Developmental Review xxx (2014) xxx–xxx

Contents lists available at ScienceDirect

Developmental Review

journal homepage: www.elsevier.com/locate/dr

Post hoc versus predictive accounts of children's theory of mind: A reply to Ruffman

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ARTICLE INFO

Article history:
Received 2 May 2014
Available online xxxx

Keywords:
False belief
Theory of mind
Mentalism
Minimalism

ABSTRACT

Ruffman (in press) argues for a minimalist account of infants' performance on theory of mind tasks. This commentary argues that because Ruffman's minimalist account is post hoc, it neither generates testable predictions about how infants will respond in new situations, nor does it offer a coherent explanation for existing false-belief findings. An alternative, mentalist account is presented. This account integrates infancy findings with prior theory of mind literature and generates novel predictions about children's false belief performance.

Over the past two decades, numerous studies have presented evidence that infants can attribute goals, perceptions, and beliefs to agents (e.g., Gergely, Nádasdy, Csibra, & Bíró, 1995; Onishi & Baillargeon, 2005; Woodward, 1998; see Baillargeon et al., 2014, for a review). These findings have led many investigators to adopt a *mentalist* account that assumes mental-state reasoning is present in infancy. Ruffman (in press) challenges this conclusion, offering an alternative *minimalist* account of these findings. According to this account, infants' statistical learning abilities allow them to learn how agents tend to behave in particular situations. Together with a capacity to track an agent's perceptual access to events, these learned patterns of behavior (i.e. behavioral rules) allow infants to interpret and predict intentional actions without any understanding of the agent's mental states.

Two central issues arise from Ruffman's review: the post hoc nature of the minimalist account, and the importance of integrating infancy research with the broader literature on theory of mind.

More than intuition: Evidence that the minimalist account is post hoc

The primary flaw of Ruffman's minimalist account is that, despite his assertions to the contrary, it is post hoc. Ruffman carefully examines existing findings and for each experimental condition derives a

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http://dx.doi.org/10.1016/j.dr.2014.05.001
0273-2297/

Please cite this article in press as: Scott, R. M. Post hoc versus predictive accounts of children's theory of mind: A reply to Ruffman. *Developmental Review* (2014), http://dx.doi.org/10.1016/j.dr.2014.05.001

behavioral pattern or rule that could have generated the infants' responses. However, in any given situation there are many statistical patterns available that could influence infants' responses. This is true even if one assumes that infants are biased to attend to and learn statistical patterns that involve agent–object relations. Ruffman offers no *a priori* explanation for why infants' responses in a particular situation would be guided by one statistical pattern over another. As a result, Ruffman's account neither generates specific predictions about how infants will respond in novel situations (rendering the account unfalsifiable), nor does it offer a coherent explanation for the results from prior infant false-belief tasks.

To illustrate, consider the findings of Experiment 1 in Scott and Baillargeon (2009). This experiment involved two toy penguins that were identical except that one could come apart (2-piece penguin) and one could not (1-piece penguin). In each familiarization trial, while a female agent watched, an experimenter's hands placed the 1-piece penguin and the two pieces of the disassembled 2-piece penguin on platforms or in shallow containers. The agent then placed a key in the bottom piece of the 2-piece penguin and stacked the two pieces; the two penguins were then indistinguishable. During the test trials of the false-belief condition, the agent was initially absent. The experimenter assembled the 2-piece penguin, covered it with a transparent cover, and then covered the 1-piece penguin with an opaque cover. The agent then returned with her key and reached for one of the two covers. Infants looked reliably longer when the agent reached for the transparent cover. This suggests that they expected her to falsely assume that the penguin under the transparent cover was the 1-piece penguin, and hence have a false belief that the disassembled 2-piece penguin was under the opaque cover. In the true-belief condition, the agent was present throughout the test trials; in this case, infants looked longer when the agent reached for the opaque cover, suggesting they expected her to reach for the transparent cover because she had just seen the 2-piece penguin hidden there.

Ruffman (in press) argues that infants could succeed in this task by reasoning solely about the agent's behavior rather than her mental states. He claims that in the true-belief condition, infants expected the agent to reach for the transparent cover because (1) she had always reached for the 2-piece penguin before and (2) she had perceptual access when the experimenter placed the 2-piece penguin under the transparent cover. For the false-belief condition, he states that infants looked longer when the agent reached for the transparent cover because (1) the penguin under the cover was an "intact" penguin and (2) the infants had never seen the agent reach for an intact penguin before so (3) they looked longer in order to encode this novel agent–object relation. Infants in this condition looked less when the agent reached for the opaque cover because she reached away from the intact penguin, as she had done in the past.

Ruffman thus invokes two statistical patterns to explain infants' responses: (1) the agent had always reached for a 2-piece penguin and (2) the agent had never reached towards a 1-piece or intact penguin. While infants could have learned both of these statistical patterns over the course of the experiment, they could have learned others as well. For instance, the agent always reached for a visible penguin. Detecting this pattern would have lead infants in both conditions to expect the agent to reach for the transparent cover in the test trial. Similarly, infants likely learned that the agent always reached for a penguin of some kind, and that she never reached for the various platforms and containers that the penguins rested on during the familiarization trials. This pattern should have led infants in both conditions to look longer when the agent reached for the opaque cover rather than the visible penguin, as this novel agent–object relation was inconsistent with those they had observed in the past. If the infants were responding to statistical patterns in the agent's behavior, it is unclear why the infants' responses were not influenced by these other statistical patterns that consistently occurred during the experiment.

Even if one assumes that infants only learned the two statistical patterns that Ruffman appeals to in his explanations, it is unclear why infants should respond to *different* statistical patterns in the two conditions. The final displays of each event were visually identical across conditions, and thus any response that was based on the perceptual properties of the display should have occurred in both conditions. If infants needed to encode a novel agent–object relation when the agent reached towards an intact penguin, then this should have been true in both conditions. Yet when the agent witnessed the hiding event, infants did not exhibit increased attention when she reached for the transparent cover.

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Ruffman does not explain why the agent's perceptual access overrode the need for infants to encode a novel agent–object relation when the agent reached towards an apparently intact penguin.

Similar issues arise when one considers the alternative explanations that Ruffman offers for other false-belief tasks. For instance, Surian, Caldi, and Sperber (2007) presented infants with a caterpillar that repeatedly chose one food over another. In the test trial, the two foods were hidden behind barriers. If the caterpillar witnessed the food being hidden in the test trial (true-belief condition), then infants looked longer at the new- than at the old-food event. If the caterpillar did not witness the food being hidden (false-belief condition), the infants looked equally at the two events. Ruffman (in press) argues that the infants in the true-belief condition looked longer when the caterpillar approached a new food because they had never seen the caterpillar approach this food before and thus they needed to encode a novel agent–object relation. If that were the case, then the infants in the false-belief condition should also have looked longer when the agent approached the new food. Ruffman's account cannot explain why this particular pattern affected infants' responses in one condition but not the other.

Note that the behavioral rules that Ruffman invokes are also inconsistent across studies. To explain Scott and Baillargeon (2009), Ruffman argues that infants must encode a novel agent–object relation in the false- but not the true-belief condition. For Surian et al. (2007), he makes the opposite claim, despite the fact that in both experiments *both* conditions involve novel agent–object relations. In order to explain the results of Song and Baillargeon (2008), Ruffman states that when infants see an agent repeatedly reach for a doll, they assume that aspects of the doll cause the agent's search (rather than the entire doll). Yet his explanations for other studies assume that infants are encoding a relation between an agent and a particular object (i.e. a 2-piece penguin) rather than aspects of that object (i.e. the penguin's feet). No explanation is offered for these inconsistencies in how infants respond to events across studies.

In order for the minimalist account to constitute a fair alternative to the mentalist account, it must be a coherent theoretical framework that includes a rationale for which behavioral patterns infants will respond to in a given situation. Such an account would not only explain existing findings, but it would also yield falsifiable predictions about how infants will respond in *novel* experimental situations. This would allow researchers to directly test these two accounts empirically. In its present form, however, the minimalist account falls short of this goal and thus it continues to appear as if minimalists appeal to whichever behavioral pattern is necessary in order to generate a post hoc explanation for a given experimental finding.

Integrating infancy findings with previous theory of mind research

Ruffman (in press) accuses mentalists of focusing solely on infancy and ignoring later development in theory of mind. This is not the case: over the past several years, a number of mentalist accounts have attempted to integrate recent findings from infancy research with the large body of literature on false-belief understanding in school-aged children (e.g., Baillargeon, Scott, & He, 2010; Carruthers, 2013; Helming, Strickland, & Jacob, 2014; Southgate, 2013).

For instance, my colleagues and I have argued for a *processing-load* account of children's false-belief performance (e.g., Baillargeon et al., 2010; He, Bolz, & Baillargeon, 2012; Scott & Baillargeon, 2009). According to this account, false-belief tasks in which children are asked a direct question (i.e. elicited-response tasks) are more challenging than those that do not require children to answer direct questions (i.e. non-elicited tasks) because they involve a number of additional demands that overwhelm young children's processing resources. In particular, we assume that when children are asked a direct question in a standard elicited-response task (i.e. "Where will Sally look for her marble?"), a *response-selection* process is activated: children must interpret the test question, choose to answer it, and select an appropriate response (e.g., Scott & Baillargeon, 2009; see also Mueller, Brass, Waszak, & Prinz, 2007; Saxe, Schulz, & Jiang, 2006). Executing this response-selection process involves linguistic demands because children must interpret the words used in the test question and consider the experimenter's pragmatic intent (e.g., Hansen, 2010; Siegal & Beattie, 1991). This process also involves working-memory demands because children must hold in mind the agent's false belief while planning

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and executing a response. In some tasks, the test question may also trigger a prepotent bias to respond based on reality and children need sufficient inhibitory skills to suppress this inappropriate response (e.g., Birch & Bloom, 2003; Carlson & Moses, 2001; Leslie & Polizzi, 1998).

This analysis can explain the positive results obtained in non-elicited false-belief tasks with infants and toddlers: when children do not need to cope with the demands imposed by response-selection and inhibition, they can demonstrate false-belief understanding at younger ages. This account can also explain prior evidence that children's ability to pass elicited-response false-belief tasks is correlated with their language (e.g., Milligan, Astington, & Dack, 2007) and executive function skills (e.g., Carlson & Moses, 2001). Moreover, this account generates novel predictions about the circumstances under which children should succeed in false-belief tasks.

One such prediction is that if response-selection and inhibitory demands were reduced, younger children might succeed in an elicited-response false-belief task. To test this prediction, my colleagues and I devised a low-demand elicited-response task (Setoh, Scott, & Baillargeon, 2011) in which 2.5-year-olds listened to a false-belief story accompanied by a picture book. The story introduced Emma, who found an apple in one of two containers (e.g., a box), moved it to the other container (e.g., a bowl), and then left. In her absence, her brother found the apple and took it away to an undisclosed location. Emma then returned to look for her apple. Children were shown the two containers and asked where Emma would look for her apple. Interspersed among the lines of the story were two practice trials designed to reduce response-selection demands: children saw two pictures (e.g., apple, banana) and were asked a "where" question (e.g., "Where is Emma's apple?"). These trials provided children with the opportunity to practice interpreting a question, choosing to respond, and selecting between two pictures.

Children performed reliably above chance in the test trial, selecting the container in which Emma falsely believed her apple was located. We have subsequently replicated these results and demonstrated that 2.5-year-olds' ability to succeed depends on both the nature (e.g., Scott & Setoh, 2012; Setoh et al., 2011) and amount (Setoh et al., 2011) of response practice that they receive. These results support the notion that young children's failure in elicited-response tasks stems from their inability to cope with the tasks' processing demands, and demonstrate that, as predicted by the processing-load account, younger children can succeed in elicited-response tasks if these demands are reduced.[1]

This account and its supporting findings represent a first step towards integrating positive results in infancy with findings on later false-belief understanding, including individual variation in false-belief performance. Given the youth of this field (Onishi and Baillargeon's results were published less than 10 years ago), many unanswered questions remain. Most notably, our processing-load account does not yet incorporate social input and its role in the development of false-belief understanding (although research underway in my lab is beginning to address this issue; see Roby & Scott, 2014). However, these ongoing efforts demonstrate that infancy researchers do not assume that their findings simply supplant the prior literature on false-belief understanding. Rather, mentalists are actively working to create a comprehensive account of the development of theory of mind.

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[1] This account also generates a complimentary prediction: that increasing the demands involved in a non-elicited task should impair children's performance. This prediction has also received empirical support (e.g., Scott, Setoh, & Baillargeon, 2013).

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