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How to Pass the False-Belief Task Before Your Fourth Birthday

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Abstract

The experimental record of the last three decades shows that children under 4 years old fail all sorts of variations on the standard false-belief task, whereas more recent studies have revealed that infants are able to pass nonverbal versions of the task. We argue that these paradoxical results are an artifact of the type of false-belief tasks that have been used to test infants and children: Nonverbal designs allow infants to keep track of a protagonist's perspective over a course of events, whereas verbal designs tend to disrupt the perspective-tracking process in various ways, which makes it too hard for younger children to demonstrate their capacity for perspective tracking. We report three experiments that confirm this hypothesis by showing that 3-year-olds can pass a suitably streamlined version of the verbal false-belief task. We conclude that young children can pass the verbal false-belief task provided that they are allowed to keep track of the protagonist's perspective without too much disruption.

Keywords

social cognition, theory of mind, task complexity, false-belief task

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The intensely social lifestyle of the human species requires that people are constantly monitoring each other's intentions, beliefs, desires, and other mental states. Over the last three decades, developmental psychologists have studied how mind-reading skills begin to unfold in early childhood. The vast majority of these studies have employed variations of the false-belief task pioneered by Wimmer and Perner (1983). In a false-belief task, the child witnesses an agent interacting with an object and then storing it in location A. Next, in the *displacement phase* of the task, the agent leaves the scene, or is otherwise distracted, and the object is transferred to a second location, B. Lastly, in the *test phase* of the task, the experimenter must establish whether the child realizes that the agent mistakenly believes that the object is still in location A. This can be tested in a variety of ways, for example, by asking the child where the agent will look for the object (Baron-Cohen, Leslie, & Frith, 1985; Wimmer & Perner, 1983), by tracking the child's eye gaze to see whether he or she is looking at location A or B in anticipation (Clements & Perner, 1994; Southgate, Senju, & Csibra, 2007) or shows surprise if the agent heads for location B (Onishi & Baillargeon, 2005; Song & Baillargeon, 2008), or by engaging the child to help the agent (Buttelmann, Carpenter, & Tomasello, 2009; Southgate, Chevallier, & Csibra, 2010).

There are two main types of false-belief task: verbal and nonverbal. In verbal designs, such as Wimmer and Perner's (1983), the experimenter crucially relies on linguistic means to interact with the child, present the story, and so on. In nonverbal designs, by contrast, language either is not used at all or is merely supplementary to what is chiefly a nonlinguistic mode of interaction and presentation.

Although literally hundreds of studies have shown that, by and large, children fail at verbal false-belief tasks prior to age 4 years, a considerable number of recent articles have reported that toddlers and even infants pass all kinds of nonverbal false-belief tasks (see Baillargeon, Scott, & He, 2010, for a review). What is one to make of this discrepancy? This question has been answered in various ways. Clements and Perner (1994) maintained that the two types of false-belief task probe different kinds of understanding. In their view, nonverbal and verbal tasks require implicit and explicit understanding, respectively, and the former precedes the latter in development. In a similar

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spirit, Apperly and Butterfill (2009) hypothesized that there are two *mind-reading systems*: an early-developing system for tracking belief-like states that guides children's looking behavior, and a later-developing system that guides children's explicit judgments about beliefs. Baillargeon and her colleagues (2010) contended that verbal tasks involve two mental processes that are not implicated in nonverbal tasks: a process of response selection and a process of inhibition of what is sometimes called "the pull of the real"—a prepotent tendency to answer the test question on the basis of one's own knowledge about the facts (for a recent review of these and other dualist accounts, as well as a new proposal, see de Bruin & Newen, 2012).

All these explanations have two things in common. One is that they presuppose a qualitative difference between the mental mechanisms needed for solving verbal and nonverbal tasks. The second commonality is precisely that these explanations are cast directly in terms of mental processes and representations rather than starting with a proper analysis of the tasks as such. We adopted an alternative approach that makes minimal assumptions about children's cognitive abilities and focuses instead on the differences between the two types of false-belief task.

Our point of departure is that already before their first birthday, children are naturally inclined to track other people's perspectives. This ability allows infants to anticipate another person's actions, even when their predictions are based on false information (cf., e.g., Kovács, Téglás, & Endress, 2010; Senju, Southgate, Snape, Leonard, & Csibra, 2011). This finding is not controversial anymore. In contradistinction to the dualist theories discussed previously, our approach requires only minimal assumptions about this ability. To say that children can track another person's perspective is merely to say that they can form expectations about that person's actions based on observations of his or her behavior. What kinds of mental processes and representations underwrite this capacity is largely irrelevant to our project. For example, it is immaterial whether this capacity involves explicit representations of beliefs (Perner, 1991, 2010), whether it undergoes a conceptual change around age 4 years (Bartsch & Wellman, 1995; Wellman, Cross, & Watson, 2001), or whether it involves just one system for mind reading or several (Apperly & Butterfill, 2009; Scholl & Leslie, 1999). The only assumption that is critical to our approach is that perspective tracking depends to some extent on cognitive resources and is therefore more susceptible to disruption in children than in adults.

One rather blatant difference between verbal and nonverbal false-belief tasks is that the former are inherently more complex in that they require the integration of linguistic information, whereas the latter do not. However, if our hypothesis is correct, the lack of language skills is not the only reason why children under 4 years old fail verbal false-belief tasks. As well as imposing weaker linguistic demands (if, indeed, they impose any), nonverbal false-belief tasks are normally designed to minimally interfere with children's natural ability to track events from the agent's point of view, whereas verbal false-belief tasks tend to disrupt this process in various ways. For example, nonverbal false-belief tasks generally feature a single protagonist, whereas verbal false-belief narratives typically include more than one character. Given that tracking two perspectives is more demanding than tracking one, this difference goes some way toward explaining why children perform better in nonverbal tasks than in verbal tasks. Speaking more generally, we hypothesize that, for a child's perspective-tracking ability to work effectively, it should be disturbed as little as possible, especially in young children.[1]

To illustrate our key point, we will briefly compare the studies that have come to exemplify the two main experimental paradigms. The method and design of Onishi and Baillargeon's (2005) nonverbal false-belief task was extremely simple. Apart from the protagonist and a few props, the stage was empty, the experimenter did not interfere with the proceedings, and the target object was self-propelled, moving from box to box of its own accord. In short, there was nothing to distract the children's attention and prevent them from tracking the protagonist's point of view. In stark contrast to that minimalist approach, Wimmer and Perner's (1983) scenario featured two characters (a little boy named Maxi and his mother) in a rather elaborate story, told by a recorded voice and simultaneously acted out by the experimenter with dolls and various other props. During the story, Maxi disappeared for a considerable period of time while his mother was baking a cake. Finally, as Maxi returned from the playground, the experimenter intervened with the key question: "Where will Maxi look for the chocolate?"

The complexity of Wimmer and Perner's (1983) experimental task is clearly an order of magnitude greater than the complexity of Onishi and Baillargeon's task, which goes a long way toward explaining why children performed so much better on the latter than they did on the former. There are two features, in particular, that may have disrupted children's perspective tracking in Wimmer and Perner's task: It may not have been clear who the main character of the story was—Maxi left the scene for a while, during which time his mother held the stage with her baking—and the experimenter switched roles from puppeteer to interviewer to spring her question on the unsuspecting child. Each of these factors may have drawn the younger children away from tracking Maxi's perspective, and their combined effect may have been even greater. Other versions of the standard false-belief task impose similar demands and are therefore liable to the same sort of criticism.

In their influential meta-analysis, Wellman and his colleagues (2001) discussed a number of task variations that have been found to improve children's performance on verbal false-belief tasks. Most of these are plausibly seen as facilitators for perspective tracking. For example, several studies have shown that success rates increase when children participate in misdirecting the agent, the agent's mental state is made more explicit, or the target object is made less salient, thus reducing the pull of the real. It should be noted, however, that in isolation, none of these factors would raise 3-year-olds'

performance above chance level (Wellman et al., 2001). In our view, for these and other task variations to be successful, the process of perspective tracking must be allowed to run its course throughout the task. This is precisely what we tried to do in our study.

The Duplo Task

The protocol for the Duplo task was a variation on the standard false-belief task. The experimenter showed the child a set of Duplo toys (i.e., large Lego toys for small children; Lego, Billund, Denmark) that she had on a table: a girl figure, a bunch of bananas, and two little yellow cupboards that were referred to as "fridges," one with a blue door and one with a red door. The child was told that the girl loved bananas and had one for breakfast every morning. This morning, she had already had a banana, so she wanted to return the remaining ones to the fridge. At this point, the experimenter made the girl put the bananas inside one of the two fridges (the choice of refrigerator was counterbalanced across participants) and told the child that the girl now wanted to go for a walk.

Up to this point, our procedure was the same as in a standard false-belief task. In the remainder of the task, two novel sets of variations were introduced, both intended to help the child keep track of the Duplo girl's perspective. First, we made sure that the child could see the Duplo girl throughout the session. Hence, rather than making the figure disappear, as experimenters had done in earlier studies, our experimenter made the Duplo girl walk in the direction of the child and turn her back on the scene.[2] Then the experimenter asked the child, in a secretive manner, "Can the girl see me from where she is?" This was only a prompt: If the child did not answer, the experimenter filled in, saying, "She surely can't see me from over there." Then, looking at the child with an expression suggesting connivance, the experimenter moved the bananas from one fridge to the other. At this point, the experimenter asked the child, pointing at the girl figure, "She hasn't seen what I did, has she?" This, too, was only a prompt: If the child did not answer, the experimenter would say, "No, she hasn't seen what happened!" These prompts were intended to help the child keep track of the girl's perspective.[3] For the same reason, we had the experimenter move the bananas herself, rather than introducing a second character in the story, which might result in the child losing track of the protagonist's perspective.

The second set of task variations we introduced was intended to help the child keep track of the Duplo girl's perspective during the test phase. Once it had been established that the Duplo girl had not seen the experimenter move the bananas, the experimenter returned the girl figure back to the center of the scene. She placed the figure in front of the two fridges, facing the empty space in between, and asked the child whether he or she would like to play with the girl now. The experimenter encouraged the child to take the lead by saying, "What happens next? You can take the girl yourself if you want. . . . What is she going to do now?"

Instead of asking the standard false-belief question, "Where will the girl look for the bananas?"—which requires a referential response—we used the open questions "What happens next?" and "What is she going to do now?" We also encouraged the child to continue acting out the story. This approach deviates from the standard one in four ways that are relevant to our main hypothesis. First, the standard procedure may be an unnatural test for young children in that, up to the false-belief question, the experimenter just tells them a story (why then start interrogating the child about the protagonist?). In our study, the experimenter adopted a more interactive stance throughout, and there was no abrupt break between the narrative and response elicitation. Second, act-out responses are generally easier for young children than verbal responses are, perhaps partly because they make it easier for them to identify with the protagonist. Third, unlike the standard question, ours did not confront children with a binary choice; a more open question should help the child to keep track of the protagonist's perspective rather than having to consider alternative options. Finally, whereas the standard question focuses on the target object (in our case, the bananas), which might well reinforce the pull of the real, the questions we asked the child did not mention the target object at all. For all these reasons, we expected that our procedure would minimize interference with perspective tracking.

Given that the only link between the Duplo girl and the empty fridge was that she put the bananas in that fridge before leaving, we assumed that if a child had the girl figure return to the empty fridge, it was because the Duplo girl wanted to fetch her bananas. Hence, children's responses were coded as correct if they moved the girl figure to the empty fridge and incorrect if they moved the Duplo girl to the fridge with the bananas.

In our first experiment, we administered the task as described. Then, in two follow-up experiments, we further investigated the effects of the two sets of task variations—one during the displacement phase and one during the test phase—intended to help perspective tracking.

Experiment 1

Participants

Twenty-eight children were recruited from a local nursery in Salinas (Asturias, Spain). The nursery is part of a public primary school and serves middle-class families. The children had been attending nursery for 3 months. The group consisted of 15 girls and 13 boys, and their mean age was 3.5 years (range = 3.0–4.0).

Design and procedure

Children were tested individually by the first author in a quiet area of their nursery. Each session lasted approximately 10

min. All children were tested on two verbal false-belief tasks: the standard "Smarties" task (Hogrefe, Wimmer, & Perner, 1986) and the Duplo task (in that order).

The Smarties task served as our benchmark because Wellman and his colleagues (2001) reported in their meta-analysis that children performed similarly in the Smarties task and the standard displacement false-belief task. Furthermore, Wellman and his colleagues showed that even those task variations that significantly improved children's performance on verbal false-belief tasks did not allow 3-year-olds to go from below-chance to above-chance performance, and therefore the crucial measure in our study was to compare children's performance on the two tasks to chance level.

Results and discussion

Of the 28 children who participated in the study, 6 failed to give a response in the Smarties task. Of the remaining 22 children, 5 passed the task, whereas 17 failed it (22.7% success rate). In the Duplo task, 1 child failed to cooperate, and 2 children gave two responses (i.e., they first moved the girl figure toward the empty fridge and then moved it to the fridge with the bananas), so data from all 3 were removed from the analysis. Of the remaining 25 children, 20 passed the task, whereas 5 failed it (80% success rate).[4]

The critical test of our hypothesis was a comparison between children's performances on the two false-belief tasks relative to chance performance. A McNemar test with continuity correction revealed a significant difference in children's performance on the two tasks, $\chi^2(1, N = 19) = 6.750$, $p < .01$. Moreover, although children performed significantly below chance level in the Smarties task ($p < .042$, two-choice binomial test, two-tailed), they performed significantly above chance level in the Duplo task ($p < .005$, two-choice binomial test, two-tailed).

To test the assumption underlying the correct responses (i.e., that, according to the child, the Duplo girl was going back to the empty fridge to fetch her bananas), we ran a control true-belief condition in which the Duplo girl herself moved the bananas from one fridge to the other. We tested 14 children from the same nursery (6 girls and 8 boys; mean age = 3.10 years, range = 3.7–4.2). Two children did not cooperate, and a third child was eliminated from the analyses because he gave two conflicting answers (i.e., he said the girl was going to get her bananas but then moved the girl figure to the empty fridge). Of the remaining 11 children, 10 moved the girl figure to the fridge containing the bananas, and 1 moved it to the empty fridge. The preference for the fridge with the bananas was reliable ($p < .012$, two-choice binomial test, two-tailed), which supports our interpretation of the false-belief data.

Our results support the hypothesis that 3-year-old children are able to pass a verbal false-belief task provided that they are allowed to keep track of the protagonist's perspective. The question remains, however, as to which of the two sets of variations introduced to the standard false-belief task were more effective in allowing children to succeed in the task.

Experiment 2a

Participants

Nineteen children were recruited from the same nursery as in Experiment 1. Children in this group had been attending nursery for 2 months. The group consisted of 10 girls and 9 boys, and their mean age was 3.5 years (range = 2.10–4.0).

Design and procedure

Testing conditions were the same as in Experiment 1. A similar set of Duplo toys was used, and the protocol was the same in all but one respect: After the Duplo girl put the target objects in one of the two locations on the table, the experimenter made the girl figure disappear from the scene by suddenly dropping her into a bag of toys under the table. The experimenter did not comment on the girl's disappearance and instead engaged the child's attention by continuing to play with the remaining toys on the table. On the basis of our main hypothesis, we expected that this manipulation would interfere with children's ability to track the Duplo girl's perspective and thus diminish their performance.

It should be noted that as the experimenter was displacing the target object, she acted as secretively as she did in Experiment 1. However, because she did not refer to the Duplo girl (or indicate who was being deceived), we assumed that the element of deception would not facilitate children's perspective tracking in this version of the Duplo task.

Results and discussion

Of the 19 children who participated in the study, 2 did not cooperate when they were asked to play with the girl. Of the remaining 17 children, 3 passed the task, whereas 14 failed it (17.6% success rate). These results reveal below-chance performance ($p < .014$, two-choice binomial test, two-tailed). A chi-square test with Yates correction revealed a significant difference in children's performance on the Duplo task in Experiments 1 and 2a, $\chi^2(1, N = 42) = 13.464$, $p < .001$.

These results confirm that the perspective-tracking variations introduced in the displacement phase of the Duplo task in Experiment 1 were crucial to the children's success. Apparently, the perspective-tracking variations in the test phase were not sufficient for 3-year-olds to recover from the disruptive effect of the Duplo girl's sudden disappearance in Experiment 2a.

As we expected, having the experimenter act secretively did not prevent children from performing below chance level when their perspective tracking was disrupted. We suggest that to the extent that deception facilitates false-belief reasoning (as it may have done in Experiment 1), it does so because deception helps children stay tuned to the perspective of the character who is being deceived.

Having established that the perspective-tracking variations in the displacement phase of the Duplo task enhanced children's performance, we next sought to determine whether these variations alone would suffice to enable 3-year-olds to

pass a verbal false-belief task using the standard probe question.

Experiment 2b

Participants

Eighteen children were recruited from the same nursery as in Experiments 1 and 2a. The children in this group had been attending the nursery for 2 months. The group consisted of 10 girls and 8 boys, and their mean age was 3.4 years (range = 2.10–3.9).

Design and procedure

The testing conditions were the same as in Experiments 1 and 2a. The Duplo toys and the protocol were the same as in Experiment 1, with this exception: At the end of the narrative, instead of inviting the child to play with the Duplo girl, the experimenter kept hold of the figure and asked the child the standard false-belief question: "Where will the girl look for the bananas?" As in a standard false-belief task, children in Experiment 2b had to answer the probe question either verbally or by pointing to one of the two locations.

Results and discussion

Of the 18 children who participated in the study, 4 passed the task, whereas 14 failed it (22.2% success rate). Children's performance was below chance ($p < .032$, two-choice binomial test, two-tailed). A chi-square test with Yates correction revealed a significant difference in children's performance on the Duplo task in Experiments 1 and 2b, $\chi^2(1, N = 43) = 11.920$, $p < .002$. Moreover, children's performance in Experiments 2a and 2b was comparable, $\chi^2(1, N = 35) = 0.114$, $p = .735$.

These results show that the high performance observed in Experiment 1 was contingent on our perspective-tracking variations in the displacement phase as well as in the test phase. Presumably for the reasons discussed previously, the standard false-belief question, "Where will *x* look for *y*?" throws young children off track. This is an important finding because hundreds of studies have used this question as a diagnostic for false-belief understanding.[5]

General Discussion

The aim of our study was to investigate the paradoxical results found in the theory-of-mind literature, where it has been reported that infants are able to pass nonverbal versions of the false-belief task (Baillargeon et al., 2010; Onishi & Baillargeon, 2005) but children under 4 years old fail myriad versions of the verbal false-belief task (Wellman et al., 2001; Wimmer & Perner, 1983). Here, we hypothesized that those differences were caused, at least partly, by accidental differences between the tasks that were used to test these two age groups: Nonverbal versions of the false-belief task allow infants to track a person's perspective during a sequence of events, whereas verbal versions of the task tend to disrupt the perspective-tracking process in various ways, which may be critical for younger children.

In our study, we used a new verbal false-belief task designed to minimize these disruptions. As expected, 3-year-olds (mean age = 3.5 years) were able to pass this task, with a success rate of 80%. Moreover, their performance in the new false-belief task was significantly better than that in a standard task, going from below chance to above chance level. Two follow-up experiments confirmed that allowing the children to keep track of the protagonist's perspective during the false-belief narrative was crucial to their success. Furthermore, instead of making the child choose between the two possible answers to the standard probe question, we used open questions inviting the child to continue acting out the story.

Rather than trying to solve the theory-of-mind paradox by drawing a distinction between different types of mind-reading systems or knowledge available to infants and young children (Apperly & Butterfill, 2009; Baillargeon et al., 2010; Clements & Perner, 1994; de Bruin & Newen, 2012), we assumed only what is evident from the infant studies, namely, that from a very early age, children are able to track other people's perspectives. This fundamental ability allows them to anticipate other people's actions, even when their inferences are based on false information. What our study shows, in addition, is that it is relatively easy to interfere with young children's perspective-tracking ability. Therefore, in a laboratory setting, it is necessary to take measures to minimize the disruption of the perspective-tracking process to ensure that 3-year-olds pass a verbal false-belief task.

The old debate about what changes between ages 3 and 4 years in theory-of-mind development can be reevaluated in light of our findings. How does the perspective-tracking ability of 3-year-olds develop in order to enable them to pass the standard false-belief task at around age 4 years? One possibility is that with an increase in their executive control comes an improved ability to stay tuned to the protagonist's perspective. Although this is highly likely (with school children generally having a better capacity to concentrate than preschoolers), we have found further evidence suggesting that even adults' perspective tracking can be momentarily interrupted by subtle task manipulations (Rubio-Fernández, 2012). The differential performance of 3- and 4-year-olds in the standard false-belief task is therefore more likely to result from an improvement in their capacity to recover from the disruption of their perspective tracking by task manipulations than from an increased capacity to stay with the protagonist's perspective throughout the task.

However, our main concern is not to advocate any particular view on development. Rather, it is to argue for a reinterpretation of the data. As discussed in the introduction, thus far the experimental record has shown that although infants can pass

nonverbal versions of the false-belief task, children under 4 years old have always failed verbal versions of the same task. This pattern of results led researchers to postulate a qualitative change in development at the end of age 3 years, which all dualist theories of false-belief reasoning seek to account for. Our findings suggest that this alleged change may be an artifact of widely adopted design features that are extrinsic to the verbal task. To be sure, we cannot rule out the possibility that a developmental discontinuity may eventually be found, perhaps at a younger age. But that remains to be seen.

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Notes

1. This is not to imply that introducing a second character will always cause children to fail the task. For example, if the experimenter who acts as the protagonist creates rapport with the child (thus allowing the child to identify with his or her perspective), then the introduction of a second character may be less disruptive (cf., e.g., Southgate, Chevallier, & Csibra, 2010). The more general point here is that no single factor need be decisive across the board.

2. Although we believe that this manipulation facilitated perspective tracking, we do not want to suggest that removing the main character from the scene necessarily impedes children's performance. As several infant studies have shown, the mere fact that the protagonist temporarily leaves the child's field of vision does not result in his or her losing track of that character's perspective (see, e.g., Onishi & Baillargeon, 2005). It is important to note, however, that the disruptive effect of the protagonist's disappearance is likely to be increased in verbal false-belief tasks in which the experimenter continues with the story and shifts the focus of attention to the secondary character (e.g., Baron-Cohen, Leslie, & Frith, 1985; Wimmer & Perner, 1983).

3. Previous studies suggest that introducing deception may help young children succeed in verbal false-belief tasks (Chandler, Fritz, & Hala, 1989; Sullivan & Winner, 1993; also see Wellman, Cross, & Watson, 2001). Although the experimenter in our study was clearly acting behind the Duplo girl's back, this manipulation was very subtle compared with manipulations in previous studies, in which children were asked to join a plot to deceive the protagonist. This distinction is important because it means that the issue of whether children really understand trickery does not apply here (see Sodian, Taylor, Harris, & Perner, 1991).

4. To see whether the older children in our sample performed better than did the younger ones, we divided the children into two age groups: those under 3.6 years and those 3.6 years or above. The performance of the two groups did not differ significantly in any of the four false-belief tasks administered in the study (all $ps > .270$, chi-square tests, two-tailed).

5. In their meta-analysis, Wellman and his colleagues (2001) suggested that future research should combine those variations of the false-belief task that have been shown to independently improve children's performance up to chance level. We stress that our study was not intended to implement this suggestion. In fact, when our own task variations were used separately, children performed below chance level. The aim of our task was to facilitate perspective tracking throughout the experiment, and our results indicate that, indeed, no single factor was decisive.

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