perone and others, S&P argue that infants may have been simply associating "the particular shape of the object with the particular action performed on the object." That is, they argue that infants did not form a mental representation of the object as possessing the nonobvious property.

In response to these claims, we must first point out that S&P do not accurately report our findings. They summarize these findings as demonstrating that "infants more frequently reproduced target actions in the unpredicted condition and those actions were more likely to be performed on test objects that shared the same shape with the target object than test objects that shared either color or texture." Later, in presenting an alternative interpretation of our findings, S&P declare that "infants may then reproduce the target actions on novel objects more frequently in the unpredicted than the predicted condition. . ." This is incorrect. Infants did not reproduce actions more frequently in

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the unpredicted than in the predicted condition. In fact, they acted on the objects more frequently in the predicted condition, where the test objects possessed the same property as the target objects. Furthermore, infants performed more target actions on the shape-match object versus the color-match and the texture-match objects *only* in the unpredicted condition. There were no differences in target actions performed on the different test objects in either the baseline or predicted conditions. This is a crucial distinction because it nullifies S&P's claim regarding what infants in our study learned. That is, if infants had only formed an association between the shape of the object and the action performed on it, they should have privileged shape over color and texture in the predicted condition as well.

Moreover, S&P's interpretation of our findings fails to provide a convincing account of why infants in our study privileged shape over other perceptual properties in the unpredicted condition. That is, if infants were simply forming associations between actions and objects, why would they have differentially persisted in performing the action on the shape-match test object in that condition? After all, all test objects presented to infants afforded the action (as infants demonstrated in the predicted condition). Thus, counter to S&P's account, our findings indicate that the only aspect of the unpredicted condition that was "surprising" and thus somewhat bothersome to infants, was the lack of *sound* in the shape-match test objects. For this reason, we concluded that infants expect same shape objects to also share nonobvious properties, suggesting that they appreciate that shape similarity is a reliable cue to category membership.

Finally, we disagree with S&P's characterization of how findings by Perone and colleagues contradict our interpretation of our results. First, it is important to note that the studies by Perone and colleagues described in S&P's commentary involve 10-month-olds, infants five months younger than those in our experiments. Second, the methods employed are visual habituation paradigms using videotaped events, in contrast to our interactive manual exploration paradigm. There is a long history of debate regarding the possible differences in categorization processes that are engaged in looking versus interactive tasks (Mandler, 2000). Thus, the applicability of findings from studies involving infants so much younger than ours and from such different paradigms as ours is questionable. Nevertheless, we welcome the opportunity to address the issues raised by S&P.

In Perone and Oakes's (2006) Experiments 3–5, on which Panel E in S&P's Figure 1 is based, infants were habituated to videotaped stimuli with variations on two dimensions. Infants failed to learn relations between particular actions and sounds or between objects and particular sounds. Importantly for the present argument, the combined manipulations of two dimensions used in these experiments were more complicated and very different from what infants in our experiment had to process. A more relevant assessment of the types of relations our infants might have formed would be to habituate infants to stimuli with one value on each dimension, to see if they dishabituate equally to changes in each of the three dimensions. In fact, when presented with such an assessment in Perone and Oakes's Experiments 1 and 2, 10-month-olds did dishabituate to changes in either action or sound. That is, infants were surprised if an object similar in shape to the habituation object did not squeak when squeezed, or if it squeaked when rolled. Thus, in a task similar to ours, 10-month-olds did code the association between appearance and sound, and between appearance and action. Furthermore, Perone, Madole, Ross-Sheehy, Carey, & Oakes (2008) found that 7-month-olds were more surprised when an experimenter performed a novel action with sound on a similarly shaped object, than when the experimenter performed the same action with sound on a differently shaped object (action and sound were confounded). Thus, consistent with our findings, 7-month-olds expected two similarly shaped objects to be acted on similarly and produce the same sound. That is, many things can be squeezed and then squeak, but if something does so, that is all it does! This finding is consistent with our conclusions—shape-based categories determine the type of property an object possesses. Our study suggests that infants recognize this by 15 months of age, and others have found that 2-year-olds do so even more explicitly (Casler & Kelemen, 2007).

S&P conclude that explanations of early categorization behaviors that fail to fully consider what is revealed by behavioral measures can lead to unwarranted assumptions regarding infants' representations. We could not agree more. What is crucial, of course, is first accurately portraying the evidence.

The necessity and logic of kinds

In the second part of their commentary, S&P question the need to postulate that infants hold a concept of kind. One can divide their argument into three claims: (a) infant/child data can be explained without postulating kinds; (b) data similar to those produced by infants/children can be simulated in entities that arguably do not have conceptual representations of kinds; and (c) the postulation of kinds creates a logical conundrum. We dispute each of these claims.

First, there is a vast literature on how a conceptual representation of kinds underlies infants' and children's responses in categorization tasks (Booth & Waxman, 2008; Gelman, 2003; Markson, Diesendruck, & Bloom, 2008, for reviews). One particularly powerful demonstration comes from Booth and Waxman (Booth & Waxman, 2002; Booth, Waxman, & Huang, 2005). They demonstrated that children's criteria for generalizing a name to other objects vary depending on object kind. That is, if children were told that certain objects were animals, they generalized the name of a target object to other test objects based on their similarity on visual dimension *X*. If children were told that the same objects were inanimates, they generalized the name of that target object to other test objects based on their similarity on a different visual dimension. (See Graham et al., in press, for similar findings.) Another relevant set of studies demonstrates how children's naming of a novel object or symbol depends on information children received or inferred regarding the intentional creation of the object/symbol. For instance, a splash of paint that looks like a sun will be called "sun", if it was intentionally drawn by someone (Bloom & Markson, 1998; Gelman & Bloom, 2000; Gelman & Ebeling, 1998); an object shaped like a novel object will not be called by the novel object's name if it is a container for that novel object (Diesendruck, Markson, & Bloom, 2003). Finally, recent work with infants has demonstrated that infants make purposive generalizations based on *kind-relevant cues* (i.e., shape and/or labels), but not based on other types of similarity cues. For example, Dewar and Xu (2009) demonstrated that 10-month-olds expect that objects that share a label will share an internal property, regardless of perceptual similarity or dissimilarity. Similarly, a number of studies from our laboratory have demonstrated that when objects are labeled with shared count nouns, infants will generalize nonobvious properties to objects that differ significantly in shape (Graham & Kilbreath, 2007; Graham, Kilbreath, & Welder, 2004; Welder & Graham, 2001). Moreover, 16-month-olds rely on shared labels to guide their inductions only when those labels are presented within a naming context that clearly marks them as count nouns (Keates & Graham, 2008). In other words, infants do not infer that two objects with the same shape or same label inevitably have the same hidden properties. Rather, they use these cues to guide their inferences only when these types of cues provide guidance regarding the object's category or kind.

It is clear in these studies that visual information, task demands, and context do not suffice to explain children's behavior. One could in fact read S&P's position as consistent with this statement. They propose that "categorization behaviors emerge in a moment as prior knowledge is brought to bear in a task by relevant perceptual cues." So, clearly, "prior knowledge" is crucial to categorization. Indeed, proponents of kind-based accounts take as a central goal to establish what prior knowledge children have about different ontological kinds, language, and the possible interactions between them. Of course, these accounts do not deny the importance of children's capacities to detect statistical regularities in the input for a complete explanation of conceptual development. They simply maintain that children's concepts cannot be solely the product of associative learning (Waxman & Gelman, 2009).

S&P reinforce their claim regarding the lack of utility of "kinds" by describing a word-learning robot. They conclude that if a robot can succeed in word learning without a representation of kinds, there is no need to posit kinds. This argument reminded one of the present authors (GD) of a lecture given in a cognitive science colloquium by a computer engineer. The engineer described the marvels of a connectionist network that, "analogously" to mice in a food-searching procedure, was capable of developing a spatial representation of a maze. At the end of the lecture, a comparative psychologist asked the engineer how well the computer program performed compared to mice. The engineer replied: "It did much better!". The engineer's smirk when reporting the program's performance likely reflected his attitude that he was not trying to simulate mice; he was solving a computational problem.

We argue that the comparison between the word-learning robot and the infant is not germane to the discussion of the relevance of kinds. We cannot induce identity in process from similarity in behavior. S&P are quite likely aware of this fact, as they specify that the robot in their example takes "input *similar* to that presented to children." One wonders, however, what "similar" truly means. Was the robot fed verbs, prepositions, adjectives, full sentences to parse? Did it get actions, movements, internal states, multiple visual perspectives? Did it get input that was not directed at it? Does it know anything else about the world besides what it was fed during the learning trials? Does it grow? Does its hardware mature? Is it the outcome of natural selection? Surely, the input and environment were not as varied as that experienced by a 15-month-old infant. To ascertain with confidence that the computer simulation is a valid rendering of what children experience – both in terms of the input each is exposed to and the computational/representational machinery at their disposal – seems to us a much bigger leap than the one presumably we are making by arguing that children's brains can represent abstract concepts.

Which brings us to the last part of their argument: "Such [kind-based] accounts are also particularly problematic for our understanding of development because explaining infant behavior as resulting from an understanding of "kinds" or "conceptual understanding" begs the question of where an understanding of kinds or concepts comes from." (p. 7). The inference S&P seem to draw from this conundrum is that if we do not know how to explain X (i.e., an understanding of kinds), then X must not exist. But the history of science is full of examples that prove this inference wrong. In fact, many would say that that is exactly the function of scientific theories: you posit the existence of a concept X to explain a certain phenomenon because it is the concept consistent with most of the observations regarding the phenomenon. Backed by this accumulated body of evidence, you then pursue an explanation for the concept X. This was true for atomic physics and for genetics, so why not for psychology? S&P view the "begging" question of where an understanding of kinds or concepts comes from as a fatal blow to kind-based accounts; we see it as one of the fundamental functions of scientific theories. In fact, advocates of kind-based accounts have indeed begun to posit the learning mechanisms that may lead to the development of kinds (see Xu, Dewar, & Perfors, 2009).

Final thoughts

We believe it is time to move beyond theoretical reinterpretations of data, and provide data that directly address alternative accounts (see exchange between Samuelson & Smith, 1998, and Diesendruck, Markson, Akhtar, & Reudor, 2004, for an example). We believe that our study accomplishes this goal. It contrasts two alternative accounts, which, at least at some point in their formulations, made two very clear and different predictions. One account claimed that the shape bias should appear initially, and primarily, in lexical extension tasks because it is the product of second-order generalizations based on the frequency of correlations found in the linguistic input. The other account claimed that the shape bias may not be lexically specific, because a notion of conceptual similarity is primary, including a realization that shape is the best predictor of such similarity. Translating our theoretical positions into empirically testable and viable alternative predictions is a necessary step if we are to seriously engage in such theoretical debates. If we each keep to our corners and produce empirical tests that are impenetrable to advocates of the other position, then no real scientific progress will be achieved.

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