The Conceptual and Empirical Case for Social Evaluation in Infancy

Commentary on Tafreshi, Thompson, and Racine

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Key Words

Helpers · Hinderers · Infancy · Looking time · Preferential looking · Social evaluation

Tafreshi, Thompson, and Racine [this issue; hereafter TTR] critique the use of preferential looking paradigms in empirical examinations of infant social and moral cognition. Their analysis has two main points. First, TTR argue that preferential looking paradigms lead to incorrect conclusions about the nature of infants' minds in general due to researchers' conceptual confusion with what patterns of attention can and cannot tell us. Second, TTR claim that, due to this confusion, to use evidence gleaned from preferential looking paradigms to attribute high-level social and/or any-level moral capacities to infants is inappropriate. It may not surprise the reader to hear that I will be arguing against each of these points below: First, I will address the notion that infancy researchers are confused by what attention data can tell them; and second, I will address the specific claims made about the evidence for sociomoral evaluation in infancy. Despite this, I do appreciate respectful scientific debate and absolutely concur that there must be conceptual and empirical clarity within developmental psychology and any other scientific discipline. I am simply unconvinced that the picture of infant cognition research is nearly as bleak as TTR suggest.

Researchers Are Confused about What Preferential Looking Paradigms Can Tell Us about Infants' Minds

TTR claim that infancy researchers, in particular those who ascribe core knowledge (CK) to the human mind, readily use preferential looking tasks to attribute multiple different capacities to infants based on the very same behavior – more looking

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to stimulus A than to stimulus B. They imply that CK researchers use preferential looking tasks sloppily, without the necessary conceptual work to understand what such tasks can really tell us about infants' minds. They say, "The very same behavior can be said to show, depending on the paradigm, surprise, preference, or anticipation," and "theorists could just continue to reinterpret the same empirical results in wildly different ways."

Any infant researcher, including myself, would happily cede that "more looking" or "change detection" could reflect a large set of underlying processes, motivations, etc., and I absolutely agree with TTR that to use such broad categories of behaviors to attribute very specific sorts of knowledge to infants without further constraints on interpretation would be entirely unjustified. What I disagree with is TTR's suggestion that CK researchers (or any others) actually do this. There are very clear boundaries between various looking time techniques. Each one is clearly appropriate in some circumstances to explore certain empirical questions and inappropriate in others. Researchers do not pick methods willy-nilly in a search for $p < 0.05$ and therefore attribute infants competence inappropriately. Although a lesson on the intricacies of various attentional paradigms is outside of the scope of this commentary [and excellent reviews and analyses are available elsewhere; Aslin, 2000, 2007; Colombo & Mitchell, 2009; Kidd, Piantadosi, & Aslin, 2012; Schöner & Thelen, 2006; Sirois & Mareschal, 2004], CK researchers do not reinterpret the same empirical results in wildly different ways but use very specific types of "more looking" to claim surprise, to claim anticipation, to claim preferences for novel or familiar stimuli, and to claim evaluative preference over and above novelty or familiarity.

Surprise is indicated by both habituation-dishabituation and violation of expectation (VOE) methods and is inferred when infants look longer after they have viewed an event inconsistent with what has come before (stimulus A) than they do after they have viewed an event that is consistent with what has come before (stimulus B). Events A and B are presented in alternation, and they are never visible at the same time. The "surprise" could be from many different things (researchers carefully manipulate what differs between A and B to ask their questions) but is akin to when adults stare longer at something unanticipated, something that does not make sense to them, or when there is "something fishy" going on. In contrast, anticipation or prediction is indicated when infants look, during an event, to a very specific location at a very specific time, just before something is likely to happen at that location. Doing so suggests that infants predict what an object or individual is likely to do before their action occurs; predictive looking is a paradigmatic aspect of adult attention. Surprise and prediction measures are alike to the extent that they involve attention during or after single unfolding events, but they have clear differences: One involves looking to specific locations within a scene, and the other does not; one involves temporal information regarding when an infant attends somewhere, and the other does not.

Preferential looking paradigms are considerably different from either VOE or predictive looking measures, since they measure *relative* attention to one of two stimuli when they are presented side by side. If infants look more to stimulus A than to stimulus B during a set period, it is inferred that they prefer stimulus A (for some to-be-determined reason). Sometimes, this preference is considered reflective of novelty (or, in certain cases that are well understood, familiarity); other times it suggests that one stimulus is perceptually more complex or interesting; and, when A and B are

equally complex, familiar, and/or novel, it suggests some higher form of evaluative preference, although the exact nature of that preference must be elucidated empirically. TTR claim that preferential reaching and preferential looking are exactly the same thing, although it is unclear why. Reaching has been the gold standard of “active” behaviors reflecting actual cognition in infancy since Piaget, and preferential looking and preferential reaching can be distinguished empirically in relation to infants’ social preferences [e.g., Kinzler & Spelke, 2011]. That said, as long as stimuli are presented together and an individual is given the opportunity to “choose” between them (with looking or reaching) infants’ novelty, familiarity, stimulus complexity, or evaluative preference can be measured.

TTR are correct that attention (or reaching) is measured in each of these techniques, but the *kind* of attention measured is very different. That is not to claim that, as TTR suggest, there are not situations in which attention to stimulus A versus B in a given paradigm could reflect a different process from the one researchers attributed to it; however, these feel like exceptions that prove the rule rather than reasons to reject the rule entirely (it might be rude to stare at odd behavior, but we all do it, and, either way, infants are clearly not constrained by such social niceties). Furthermore, the alternatives raised by TTR have consistently been ruled out with control conditions in which researchers use the very same methods (VOE in VOE studies, preferential looking in preferential looking studies) with slightly adjusted comparisons, and they demonstrate that infants do not look longer at A than B in the control conditions, narrowing the hypothesis space for what drives their looking to A in the test conditions. Curiously, TTR describe CK theorists as using terminology in ways that we typically would not. For example, the term “preferential looking paradigm” is used specifically to refer to the paradigm originally developed by Fantz [1958] in which infants can gaze back and forth between two stimuli, A and B. In fact, the phrase “preferential looking” is *entirely absent* from both Baillargeon, Spelke, and Wasserman [1985] and Baillargeon [1987] VOE papers, despite being the first papers TTR cite for misinterpreting preference methodologies. Baillargeon and colleagues *do* use the phrase “preference for the inconsistent outcome” in these early papers, but clearly they do not mean it in an evaluative sense. Indeed, the use of the word preference in VOE papers was more common when the first papers were written 30 years ago than it is today, perhaps because then it highlighted the link between VOE measures and Fantz’s preferential looking paradigm that was considered a valid measure of infant cognitive processing. Today, authors are well aware of the potential for conceptual confusion that could result from using “preference” ambiguously and are more likely to describe VOE results with, for instance, “looked longer toward the inconsistent outcome” than “preference for the inconsistent outcome.”

So, TTR claim that infancy researchers are confusing the meaning of preference across methodologies, and I claim that they are not. These claims are fairly difficult to adjudicate, so to assess TTR’s, I used a “find” function to examine how many times the word “preference” appears in the following papers: the first VOE demonstration of false belief competence in infants by Onishi and Baillargeon [2005], the first predictive-looking demonstration of false belief competence in infants by Southgate, Senju, and Csibra [2007], and a review of the preceding five years of evidence for false belief in infancy by Baillargeon, Scott, and He [2010]. The word preference never appeared in any of them [though “preferred” appears in Baillargeon et al., 2010, in ref-

erence to a clear case of positively evaluating or liking]. Thus, TTR's statement that "because the way in which Hamlin and colleagues interpret preference differs from interpretations made by early false belief researchers using the VOE paradigm, their ultimate conclusion also differs" is unfounded; false belief researchers rarely discuss preferences at all. TTR might claim that infancy researchers using VOE and predictive looking measures *really are* studying preference in all of these measures, even if they do not claim to be doing so, but, if so, then it seems difficult to argue that it is the researchers' understanding of what preference means that is lacking. Indeed, thanks to the careful empirical work of infancy researchers from various theoretical perspectives, we are now more aware than ever of the role of various processes in driving specific patterns of attention across these unique methodologies, and progress continues to be made [see, amongst many others, Aslin, 2000, 2007; Colombo & Mitchell, 2009; Kidd et al., 2012; Schöner & Thelen, 2006; Sirois & Mareschal, 2004; Teglas, Vul, Giroto, Gonzalez, Tenenbaum, & Bonatti, 2011].

Without getting into any specific CK debate here (I will argue for infant competence in the realm of social evaluation below), there is now empirical evidence that various infant attentional methodologies are more than mere change detection paradigms; they are dissociable from each other and tap actual competence in a given domain. For instance, methods purporting to tap anticipation point to the same conclusions as do methods purporting to tap surprise, despite their very different looking patterns [e.g., Fawcett & Liskowski, 2012; with Hamlin, Wynn, & Bloom, 2007; and Kuhlmeier, Wynn, & Bloom, 2003; see also Southgate et al., 2007; with Onishi & Baillargeon, 2005; Woodward & Cannon, 2013; with Woodward, 1998]. Additional convergent evidence has come from comparing results from attentional methodologies with more active, non-attentional tasks in infancy, such as active helping [Buttleman, Carpenter, & Tomasello, 2009; compared with VOE measures by Onishi & Baillargeon, 2005] and selective reaching [Hamlin, Hallinan, & Woodward, 2008; compared with habituation-dishabituation measures by Woodward, 1998]. In both cases, action tasks and attention tasks point to the same underlying competence, supporting the validity of each method. Finally, a number of recent publications have demonstrated that individual differences in infants' attentional responding during infancy correlate with performance in more active paradigms, both within the infancy period [Thoermer, Woodward, Sodian, Perst, & Kristen, 2013], and when children are in preschool [Starr, Libertus, & Brannon, 2013; Wellman, Lopez-Duran, LaBounty, & Hamilton, 2008; Wellman, Phillips, Dunphy-Lelii, & LaLonde, 2004; Yamaguchi, Kuhlmeier, Wynn, & vanMarle, 2009; see also Halberda, Mazocco, & Feigenson, 2008]. These relationships suggest that infancy methods are tapping actual cognitive processing of the level attributed to them by CK researchers.

Given the Conceptual Confusion regarding Infant Attentional Measures and What They Mean, CK Theories (in Particular, that Infants Engage in Sociomoral Evaluations) Are False

Here I will focus on my own work claiming to demonstrate sociomoral evaluation in infancy, addressing whether it can be considered evidence for a nascent moral sense. [For extended discussion of these issues, please see Hamlin, 2013a, b, in

press a, b.] My work is not the only evidence in the literature relevant to these issues (far from it), but, since it is the only work TTR criticize in this domain, I will limit my discussion here. TTR's argument that the evidence for sociomoral evaluation is misinterpreted rests on two claims: the first, that to look at or reach for a puppet need not reflect *evaluative* preference; the other, that there is more to morality than a preference for helpers and that a preference for any individual might depend on the specific needs of the evaluator and the constraints of the situation. I agree wholeheartedly with both claims and have spent the last four years exploring whether infants' puppet choices are evaluative and whether they reflect more than a preference for helpers. Although some of this work has not yet been published, much of it already has, and so I find it disappointing that TTR chose not to address any of it in their analysis. It has been four years since the last "infants prefer helpers to hinderers [full stop]" paper was published. Specifically, I am not as pessimistic as TTR as to whether we simply cannot know whether an infant's evaluation has any "moral" content because we cannot ask them; various patterns of choice have led me to believe that infants' choices are evaluative and that those evaluations reflect much more than a preference for helpers.

Are Infants' Choices Evaluative?

Infants Do Not Attend Differentially after Helping and Hindering Events. TTR are correct that three-month-olds might look at stimulus A, and five-month-olds might touch it first, for a variety of reasons having nothing to do with evaluation. A might be more familiar or more novel, it might be more perceptually interesting, etc., etc. Critically, we find that infants *only* prefer helpers to hinderers during our choice phases. That is, they do not look longer after helping events than after hindering events (or vice versa), and how much individual infants attend following helping or hindering has no effect on their puppet choices. If infants were, for example, entering the lab having seen lots of helping and only a bit of hindering in their lives, one might expect them to find helping more *familiar* and prefer helpers for that reason (or prefer novel hinderers). This simply does not come out of their patterns of attention to helping and hindering events. This, combined with all of the within-experiment controls for order, side, color, etc., makes the possibility that there is something perceptually interesting, novel, or familiar that drives their preference for A over B during choice less likely. Finally, studies using different *kinds* of attentional methods to show infants helping and hindering events show the very same results: Infants prefer pro-social characters whether or not they are habituated to helping and hindering, as well as when they are shown each action only once. As various familiarization techniques show marked influence on infants' responses in other paradigms, thought to reflect the interplay between familiarity and novelty preferences, this is suggestive that social evaluations reflect more than such preferences.

Infants' Evaluations Are Extremely Selective. Although infants from very young ages select helpers over hinderers across several different helping and hindering scenarios, their selections are specific to scenarios that reflect actual helping and hindering. For example, they do not prefer characters who engage in identical acts toward a non-social target, suggesting their preferences are specifically social [Hamlin &

Wynn, 2011], and they do not prefer characters who engage in identical acts toward a social target who performed all of the same physical activities to signal an unfulfilled goal but whose goal was ambiguous because he was looking the wrong way [Hamlin, in revision]. Thus, infants do not prefer characters who perform particular actions nor those who act in a certain way toward a social other; they prefer helpers to hinderers.

Toddlers' Far More Complex Actions Lead to the Same Conclusions. Studying social evaluation in young infants is not trivial: The *only* thing our youngest participants can do is watch a puppet show and then look back and forth between two puppets, and the only thing somewhat older infants can do is reach for one puppet or another. As Aslin [2007] has pointed out, without attentional methodologies, we would know almost nothing about infant cognitive processing at any level. Toddlers, however, can do much more than attend. In one study, we found that 16-month-olds who first watched helping and hindering events subsequently matched the food preference of the helper and of an unknown neutral informant, but they failed to match the food preference of the hinderer [despite attending equally to preference displays; Hamlin & Wynn, 2012]. In another study, 21-month-olds selectively gave the “last treat” to the helper but took away a treat from the hinderer in order to give it to an unknown target who had not received a treat. Toddlers showed the opposite effects when the targets were formerly helped or hindered by others (a beneficiary and a victim); more toddlers took from the beneficiary and more toddlers gave to the victim [Hamlin, Wynn, Bloom, & Mahajan, 2011]. Thus, the puppets that infants look longer at are the same as those that toddlers emulate and share with, and toddlers are willing to approach hinderers, in order to direct an antisocial act at them. These studies provide converging evidence that infants' preferential reaching and attention is evaluative.

Are Infants' Preferences (Just) Based on Liking Helpers and Disliking Hinderers?

“A second major conceptual problem in Hamlin and colleagues' use of the FPL [forced-choice preferential looking] procedure involves slippage between the moral goodness of the characters in the story and their helpfulness” [TTR].

We have found that infants prefer helpers to hinderers in a variety of social scenarios beginning at three months and that, by six months, they prefer helpers to neutral others and neutral others to hinderers [Hamlin et al., 2007; Hamlin, Wynn, & Bloom, 2010]. At three months, infants do not yet prefer helpers to neutral but prefer neutral to hinderers, suggesting social evaluations begin with an *aversion* to hinderers. TTR suggest this possibility is problematic, but it is unclear why, as an aversion to antisocial behavior is clearly part of a moral sense. That hinderer aversion turns up first certainly makes sense personal-safety wise, and yet it makes less sense in terms of experiential learning arguments; extremely young, immobile infants are doing little to *be* hindered, and the same patterns of results are observed in first- and later-born children who presumably have drastically different amounts of experience with and observational experience of antisocial acts. These results suggest that experiencing being hindered and/or observing hindering at different rates in the environment does not influence infants' puppet choices (or not enough to show up in our rather crude methodologies).

But infants do not always prefer helpers. In analyses much like the one TTR make, we have argued specifically that morality is much more than a preference for helpers and aversion to hinderers (specifically, those whose actions facilitate or prevent others' unfulfilled goals). For instance, by four and a half months of age, infants prefer those who *prevent* the goals of those who have hindered others [Hamlin, 2014; Hamlin et al., 2011]; by eight months, they prefer those who try but fail to help over those who try but fail to hinder [but, intriguingly, not those who successfully help or hinder over those who try but fail to do so; Hamlin, 2013c]. And at 10 months, infants only prefer helpers to hinderers if both *knew* what they were doing was helpful or unhelpful, just as adults do [Hamlin, Ullman, Tenenbaum, Goodman, & Baker, 2013]. That is, infants' preferences for certain social others seems specific to the context in which those behaviors occur and are focused specifically on the mental states of the characters involved. Thus, we do not "say that an infant shows a sense of moral understanding or moral judgment on the basis of the synonymy between the terms helpful and moral" as TTR suggests. We have performed just the conceptual analysis TTR did and wholeheartedly agree that to make such a conflation would be deeply problematic.

Conclusions

To close, it seems that one of the most common criticisms of CK claims is that, whether or not there is evidence for CK, to assert that there is evidence is to ignore development. This strikes me as odd. Providing an accurate account of development requires an accurate account of the beginning and end states; developmentalists studying the beginning should inform and be informed by the work of those studying development who should inform and be informed by the work of those studying the mature state. To the extent that CK theorists have criticized certain developmental theories, they have done so because those theories cannot account for demonstrations of infant competence, not because they find development unimportant; yet they are continually accused of doing so. Indeed, in their discussion of a developmental systems theory (DST) approach, TTR explicitly say that we should embrace DST because it is "unlikely to locate adult-like social cognition in infancy," "is not committed to the representational theory of mind implicit in CK theory," and "undercuts the very nature-nurture debate." Surely a dislike for the idea that infants could have competence, or that that competence could reflect representational theory of mind, and/or that a feeling that the nature-nurture debate does more harm than good should not be considered appropriate reasons to accept or reject a body of research! Rejecting CK theory because of a distaste for the claim that infants have competence is no better than CK researchers embracing CK because they like the claims. If TTR are correct that "moral" is not an appropriate way to describe my and my colleagues' work, then the onus is on them to provide a coherent alternative framework. As I hope I have demonstrated above, their suggestions do not hold water in light of the body of evidence for infants' social evaluations.

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