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Matched False-Belief Performance During Verbal and Nonverbal Interference

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Abstract

Language has been shown to play a key role in the development of a child's theory of mind, but its role in adult belief reasoning remains unclear. One recent study used verbal and nonverbal interference during a false-belief task to show that accurate belief reasoning in adults necessarily requires language (Newton & de Villiers, 2007). The strength of this inference depends on the cognitive processes that are matched between the verbal and nonverbal inference tasks. Here, we matched the two interference tasks in terms of their effects on spatial working memory. We found equal success on false-belief reasoning during both verbal and nonverbal interference, suggesting that language is not specifically necessary for adult theory of mind.

Keywords: Theory of mind; Language; Verbal shadowing

1. Introduction

The false-belief task has become a classic way of testing children's understanding of complex mental states (i.e., their ''theory of mind,'' Wimmer & Perner, 1983). Success on the task requires children to realize a character's beliefs can diverge from reality and to predict the character's actions accordingly. The age at which children can overtly predict actions based on false-beliefs varies across individual and tasks (Jenkins & Astington, 1996; see also Onishi & Baillargeon, 2005 for implicit false-belief reasoning at 15 months). One factor that explains some of the individual and group differences in false-belief task performance is language ability (Astington & Baird, 2005).

Evidence for a link between theory of mind and language comes largely from three types of studies. First, studies have found that language abilities, including both general syntactic

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abilities and, specifically, understanding the semantics of mental state terms (think, know, believe), are correlated across individuals and across groups with success on false-belief tasks (Astington & Jenkins, 1999; Dunn, Brown, Slomkowski, Tesla, & Youngblade, 1991; Milligan, Astington, & Dack, 2007; Watson, Painter, & Bornstein, 2002; Wellman, Cross, & Watson, 2001). Second, children who originally show no understanding of false-belief are able to pass the false-belief task after receiving training on specific linguistic structures (Appleton & Reddy, 1996; Clements, Rustin, & McCallum, 2000; Hale & Tager-Flusberg, 2003; Slaughter & Gopnik, 1996). In one such study, exposure to deceptive scenarios without language did not improve false-belief performance—children needed training on the linguistic form of false-beliefs (Lohmann & Tomasello, 2003). Third, deaf children born to hearing (i.e., nonsigning) parents shows marked delays in theory of mind development compared with deaf children born to signing parents (Peterson & Siegal, 2000; Schick, de Villiers, de Villiers, & Hoffmeister, 2007; de Villiers, 2005; Woolfe, Want, & Siegal, 2002). Furthermore, a study in Nicaragua reports that deaf adults who had no words for mental states failed nonverbal false-belief tasks, even with years of social experience; when these adults acquired terms to describe beliefs linguistically, they subsequently passed the same tasks (Pyers & Senghas, 2009).

This close developmental relationship between theory of mind and language can be interpreted in at least two ways. Language could provide a necessary scaffold for the normal development of a mature theory of mind, for example, through conversational exposure to mental state concepts (Astington & Baird, 2005). Alternatively, linguistic resources could be necessary for theory of mind reasoning itself (Newton & de Villiers, 2007; de Villiers & de Villiers, 2000); for example, the syntactic structures supporting sentence complementation could be necessary to even represent a false-belief.

Some evidence that linguistic (especially syntactic) resources are a developmental scaffold for theory of mind, but no longer necessary in adulthood, comes from studies of acquired aphasia. These individuals develop a typical theory of mind, but then subsequently lose syntactic processing abilities in adulthood because of catastrophic left-hemisphere stroke. For example, one patient has severe agrammatic aphasia and performs at chance on simple sentence comprehension tasks (i.e., cannot distinguish between ''Kate kissed John'' and ''Kate was kissed by John'') but is nevertheless successful on false-belief tasks (Siegal, Varley, & Want, 2001; Varley & Siegal, 2000). An even more striking case is a patient with severe linguistic impairment, including extremely poor comprehension of relevant grammatical structures (i.e., embedded complement clauses, relative clauses) who shows intact first- and even second-order nonverbal false-belief reasoning (Apperly et al., 2006b). Together, these studies provide evidence that in adulthood, theory of mind can function without language (or at least without linguistic syntax).

In contrast, Newton and de Villiers (2007) provide influential evidence that language is indeed necessary for typical adult theory of mind reasoning, using verbal shadowing. They find that participants' performance on a false-belief task is impaired while simultaneously performing a verbal interference task, compared with a nonverbal rhythm interference task. If participants make false-belief errors specifically because of the unavailability of language resources during verbal interference, this is good evidence that language resources are necessary, on-line, for adult theory of mind reasoning.

How can the results with aphasic patients be reconciled with the results of the verbal shadowing experiment? There are at least two possibilities. One possibility is that adults typically require language to think about false-beliefs, but can slowly learn new strategies for false-belief reasoning when linguistic resources are permanently removed (accounting for the eventual success of patients who are aphasic for years; Siegal et al., 2001; Apperly et al., 2006b).

Another, not mutually exclusive, possibility is that false belief reasoning is impaired by the absence of working memory and executive resources consumed by verbal shadowing, rather than specifically by the absence of linguistic representations. To test this hypothesis, it is necessary to test whether verbal and nonverbal interference tasks have different effects on false-belief reasoning, in spite of consuming similar working memory and executive resources. Newton and de Villiers (2007) provide evidence that their verbal and rhythm shadowing task caused similar interference on a third task: face perception. However, face perception does not require spatial memory or executive processes and, therefore, may not have been a sensitive measure of differences in the working memory demands of the two tasks.

In two experiments, we manipulate the difficulty of the interference tasks and then match them using a spatial memory task similar in executive demand to the false-belief task. We also expand the false-belief task to include more variable stimuli and to reduce the rate of chance performance. We hypothesize that when the working memory and executive demands of the interference tasks are matched, false-belief reasoning will be equally impaired by both verbal and nonverbal interference.

2. Experiment 1

This experiment replicates the methodology used by Newton and de Villiers (2007), but it uses an expanded battery of interference tasks of varying difficulty.

2.1. Method

2.1.1. Participants

Seventy-two MIT undergraduates participated in the study for payment. All participants were native English speakers and had normal or corrected-to-normal vision. Subjects gave written informed consent in accordance with the requirements set by the MIT internal review board. No other demographic information was collected.

2.1.2. Procedure

The false-belief task procedure was identical to that of Newton and de Villiers (2007). Participants watched a video of live actors who were dressed as animals; in each video a rabbit hid carrots under a box, and then a cat came and moved the object first to a second box, and then to a third box. During the movements, the rabbit was either present (''true-belief'') or absent (''false-belief''). Participants judged the most likely ''ending'' of the video (for more details, see Newton & de Villiers, 2007).

The four interference tasks were no interference, easy rhythm shadowing, hard rhythm shadowing, and verbal shadowing. In the no inference control condition, subjects performed the trial without interference. For the rhythm tasks, subjects heard alternating 4/4 measures of rhythm and silence (average notes per measure: 5.89; range: 4–8). During the silence, subjects echoed the preceding rhythm by tapping on the table. The easy rhythm was played at 125 bpm; the hard rhythm was sped up to 150 bpm. For the verbal task, subjects listened to a constant stream of English speech (an excerpt from *The World Is Flat*, by Thomas L. Friedman, played at 180 wpm) and repeated out loud what they heard as quickly as possible. Subjects listened to both interference tasks through headphones.

Each subject participated in two trials: one true-belief and one false-belief trial, each paired with one interference condition. Individual subjects participated in verbal interference and hard rhythm interference, or easy rhythm interference and no interference. The presentation order of belief type and pairing with interference task was counterbalanced across participants. Differences between conditions were tested using Fisher's exact test.

2.2. Results

There was no interaction of belief type and interference task between any conditions; however, there was a main effect of belief in all interference tasks (Fig. 1). Across all tasks, false-belief performance was worse than true-belief (Fisher's exact test, $p < .01$), including in the no interference control condition (true-belief: 16/16 correct; false-belief: 12/16 correct). Performance did not differ between no interference and easy rhythm shadowing, either for true- ($p > 0.5$) or false-beliefs ($p > 0.5$). Performance in the true-belief condition was at ceiling in both no interference and easy rhythm (16/16 correct), replicating Newton and

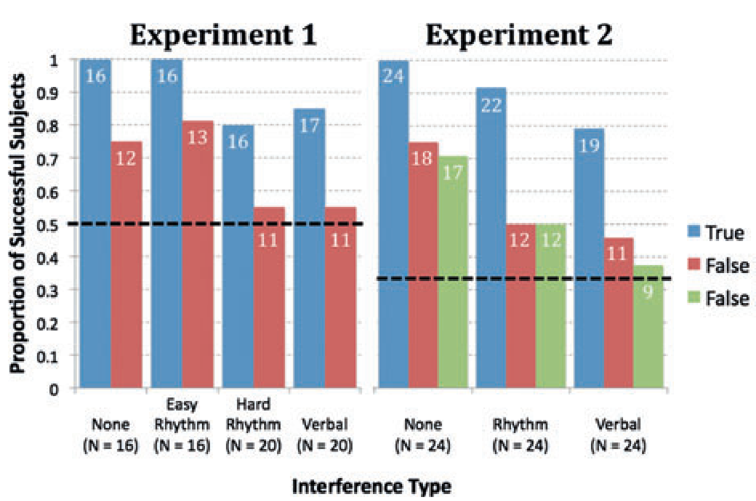

Fig. 1. Proportion of successful subjects in each interference task from Experiments 1 and 2 broken down by belief type. The dotted line indicates chance performance for Experiment 1 (50%) and 2 (33.3%). However, it is unlikely that participants in Experiment 2 were simply guessing on false belief trials: Rather, 52/64 incorrect answers were ''reality'' errors.

de Villiers (2007) results in their rhythm condition. Performance in the hard rhythm shadowing condition was worse than in the easy rhythm ($p < .025$) but matched to performance during verbal shadowing, both for true- ($p > 0.5$) and false-beliefs ($p > 0.5$).

2.3. Discussion

The results show that false-belief reasoning in this task is more difficult than true-belief reasoning and is even more difficult during interference tasks with high working memory demands (both hard rhythm and verbal shadowing). There was no specific effect of verbal shadowing (compared with hard rhythm shadowing) on false-belief reasoning. However, this procedure did not provide a sensitive and independent measure of the demands of the interference tasks. In Experiment 2, we used an independent task to match the demands of the interference tasks.

3. Experiment 2

In Experiment 2, we matched the working memory and executive demands of the verbal and nonverbal interference tasks on an independent spatial memory task. We then used these matched interference tasks on an expanded version of the nonverbal false-belief reasoning task. Experiment 1 allowed for only two response options (right/wrong) and hence false-belief errors (choosing where the object really is, rather than where the character believes it is) could not be distinguished from random guesses. For Experiment 2, we added a new condition in which the agent sees only part of the sequence of transfers, and we added a third response option. As a result, chance performance was lowered from 50% to 33.3%, and guesses could be distinguished from reality errors.

3.1. Method

3.1.1. Participants

Seventy-two MIT undergraduates (34 men) participated in the study for payment. All participants were native English speakers and had normal or corrected-to-normal vision. Subjects gave written informed consent in accordance with the requirements set by the MIT internal review board. No other demographic information was collected.

3.1.2. Stimuli

The stimuli were animations created in Microsoft Powerpoint. The animations were simpler, and perhaps less odd, than the films of costumed actors. The scene was a wood floor with a wall in the background and three buckets (red, blue, and green) in the foreground. A mouse entered through a hole in the middle of the back wall. A cat then always entered from off-screen to the left and exited off-screen to the right. In each animation, the mouse hid some cheese in one bucket; the cat then moved the cheese first to a second bucket, and then to the third bucket (in a pseudo-randomized sequence). The mouse was present for both movements (''true-belief'') or was absent, inside the hole, for both movements (''false-belief 1''), or was present for the first movement, but then absent for the second movement (''false-belief 2'').

3.1.3. Interference tasks

The difficulty of the interference tasks was matched in a pilot spatial memory study (similar to the procedure in Olesen, Westerberg, & Klingberg, 2004). Individual squares in a 3×3 grid flashed blue in a random order. The number of squares in the sequence was determined by a two-up, one-down adaptive staircase procedure. Subjects (n = 14) clicked on a blank grid to reproduce the sequence that flashed. In a within-subjects design, each subject performed the spatial task while performing three verbal and three rhythm shadowing interference tasks of varying difficulty, in counterbalanced order.

Settings for the verbal and nonverbal interference tasks were chosen such that the number of successful spatial positions reproduced in sequence was matched. The verbal task was an excerpt from *The World Is Flat*, by Thomas L. Friedman played at 198 wpm. The rhythm task was played at 140 bpm. Performance on these rhythm and verbal interference tasks did not differ from each other (mean sequence length ± SE, rhythm: 4.8 ± .2; verbal: 4.7 ± .2; $t(26) = .21$, $p > 0.7$) but was significantly worse than no interference (none: 5.9 ± .3; none vs. rhythm: $t(26) = 2.9$, $p < .005$; none vs. verbal: $t(26) = 3.11$, $p < .005$).

3.1.4. Procedure

Each of the three trials started with an instruction screen. Then, after 10 s of black screen, the movie began. When the movie ended, three images were displayed next to each other showing the mouse approaching the three different response options (red, blue, or green bucket). The subject responded by pressing a key in the middle of the keyboard covered with a red, blue, or green sticker to match the bucket options. After the subject responded, the screen returned to black for another 10 s. Subjects performed the interference task throughout the entire duration of the trial, starting after the instruction screen and ending after the final 10 s of black screen.

Each subject saw each of the three belief conditions (true, false 1, false 2) once and performed each of the three interference tasks (verbal, rhythm, none) once. The order in which they saw the belief condition and the pairing with the interference tasks was counterbalanced across subjects. The placement and movement of the cheese was randomized across subjects.

3.2. Results

A logistic regression with belief type (true, false 1, false 2), interference type (verbal, rhythm, none), and trial number (first, second, third) as regressors, and subject answer (correct/incorrect) as the dependent variable, shows a significant effect of interference type (beta = 1.9, $p < .05$, exp(b) = 7.0) and a marginal effect of belief type (beta = −1.3, $p < .08$, exp(b) = .3). There was no effect of trial number ($p > .5$). To examine which conditions were driving the effect of interference type, regressions were run using only pairs of interference type. There was a significant effect of verbal interference versus no interference (beta = 2.6, $p < .02$, exp(b) = 13.8) and of nonverbal interference versus no interference (beta = 4.2, $p < .05$, exp(b) = 63.4). However, there was no significant effect of interference condition, when comparing verbal versus nonverbal interference (beta = .60, $p > .5$, exp(b) = 1.9).

If verbal interference specifically impaired false belief reasoning we would expect to see either: (a) an interaction between interference type and belief type, or (b) a significant effect of interference type on the false-belief trials. Neither prediction was confirmed. A analysis with belief type (true, false), interference type (verbal, rhythm), and an interaction term as regressors showed no significant interaction (beta = −.36, $p > .5$, exp(b) = .69). An analysis of just the false-belief trials with interference type (verbal, rhythm) as a regressor showed no significant effect of interference type (beta = .25, $p > .5$, exp(b) = 1.2).

In both false-belief conditions, subjects could make a ''reality error'' by choosing the actual location of the cheese rather than where the mouse thinks it is, or could just be guessing. Subjects made the ''reality error'' significantly more than an error from simply guessing (54/62 reality errors, expected 31; binomial test, $p < .0001$).

3.3. Discussion

The results confirm those found by Experiment 1—when the working memory demands of the interference tasks are matched on an independent spatial task, there is no difference in accuracy of false-belief reasoning during verbal and nonverbal interference. In addition, the error analysis shows that participants are not simply guessing. Instead, they systematically predict that the mouse will look for the cheese in its true current location.

4. General discussion

We found that participants perform equally poorly on false-belief reasoning during verbal or nonverbal interference, when the interference tasks are matched on their working memory demands. When working memory demands of the interference task are high, adult participants track belief contents less effectively, and instead rely on the true location of the object to predict behavior, similar to the performance of young children. These results converge with a recent study that found no disproportionate deficit in belief reasoning (compared with physical reasoning) during verbal shadowing (Fouget d'Arc & Ramus, 2011).

Do errors on the current task actually reflect errors of theory of mind? Although the error analysis shows subjects are not simply guessing, their mistakes may be due to failures in the executive processes or task pragmatics required by the false-belief task (Bloom & German, 2000), rather than to difficulties *representing* beliefs. Notably, participants were incorrect on 25% of the false-belief trials, even in the absence of any interference. It is very unlikely that 25% of adult participants cannot reason accurately about false-beliefs. More likely, this experimental paradigm makes the relevance of the character's beliefs less salient; participants may simply not have recognized that they needed to track the character's beliefs

(Apperly, Riggs, Simpson, Chiavarino, & Samson, 2006a; Keysar, Lin, & Barr, 2003). If so, verbal (and nonverbal) interference may increase errors by making it even harder to recognize the pragmatics of the false-belief task, rather than by interfering with the representation of beliefs per se. Future studies may benefit from testing more complex situations that elicit sophisticated theory of mind reasoning in adults, such as moral judgment (Young, Camprodon, Hauser, Pascual-Leone, & Saxe, 2010; Young & Saxe, 2009).

Also note that the current results do not show that linguistic resources are never necessary for false-belief reasoning. Consistent with Newton and de Villiers (2007), we found that verbal shadowing led to a striking impairment in adults' false-belief performance. Although we found that this effect was not specific to verbal shadowing, linguistic resources may nevertheless contribute to false-belief reasoning when working memory is not impaired. An important question, not tested here, is whether typical adults can ever successfully reason about false-beliefs without language, as do aphasic patients (Apperly et al., 2006b; Siegal et al., 2001; Varley & Siegal, 2000). Consistent with this possibility, adults do show some sensitivity to a character's true- or false-beliefs during verbal shadowing (Fouget d'Arc & Ramus, 2011). Alternatively, language may offer a default strategy for representing beliefs; switching to nonverbal strategies when language resources are unavailable may take time or training. Future studies should test this possibility.

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