



RESPONSE

Rich interpretations of infant behaviour are popular, but are they valid? A reply to Scott and Baillargeon

Cecilia Heyes

All Souls College & Department of Experimental Psychology, University of Oxford, UK

I am grateful to Scott and Baillargeon (2014) for taking a close look at my fresh look. Their article deserves careful consideration, but in a 500-word reply I must confine myself to staccato expression of three points: First, Scott and Baillargeon concede that the data purporting to show that infants attribute false beliefs can also be explained in terms of low-level novelty (Heyes, 2014). If this concession is not immediately apparent, follow their footnote to my discussion of the ‘no key’ control in the penguins study (Scott & Baillargeon, 2009), and look carefully at their Table 1. My article included all of the infant studies in Table 1 except those published since it went to press, and Scott and Baillargeon do not claim that these latest studies provide counter-evidence.

Second, as I pointed out in the target article, with citations, the experiments to which Scott and Baillargeon appeal for convergent evidence – using Woodward’s preference task – are themselves subject to low-level interpretation. Therefore they do not obviate the need to control for low-level processing in infant research on false belief. A great many developmentalists favour cognitively rich interpretations of infant data, but popularity is not the same as validity. Only when conclusions are based on experimental designs with the power to distinguish low- from high-level processing can we be sure that the popularity of rich explanations is driven by evidence. Other potential drivers include the academic incentive structures created by high-impact, non-specialist journals with a taste for reporting precocity.

Third, even if research involving Woodward’s preference task or other paradigms had shown securely that infants are capable of coding ‘actions on objects by agents’, it would not follow that they are doing this in

false belief experiments. Infants may not ‘leave their psychological knowledge at the door of the laboratory when they arrive for a false-belief experiment’ (Scott & Baillargeon, 2014), but if they are anything like adults, they may well give that knowledge a rest while the experiment is in progress. We know that adults are capable of formal reasoning, but cognitive illusions such as the conjunction fallacy show that we often take short-cuts instead (Tversky & Kahneman, 1983). Similarly, adults are capable of recognizing facial identity, but do not engage in this kind of holistic processing unless facial identity is important for the task at hand (Zimmermann & Eimer, in press), and although adults can certainly construe human figures as agents, we sometimes treat them as mere objects, encoding their shape but not their visual perspective (Heyes, in press; Santiesteban, Catmur, Hopkins, Bird & Heyes, in press).

Given that (1) the infant false belief data can be explained in terms of low-level novelty, (2) convergent evidence does not distinguish high- and low-level accounts, and (3) cognitive capacity does not imply obligatory use, I continue to believe that, rather than trade plausibility arguments, we should design experiments to test rich, domain-specific accounts of infant behaviour against leaner, domain-general alternatives.

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Address for correspondence: Cecilia Heyes, All Souls College, Oxford OX1 4AL, UK; e-mail: Cecilia.Heyes@all-souls.ox.ac.uk
This is a response to the commentary by Scott and Baillargeon (2014).

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