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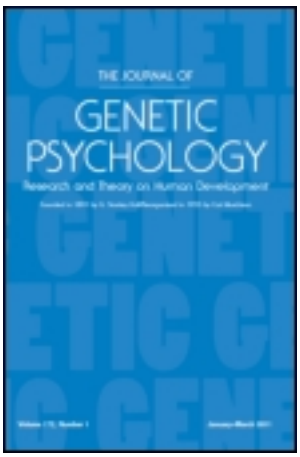

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Narrative Discourse and Theory of Mind Development

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Narrative Discourse and Theory of Mind Development

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ABSTRACT. The authors examined experimentally whether exposure to social discourse about concepts related to mental states could promote changes in children's theory of mind understanding. In 2 studies, 3- to 4-year-old children were assigned to either a training or a no training control condition. All children were administered several theory of mind measures at pretest and 2 posttests. Training was not effective in improving performance in Study 1 (n = 37); but in Study 2 (n = 54), modifications of the training procedure led to significant improvements on measures of false belief and deception from pretest to 1st posttest. The findings support the influence of social discourse on children's theory of mind development.

Key words: discourse, mind, narrative, training

A THEORY OF MIND, attributing mental states to oneself and others (Whiten, 1994), can be conceptualized as a set of concepts to be learned. During the infancy, toddler, and preschool years, a child's experiences and biological maturation interact to produce such learning. In the present study, we investigated narrative discourse as a critical determiner of theory of mind development by manipulating exposure to such discourse experimentally.

Support for the importance of social interaction in the development of theory of mind understanding has been provided by family research (e.g., Astington & Jenkins, 1995; Brown, Donelan-McCall, & Dunn, 1996; Furrow, Moore, Davidge, & Chiasson, 1992; Moore, Gilbert, & Sapp, 1995; Sabbaugh, 1995), which suggests that discourse among family members is a context in which children learn about feelings, thoughts, and beliefs and how to talk about such concepts. Findings from longitudinal studies have indicated that the frequency of family discourse about emotions is related to children's later understanding of feeling states (Dunn, Brown, & Beardsall, 1991), and the frequency of mother–child discussions

about feeling states and causes of behavior predict preschoolers' subsequent performance on theory of mind tasks (Dunn, Brown, Slomkowski, Tesla, & Youngblade, 1991). Moreover, children's theory of mind performance is related to the number of siblings (Perner, Ruffman, & Leekam, 1994), adults, and older children (Lewis, Freeman, Kyriakidou, Maridaki-Kassotaki, & Berridge, 1996) with whom children have contact. In addition to frequency and content of discussions, mother–child interaction styles have been implicated in children's theory of mind performance. Mothers' tendencies to treat their 11-month- to 3-year-old children as if their behavior is mindful and intentional are related positively to children's understanding of others at 5 years of age (Meins & Fernyhough, 1999), and children's performance on theory of mind tasks is related to the frequency with which their mothers offer new information during discussions about past events (Welch-Ross, 1997). These data indicate that conversations with others provide contexts in which children learn about their own and others' thoughts and feelings.

Although the previously mentioned studies have been highly suggestive of the importance of social factors in theory of mind development, the studies were all correlational in design; thus, causal relations cannot be identified. To assess the causal influences on children's use of mental terms and understanding of false belief, researchers must experimentally manipulate children's exposure to information about mental states. Some researchers have already conducted studies along these lines, using task-specific training in which children receive direct feedback about the accuracy of their responses to questions about concepts related to false belief (Dockett & Smith, 1995; Slaughter, 1998; Swettenham, 1996). Slaughter and Gopnik (1996), for example, used an expected contents task as the pre- and posttest and another, similar, false belief task in training. During the training sessions, children received direct feedback regarding the accuracy of their answers and could modify subsequent answers accordingly. Training enhanced performance at posttest. Appleton and Reddy (1996) examined how children learn about concepts related to a theory of mind through conversation, but the authors also used an approach with parallel training and testing tech-

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niques. Thus, these studies did not address the question of whether naturalistic concept training, rather than task-specific training, can be effective. In the present study, we used training involving naturalistic social interactions, similar to those examined in the correlational research, and testing with traditional false belief tasks to expand our knowledge of potentially important components in theory of mind acquisition.

The social discourse intervention we used involved children's increased exposure to narratives and their content during interactions with an adult. Sophisticated representational demands (like abstract decontextualization) occur more frequently in caregiver–child conversation during book reading than in other types of interactions (Sorsby & Marlew, 1991). Conversations about written and oral stories are natural extensions of children's earlier experiences with the sharing of event structures, because they imitate and reproduce actions of others in the toddler and early preschool years (Nelson, 1996, 1999). Also important, narrative skills have been found to be related to false belief understanding (Charman & Schmueli-Goetz, 1998; Lewis, Freeman, Hagestadt, & Douglas, 1994). Thus, the world of stories might be a means through which children develop, practice, and redescribe their theory of mind understanding to more complex levels (Fernyhough, 1998; Karmiloff-Smith, 1995).

Much social discourse involves personal stories and "folk" psychology (Bruner, 1990). In addition to what one reads, people are constantly telling stories about their own or others' thoughts and beliefs as well as actions. Bruner identified these story components as the *landscape of consciousness* and the *landscape of action*, respectively, and argued that an individual must comprehend both landscapes simultaneously to understand a story (Bruner, 1988, 1990). Indeed, evidence suggests the importance of the landscape of consciousness for one's organizing and comprehending of stories. Feldman, Bruner, Renderer, and Spitzer (1990) found that participants who heard a story containing the landscape of consciousness, as opposed to one containing only the landscape of action, were able to provide information beyond what was directly given in the stories. Not only did they make references to the characters' thoughts—a logical consequence of hearing the characters' thoughts and feelings—but they also made more concluding interpretations of the story and were better able to organize the events of the story.

Astington (1990) has suggested that young children fail tests of false belief because they understand the landscape of action but not the landscape of consciousness (see also Johnson, 1988; Lewis, 1994). On a transfer task, for instance, a young child can understand that the candy bar has been moved from one cabinet to another, but he or she cannot simultaneously understand the importance of the character's belief. By 4 years of age, children can comprehend both landscapes concurrently, enabling them to comprehend that another's belief is his or her representation of reality, which is a critical point for understanding false belief. As children become more familiar with the structure of stories, they might

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learn about thinking–action relations through exposure to stories containing mental content.

In the present study, we manipulated children's exposure to social discourse, centered naturalistically around children's storybooks, to examine its affect on children's theory of mind understanding. Each training session involved discussion of the mental state concepts of false belief, deception, or appearance–reality in the context of story reading. The children's books selected for use contained multiple references to theory of mind concepts (Cassidy et al., 1998). For example, at appropriate, predesignated points during each story, the storyteller (experimenter) highlighted the characters' thoughts and actions and asked the children to explain them, thus involving the children in discussions about associations between people's thoughts and behavior. Training sessions in the first study included an activity to emphasize characters' thoughts and feelings. We hypothesized that the children who participated in the training condition would perform better on the theory of mind posttests than would the children in the control group.

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STUDY 1

Method

Participants

Forty-one 3- to 4-year-old children from three preschools in a small, rural city participated in the first phase of Study 1. Four children, however, did not complete the study (3 children were unavailable, and 1 child was ill during posttesting), leaving a final sample of 37 children (15 girls and 22 boys; mean age = 46 months; range = 35 to 55 months). Children were primarily Caucasian and middle class. They were assigned to one of two conditions: (a) training (n = 19; 9 girls and 10 boys) or (b) no training control (n = 18; 6 girls and 12 boys).

Design

The present investigation included three times of measurement: a pretest and two posttests. The first posttest occurred within 10 to 17 days (M = 13), and the second occurred within 27 to 41 days (M = 33) of completion of the training sessions. We had different individuals conduct the pretest, training, and posttests for each child to ensure that any changes in the children's performance on the measures across sessions were not related to the children's familiarity with the examiner.

Pretest

Before the training sessions, the children were pretested on a language assessment, three measures of false belief, two deception tasks, and two appear-

ance–reality measures. Children were tested individually on two separate occasions in a quiet part of their preschool. During one 10–20 min session, the children received the language measure, and during a second 10–15 min session, they received the false belief, deception, and appearance–reality measures. Previous research indicated that language comprehension was correlated with theory of mind performance (e.g., Astington & Jenkins, 1995); thus, language scores also were used in equating groups before the intervention (training) began. A single experimenter administered each language assessment, and one of three trained research assistants administered the theory of mind measures. A description of each measure follows.

Language. We measured children's language comprehension with the Test for the Auditory Comprehension of Language–Revised (TACL–R; Carrow-Woolfolk, 1985), which consists of three subscales assessing various aspects of verbal ability (word comprehension, morphology, and sentence comprehension). All three subscales were administered to each child. For each item, the experimenter read a word, a group of words, or a sentence to the child and instructed the child to point to one of three pictures that best corresponded to the experimenter's utterance. Standard scoring procedures were used.

Theory of mind assessment. Children's theory of mind was assessed with Wimmer and Perner's (1983) standard unexpected change task (see also Gopnik & Astington, 1988); a two-part unexpected contents task (Bartsch & Wellman, 1989; Lewis & Osborne, 1990); two deception tasks (a modification to the unexpected change task and an active deception task; see Lalonde & Chandler, 1995); and two perceptual appearance–reality distinction tasks (Flavell, Flavell, & Green, 1987). The gender of the character in each story matched that of the child, and all stories were acted out with props.

In the unexpected change task, the children were told a story about Maxi and her mother (for girls; for boys the characters were Max and his mother). In the story, Maxi and her mother return from the grocery store and put the chocolate they bought in the blue cupboard. Then, while Maxi is out of the room, her Mother moves the chocolate to the green cupboard. Before asking the children the test questions, the experimenter asked them three unscored comprehension questions to ensure that they understood the story: (a) "Where did the chocolate used to be?" (b) "Where is the chocolate now?" and (c) "Did Maxi see the chocolate being moved?" If the children answered any of these questions incorrectly, they were corrected (see Watson, Nixon, Wilson, & Capage, 1999, for a similar approach). Next, the children were asked two test questions: (a) "Where will Maxi think the chocolate is when she comes back?" and (b) "Where will Maxi first look for the chocolate when she comes back?" The correct response to each question was "The blue cupboard."

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After the children had been told the story of Maxi and asked the test questions, the experimenter asked them to consider another possibility:

> Now let's say that Maxi's sister enters the room before Maxi looks for the chocolate. Her sister is looking for the chocolate, but she doesn't know where the chocolate is, so she asks Maxi. But Maxi doesn't want her sister to find the chocolate because she knows her sister will eat it all.

Then, the children were asked the test question, "Where will Maxi tell her sister the chocolate is?" Children received a score of 1 for a correct response of "the green cupboard" and a score of 0 for the incorrect response, "the blue cupboard."

Next, the experimenter asked the children questions concerning their understanding of their own representational change, using an unexpected contents task (Lewis & Osborne, 1990). Children were shown a Band-Aid box and asked, "What do you think is inside the box?" Then they were shown the contents of the box, a toy car. Next, the box was closed, and the experimenter asked the children the first test question: "What did you think was in the box?" If the children responded that they thought Band-Aids were in the box, they received a score of 2. If the children did not answer this first question correctly, the experimenter provided a prompt: "What did you think was in the box *before* I took the top off?" If the children answered the prompt question correctly (e.g., they thought Band-Aids were in the box), they received a score of 1. If the children answered both of these questions incorrectly, they received a score of 0. Following the test questions, the experimenter asked the children a memory question—"Can you remember what is inside the box?"—to ensure that they remembered the contents of the box.

In the second part of the unexpected contents task, the children were shown the same Band-Aid box and a similar unmarked box containing Band-Aids. Following the procedure of Bartsch and Wellman (1989), the experimenter showed the children a doll named Bill (or Sarah, for girls) and told them that Bill has a cut, and he wants a Band-Aid. The doll then approached the Band-Aid box, and the children were asked the critical test question: "Why do you think he is looking in there?" Children who gave the correct response (Bill looked in the Band-Aid box because he *thought* it contained Band-Aids) received a score of 2. If the children either did not respond or did not mention beliefs, the experimenter provided a prompt: "What does Bill think?" Children received a score of 1 for a correct prompted response. Children who answered both questions incorrectly received a score of 0. Finally, the experimenter asked the children an unscored reality question: "Are the Band-Aids there really?" to ensure that they recalled the true contents of the box.

Subsequently, the children were led through a task that involved actively deceiving a character (see Lalonde & Chandler, 1995). They were introduced to a doll named John (or Sue, depending on the child's gender) and then were told that John knows there is candy in the green drawer but that he has to leave the room for a while. While John was "gone" the experimenter told the children,

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"Let's play a trick on John. Let's move the candy to the blue drawer." After the children moved the candy to the blue drawer, they were asked two test questions: (a) "Where will John think the candy is?" and (b) "When John comes back into the room, where will he first look for the candy?" Children received a score of 1 for each correct response of "the green drawer" and a score of 0 for each incorrect response, "the blue drawer."

Finally, we assessed the children's understanding of appearance–reality using two illusion tasks based on the work of Flavell et al. (1987). These tasks involved items that appeared to be something else (e.g., a candle that looked like an apple and a piece of wood that looked like an egg). The procedure was identical for both tasks. During each task, the children were shown the object, asked what the object was, and given the opportunity to hold the object to discover its real identity. Then the experimenter asked the two test questions: "Is it really and truly _____ or is it really and truly _____?" and "When you look at this with your eyes right now, does it look like _____ or does it look like _____?" Children received 1 point for each pair of questions they answered correctly. They received a total appearance–reality score ranging from 0 to 2, with each task worth 1 point.

Preliminary Analyses and Assignment to Conditions

After all of the children had been pretested, they were assigned to either the training or the no training control condition, such that the groups did not differ with regard to mean age, language, and theory of mind scores (*p*s > .10). To achieve this design, we randomly assigned first one child to the experimental condition and then another child, of a similar age with similar language and theory of mind scores, to the control group. This procedure was repeated until all of the children were assigned to conditions. After the first phase of the study, our analyses indicated that the groups remained comparable after data on the 4 children who were unable to complete the study were removed from the analyses (*p*s > .10; see Table 1).

Training

In groups of 3 to 4 children, the children in the training condition participated in 12 to 15 sessions, each lasting 15 to 25 min, over a period of 5 weeks. Each session, conducted by one of two experimenters (storytellers), began with a question to engage the children and involved the storyteller's reading a children's storybook, highlighting and discussing episodes containing mental references, and leading the children in a related activity. For example, one session about deception focused on the book titled *Hog-Eye* (Meddaugh, 1995), a story about a pig who tricks a wolf to prevent the wolf from eating her. Throughout the story, the storyteller highlighted episodes in which the pig tricked the wolf, and the storyteller asked the children questions about the characters' thoughts. After

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TABLE 1
Means and Standard Deviations of Pretest Scores for the Final 37 Participants in Study 1

	Condition			
	Training		No training control	
Pretest	*M*	*SD*	*M*	*SD*
Language	54.73	17.63	52.56	20.22
False belief	2.36	1.74	2.44	2.04
Deception	1.31	0.89	1.44	0.98
Appearance–reality	0.53	0.77	0.39	0.78

Note. Mean ages (and *SD*) of children in the training and no training control conditions were 46.21 (5.80) and 46.55 (5.89) months, respectively.

the story, the children took turns putting on a pig nose or a wolf nose and acting out a related episode of the story.

Children worked with the same storyteller across all sessions. To ensure procedural consistency, the storytellers met weekly to discuss the stories and activities and followed scripts designed for each session (see the Appendix for an example). All training sessions were conducted in a quiet place at the children's school. Children in the no training control condition did not participate in any sessions.

Posttest

Children were tested on the same false belief, deception, and appearance–reality tasks that were used for the pretest, with one modification. We altered the object in the unexpected contents/representational change task (i.e., we used a toy fish during the first posttest and a toy boat during the second posttest, instead of the toy car used during the pretest) to permit an accurate assessment of the children's understanding of representational change.

Results

Preliminary Analyses

Relations among the children's pretest scores on each of the theory of mind tasks were examined. A series of Pearson correlations indicated that the children's false belief and deception scores were correlated significantly ($r = .52$, $p < .01$), but neither score was related to the perceptual appearance–reality scores ($rs = .10$ and .10, $ps > .10$, respectively). In subsequent analyses, a composite score, the sum of each child's false belief and deception scores (range = 0–9) was used. The appearance–reality scores were analyzed separately.

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Across times of measurement, boys and girls performed similarly on the theory of mind measures, $F(2, 72) = 2.33$, $p > .10$; thus, we did not include gender in subsequent analyses. Our examination of the control questions indicated that the majority (92%) of the children's responses were correct; thus, these data also were not considered further.

Effect of Training

The means and standard deviations of the theory of mind scores by condition and time of testing are shown in Table 2. To examine the effects of the intervention, we conducted a 2 × 3 (Condition × Time) mixed model analysis of variance (ANOVA) with condition (training vs. no training control) as the between-subjects factor and time of testing (pretest, first posttest, second posttest) as the within-subjects factor. The results indicated that the intervention did not affect the children's composite theory of mind scores. $F(2, 70) = .94$, $p > .10$, $\eta^2 = .02$. There was, however, a main effect of time, $F(2, 70) = 10.03$, $p < .001$, $\eta^2 = .22$, indicating that the average performance of the children, collapsed across conditions, improved across times of testing. A series of paired-sample t tests indicated that the children's scores significantly improved from the pretest to both the first, $t(36) = 2.93$, $p < .05$, and the second posttests, $t(36) = 4.79$, $p < .001$, but their performance did not differ across the posttests, $p > .10$.

We conducted a second 2 × 3 (Condition × Time) ANOVA to examine changes in performance on the appearance–reality tasks. The main effect of time was marginally significant, $F(2, 70) = 2.84$, $p < .10$, and the Condition × Time interaction was not significant, $p > .10$. Although there was a trend for both groups of children to improve on the appearance–reality tasks across time, training did not have an effect on performance.

TABLE 2
Study 1: Means and Standard Deviations of Theory of Mind Scores Across Conditions and Times of Testing

	Pretest		Posttest 1		Posttest 2	
Condition	*M*	*SD*	*M*	*SD*	*M*	*SD*
Training						
False belief, deception	3.69	2.18	5.37	3.06	5.58	1.89
Appearance–reality	0.60	0.82	0.75	0.79	0.90	0.85
No training control						
False belief, deception	3.89	2.80	4.56	2.77	5.22	2.53
Appearance–reality	0.39	0.78	0.56	0.70	0.78	0.88

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Discussion

Our purpose in Study 1 was to manipulate experimentally children's exposure to naturalistic discussions about mental concepts, to provide support for the importance of discourse as one determining factor in children's theory of mind development. The present data did not support this hypothesis, but several aspects of the study suggest possible modifications that might alter the results. First, children were not excluded on the basis of initial theory of mind performance, as has been the case in previous work (e.g., Appleton & Reddy, 1996; Slaughter, 1998; Slaughter & Gopnik, 1996). We included all of the children because we sought to determine whether the intervention would lead to improvements in the performances of all of them, including those who performed well on the pretest. One problem with this approach, however, is that the inclusion of those children might have masked effects of the intervention, because the higher performing children did not have much room for improvement. The importance of more precisely differentiating children according to their initial ability in training studies of this sort has been suggested (see Siegler, 1996). To test this possibility, we analyzed the data of the children who scored 3 or lower on the pretest. Although the first posttest scores of the control and experimental groups differed in the desired direction (Ms = 2.86 and 5.00, respectively), the differences were not significant. With these inclusion criteria, however, the sample sizes decreased to 7 in each group; thus the power was low. We addressed this issue in the second study with a larger sample size.

The design of Study 1 also did not include a means to control whether the children actually attended to and retained the information conveyed in the stories and discussion. One way to address this issue would be to work with children individually. Doing so would eliminate the problem of children being distracted by other children and might increase the likelihood of the children discussing the story once it is completed. In addition, individually conducted training sessions might mimic more closely what the children encounter in their daily interactions at home. Consequently, in Study 2, the children participated in individual training sessions.

An additional point with regard to Study 1 concerns the finding that the average performance of children in both the intervention and control groups improved across time. One possible explanation for this finding is that across times of measurement, the children became increasingly familiar with the tasks. We used identical tasks at each assessment to ensure that any changes in performance did not result from differences in tasks, but this led to difficulties in interpretation of the data. Thus, in Study 2, we used measures at each time of assessment that were identical in format but different in specific content to ensure that changes in performance across time were not caused by familiarity with specific tasks. Moreover, the appearance–reality tasks were not included in Study 2 because children's performance on them was unrelated to false belief and deception scores in Study 1. We

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also administered posttests earlier in Study 2 to examine the immediate effectiveness of the training procedure. The first posttest occurred within 1 week, rather than 2 weeks, and the second posttest occurred within 3, rather than 4, weeks of the completion of the training sessions.

Furthermore, potentially confusing or redundant questions were excluded from the theory of mind measures. On the unexpected transfer task, for example, the children answered one test question rather than two. Moreover, the tasks were disconnected so that performance on each measure was independent of performance on the other measures. In Study 1, the first deception task involved our adding a scenario onto the Maxi task; thus, the children's performance on the deception task was restricted by their performance on the false belief portion of the task. Such an approach might have prevented an accurate assessment of the children's understanding of the concepts being tested.

Finally, the activities were excluded from the training sessions because the children seemed to prefer to move on to the next activity rather than to discuss the story after its completion. In Study 1, each intervention session had four main characteristics: (a) The session began with an opening statement and question (e.g., "When you play a trick on someone, you want them to think something that is not true. Have you ever played a trick on somebody?"), (b) stories were read and discussed concurrently, (c) the storyteller summarized and led a discussion of the story, and (d) the storyteller conducted an activity related to the topic of the story (e.g., acting out a portion of the story focusing on the characters' thoughts). Once the children learned the format of the sessions, they wanted to begin the activity, rather than discuss the story any further. In discussing the story after it had been read, we intended to encourage the children to think about the story and to integrate the events of the story with those of their lives (Dickinson & Smith, 1994). The activities seemed to be distracting for the children in Study 1, possibly preventing them from focusing on the mental concepts being discussed. Furthermore, the activities were not appropriate for single-child sessions.

STUDY 2

Method

Participants

Seventy-three 3- to 4-year-old children from six preschools in a small, rural university town participated in the first phase of Study 2. However, 19 children did not complete the study (14 performed too well on the pretest, 2 did not complete training, and 3 did not complete the posttesting), leaving a final sample of 54 children (25 girls and 29 boys; mean age = 41 months; range = 33 to 56 months). Children were primarily Caucasian and middle class. They were assigned to one of two conditions: (a) training (n = 26; 13 girls and 13 boys) or

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(b) no training control group (n = 28; 12 girls, and 16 boys). None of the children in Study 2 participated in Study 1.

Design

As in Study 1, the present investigation included three times of measurement: a pretest and two posttests. The first posttest occurred within 6 to 12 days (M = 8), and the second occurred within 20 to 28 days (M = 22) of completion of the training sessions. Again, to ensure that any changes in the children's performance on the measures across sessions were not related to their familiarity with the examiner, we arranged for a different individual to conduct the pretest, training, and posttests for each child.

Pretest

Before beginning the training sessions, we pretested the children on the language and theory of mind measures, as in Study 1 (excluding the appearance–reality task), with the following modifications to the measures.

Theory of mind assessment. The Maxi task used in Study 1 was also used in Study 2, and the children's responses were corrected in the same manner. Children, however, were asked only one test question: "Where will Maxi first look for the chocolate when she comes back?"

The second task involved a character (Bruce or Pam) tricking his or her sibling. This task was parallel to the first deception task used in Study 1, but it was independent of the Maxi task. Children were told,

> Here is Bruce. He took the candy out of the candy box and put it in this crayon box so that his brother would not find it. Bruce did not want his brother to eat the candy before Bruce got any. When Bruce's brother comes into the room, he asks Bruce where the candy is. Bruce decides to tell his brother something *completely wrong* so his brother will *not* find the candy.

Next, the children were asked the test question: "Where will Bruce *say* the candy is?" Children received a score of 1 for a correct response of in "the candy box" and a score of 0 for an incorrect response of in "the crayon box."

We then used the same unexpected contents task and scoring procedure from Study 1 to assess the children's understanding of their own representational change and their ability to explain another's false belief. However, the second part of the task that assessed the children's understanding of another's false belief was modified slightly. After the children saw what was in the new box, the experimenter reminded them what was in the Band-Aid box ("There are Band-Aids in this box and a toy car in the Band-Aid box") to ensure that the children who failed the representational change task were not prevented from passing the explanation task. The scoring procedures used in Study 1 were applied here.

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Finally, the children were led through the active deception task from Study 1. In contrast to the first study, however, the children were asked one test question: "When Sue [or John] comes back into the room, where will she first look for the candy?" We thought that two similar test questions might have confused the children in Study 1. The scoring procedure was identical to that used in the first study.

Assignment to Conditions

In contrast to Study 1, we excluded the children who received a score of 4 or higher (of 7 possible) on the pretest from subsequent phases of the study to maximize the potential effect of training. Fourteen children were excluded for this reason. After all of the children had been pretested, the same procedures as in Study 1 were used to assign the children to either the training or the no training control conditions. The groups did not differ with regard to average age, language, and theory of mind scores (*ps* > .10; see Table 3).

Training

Working individually with an experimenter, the children in the training condition participated in 13 to 15 sessions, each lasting 10 to 15 min, over 5 weeks. Training was conducted by two experimenters, but the children worked with the same experimenter across all sessions. To ensure procedural consistency, the experimenters met weekly to discuss the stories. The stories and discussion formats were identical to those used during Study 1, except that one story was replaced because the children in Study 1 had difficulties following the story line. All training sessions were conducted in a quiet room at the children's school. Children in the no training control group did not participate in any sessions.

TABLE 3
Means and Standard Deviations of Pretest Scores for the Final 54 Participants in Study 2

	Condition			
	Training		No training control	
Pretest	*M*	*SD*	*M*	*SD*
Language	34.92	14.99	33.12	13.27
False belief	0.65	0.89	0.75	1.04
Deception	0.23	0.51	0.25	0.52

Note. Mean ages (and *SD*) of children in the training and no training control conditions were 42.23 (5.87) and 40.43 (3.67) months, respectively.

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Posttest

During both of the posttests, the children were tested on tasks that were parallel to those used during the pretest. The format, test questions, and control questions of each task were identical at each time of measurement, but the content differed. For example, during the pretest, Maxi was looking for chocolate; during the first posttest Julie was looking for her dog; and during the second posttest Missy was looking for bread.

Results

Preliminary Analyses

Children's false belief and deception scores were related at both the first (r = .60, $p < .001$) and second (r = .42, $p < .01$) posttest (pretest data were not used because the inclusion criterion restricted the range of scores on both types of tasks). The analyses also indicated that false belief (r = .63, $p < .01$) and deception (r = .34, $p < .05$) scores were reliable from the first to the second posttest. An aggregate theory of mind score (range = 0–7) was used in subsequent analyses.

A 2 × 3 (Gender × Time) ANOVA indicated that boys' and girls' performance was not meaningfully different across time, $F(2, 104) = .795$, *ns*; thus, gender was not considered in subsequent analyses. Moreover, examination of the control questions indicated that the majority (90%) of the responses were correct; consequently, these data were not considered further.

Effect of Training

To examine the effects of training, we conducted a 2 × 3 (Condition × Time) mixed model ANOVA with condition (training vs. no training control) as the between-subjects factor and time of testing (pretest, first posttest, second posttest) as the within-subjects factor. The results indicated that there was a main effect of time, $F(2, 104) = 6.36$, $p < .01$, $\eta^2 = .11$, and a trend toward a significant Condition × Time of Testing interaction, $F(2, 104) = 2.42$, $p < .10$, $\eta^2 = .04$. When we further examined the data using tests of within-subjects contrasts, however, we found a significant Condition × Time of Testing quadratic interaction, $F(1, 52) = 4.53$, $p < .05$, $\eta^2 = .08$. Although children in both groups improved across time, those in the training condition improved more dramatically. In fact, a series of paired-sample t tests suggested that the children in the training group improved significantly from the pretest to the first, $t(25) = 3.56$, $p < .01$, and second, $t(25) = 2.27$, $p < .05$, posttest, but their performance did not change significantly across posttests. The scores of the children in the control group, however, did not differ significantly across any of the sessions, $ps > .10$. Table 4 contains the means and standard deviations of theory of mind scores by condition and time of testing.

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TABLE 4
Study 2: Means and Standard Deviations of Theory of Mind Scores Across Conditions and Times of Testing

Condition	Pretest		Posttest 1		Posttest 2	
	M	*SD*	*M*	*SD*	*M*	*SD*
Training	0.89_{ab}	1.11	2.19_{a}	2.15	1.81_{b}	2.14
No training control	1.00	1.12	1.25	1.65	1.50	1.60

Note. Means that share a subscript differ: $_{a}$ = $p < .01$, $_{b}$ = $p < .05$.

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Discussion

Our purpose in Study 2 was to modify the procedures used in Study 1 (inclusion criterion, task modifications, individual training, and increased sample size). The results indicated that participation in discussions about false beliefs, deception, and appearance–reality led to improvements in performance on measures of false belief and deception. Although the scores of children in both groups increased across time, participation in the training sessions significantly accelerated such improvement.

GENERAL DISCUSSION

The results of the present research indicate that through participation in conversations, the children showed improved performance on standard measures of theory of mind understanding as compared with those children who did not participate in any training. These data fit well with the notion that narrative is an important route through which the cultural transmission of information about complex mind–behavior relations occurs (Nelson, 1999). As a child's social world expands, he or she becomes more knowledgeable of events and better able to make evaluative comments about them. A child also becomes more adept at reasoning about different perspectives (Lewis, 1994), and event representations become structured around subjective reactions to events (Haden, Haine, & Fivush, 1997; Peterson & McCabe, 1994). Evaluative comments help children differentiate events and rearrange them into more complex, hierarchical forms, thus the essence of narrative emerges (Bamberg & Damrad-Frye, 1991; Fivush & Slackman, 1986).

Critical differences between the present research and previous training studies in the theory of mind literature were related to the nature of the training procedures. First, the intervention involved an experimenter reading stories about false belief, deception, and appearance–reality rather than our using a task-spe-

cific approach (e.g., Appleton & Reddy, 1996; Slaughter, 1998). Information gained from the training sessions could facilitate performance on the posttests only if children formulated a broader understanding of false beliefs, deception, and appearance–reality. This investigation, therefore, entailed a more rigorous test of concept acquisition; namely, whether what was learned in one context could generalize to another, seemingly unrelated, context.

Second, the training sessions contained other teaching techniques thought to be important for effective training, including intensive back-and-forth interactions (Arnold, Lonigan, Whitehurst, & Epstein, 1994; Bos & Anders, 1990; Dale, Crain-Thoreson, Notari-Syverson, & Cole, 1996; Klausmeier, 1992; Valdez-Menchaca & Whitehurst, 1992), direct instruction (Gersten, Woodward, & Darch, 1986), student and teacher questioning (Gilmore & McKinney, 1986), and the use of examples and nonexamples (Prater, 1987; Ranzijn, 1991). Each training session began with a question to prompt the children to think about the relevant concepts (e.g., Do you know what a trick is? Have you ever tricked anyone?), and throughout the session, the experimenter asked the children questions about the story. The children also were able to ask questions.

Finally, during the sessions, the experimenter highlighted examples of the relevant concepts, which included discussions of nonexamples (e.g., instances when the character had an accurate belief). The approach of this research adds to previous work that demonstrated the importance of direct feedback regarding performance (e.g., Appleton & Reddy, 1996; Slaughter & Gopnik, 1996). If researchers include these factors in training procedures, children might be better able to generalize their knowledge of theory of mind concepts across settings. In particular, such approaches might be effective with other populations (e.g., children with autism), for which there has not yet been successful generalization of theory of mind concepts (McGregor, Whiten, & Blackburn, 1998; Swettenham, 1996; Swettenham, Baron-Cohen, Gomez, & Walsh, 1996).

The present study had a few limitations. First, a control group in which children participated in discussions about concepts unrelated to theory of mind was not included. Thus, one might question whether the important aspect of training was participation in discussion about mental states or just one-on-one attention. Previous research clarifies this question to some degree. In their control condition, Appleton and Reddy (1996) read books to children individually. That social interaction did not improve children's theory of mind performance significantly. Although a direct comparison between their sample and the present one might not be completely possible, their data suggest that participation in story sessions without discussions does not improve theory of mind understanding. Future research is necessary to address this issue and to disentangle the specific aspects of training that led to improved performance; nevertheless, the present data suggest that participation in narrative discourse can be an effective tool for learning theory of mind concepts.

There are two other limitations with respect to the generalizability of the

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findings. Consistent with previous research in this area (e.g., Appleton & Reddy, 1996; Slaughter, 1998; Slaughter & Gopnik, 1996), an inclusion criterion was used in the second study: Children who earned 3 or fewer points on the pretest completed the study. One possibility is that an intervention incorporating narrative discourse is effective only with children who perform poorly on false belief and deception tasks. It is plausible, however, that once children develop a foundational understanding of theory of mind concepts (e.g., false belief, deception), narrative discourse is the means through which they continue to develop a more advanced understanding of others' thoughts and feelings, including such concepts as forgetting and remembering that are acquired later (Pillow & Lovett, 1998). This is an interesting question for future research.

The findings of the present investigation also might suggest that an intervention like the one we used is effective only in the short-term. The intervention was effective in the second study when the time between the completion of the intervention and the first posttest was 1 week, rather than 2 weeks, as in the first study. Also, although the mean of the intervention group remained higher, the difference between the performances of the intervention and control groups was not significant at the second posttest. Such results suggest that the effectiveness of the present intervention does not endure. It is possible that such interventions must continue for them to have lasting effects. Future research needs to address this question.

This investigation adds to current knowledge of whether theory of mind concepts can be trained. Approaches that focus on specific task information, as well as those that emphasize the concepts during discussion, lead to at least modest improvements in children's theory of mind development. With future research, developmental psychologists can continue to identify the specific means through which children acquire understanding of mental concepts.

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APPENDIX

Sample Script for the Training Sessions

Story: Meddaugh, S. (1995). *Hog-Eye.* Boston: Houghton Mifflin

Topic: Trickery

Starting Question: What is a trick? (You make someone think something that isn't true.) Has someone tricked you? What did they do? Sometimes we trick people just to be funny. Now we are going to hear a story about a pig that tricked a wolf so that he wouldn't eat her. Hog-eye tricked the wolf to save her life.

Page 6: What was Hog-eye's wish? Why did she think it came true? (No one else was on the bus.) Did her wish come true? (No, she just thought it did; she was really on the wrong bus.)

Page 16: Could the wolf read? (No.) Why did the wolf say he could read? (He didn't want Hog-eye to know he could not read. So he tried to trick her into thinking that he could read.)

Page 17: But they were really reading a book about cars. So did the book tell them they needed carrots to make the soup? (No, she tricked him. She just wanted him to go away so that she could get away.)

Page 20: Were there really onions and green peppers at the bottom of Devil's Cliff? (No, she tricked the wolf. She just wanted him to think that he needed onions and green pep-

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pers from there.)

Page 21: Why did she tell the wolf she needed water from Torrential Falls? (So she had time to get away, and she hoped the wolf would get hurt.)

Page 30: Did Hog-eye really put a spell on the wolf? (No.) Really, why did the wolf itch? (He had rolled in poison ivy.) Did he think that Hog-eye put a spell on him? (Yes.) So Hog-eye tricked him.

Conclusion: So Hog-eye tricked the wolf into thinking that she put a spell on him, so he would let her go. She knew that poison ivy would make him itch, but the wolf did not know it was poison ivy. So she made him think she put a spell on him to make him itch. But really it was just the poison ivy that made him itch.

Activity: Have the children act out an episode of the story. Have children talk about what the characters' were thinking. Make sure each child has a turn as either Hog-eye or the wolf.

Props: wolf nose, pig nose

Note. The activity and props were used only in Study 1.

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