I know what you need to know: Children's developing theory of mind and pedagogical evidence selection

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Abstract

Natural pedagogy emerges early in development (Knudsen & Liszkowski, 2012), but *good* teaching requires presenting evidence specific to learners' knowledge (Shafto, Goodman, & Griffiths, 2014). How might the development of Theory of Mind (ToM) relate to the ability to select pedagogical evidence? We present a training study in which we investigated the link between preschool-aged children's false-belief understanding and their ability to select evidence for teaching. Our results suggest that children with more advanced ToM abilities were better evidence selectors, even when controlling for effects of age and numerical conservation abilities. We also found that children who improved more in false-belief understanding from pre- to post-test performed better on the pedagogical tasks over the course of the training. Finally, we report tentative evidence for a link between the pedagogical training and improvements in ToM. Our findings suggest important connections between ToM and evidential reasoning in natural pedagogy in early childhood.

Keywords: pedagogy, theory of mind, evidence selection

Introduction

The ability to teach and be taught by others is an indispensable human capability. Social transmission of information is one of the key ways in which both children and adults learn about the world, and some have argued that the natural tendency to teach and to be ready to learn from others may be what sets human intelligence apart from other animals (e.g., Moll & Tomasello, 2007). Indeed, teaching in children emerges at an early age: Three-year-olds spontaneously engage in teaching behavior with their peers (Ashley & Tomasello, 1998), and infants as young as 12 months selectively point to convey information to naïve (as opposed to knowledgeable) adults (Knudsen & Liszkowski, 2012). Investigating children's developing ability to teach others may shed insight into the cognitive mechanisms that support natural pedagogy. We will suggest that the factors that support this skill – reasoning about the knowledge states of others and reasoning about evidence – are intertwined.

Teaching in Early Childhood and Theory of Mind

Children's teaching abilities improve considerably between the ages of three and five years. Davis-Unger and Carlson (2008) had three- to five-year-old children teach a confederate how to play a novel board game, and found that older children 1) taught for longer periods of time, 2) explained more of the rules, and 3) used a more diverse range of teaching strategies. Similarly, Strauss, Ziv, and Stein (2002) found that five-year-olds taught others by providing verbal explanations, whereas three-year-olds used more demonstration-based teaching strategies. There is also evidence that older children possess more declarative knowledge about pedagogy in general (Ziv & Frye, 2004).

What are the fundamental cognitive underpinnings that support the development of children's pedagogical skills? Theory of Mind (the ability to represent others' mental states and to understand that others may experience mental states that are different from one's own) has been proposed as being critical for children's teaching. Intuitively, a relationship between Theory of Mind (ToM) and children's developing teaching skills makes sense: ToM involves monitoring the mental states of others, and effective teaching requires understanding what your student does and does not know. Additionally, ToM undergoes drastic qualitative change between the ages of three and five, the same period during which children's pedagogical skills are developing. Indeed, there is a wealth of empirical work that provides evidence for a link between ToM development and pedagogical skill (Davis-Unger & Carlson, 2008; Strauss et al., 2002). ToM may thus be an important cognitive mechanism that drives the development of children's ability to teach others.

Evidence Selection in Teaching

The past work on children's teaching and ToM ability has operationalized teaching ability in various ways, including by the length of the teaching interaction, the types of strategies used, and whether children recognize that some individuals need to be taught while others do not. An additional and perhaps more detailed way of conceptualizing pedagogical skill comes from the distinct but related body of literature on concept learning and pedagogical sampling. Research in this field emphasizes the importance of selecting and presenting a learner with specific evidence that will allow them to infer a particular conclusion (e.g., Gweon, Tenenbaum, & Schulz, 2010; Shafto et al., 2014). According to this view, being a "good" teacher requires more than just recognizing whether or not someone needs to be taught, or even that some learners need to be taught more than others; rather, good teaching depends on having a deeper understanding of the precise evidence that certain learners may or may not need in order to infer a particular conclusion.

Prior work has shown that children are sensitive to learning goals in pedagogical scenarios. Six-year-olds will select diverse samples to teach a novel concept to a peer, but not to learn a novel concept for themselves (Rhodes, Gelman, & Brickman, 2010). Preschoolers are even capable of selectively presenting evidence to intentionally deceive learners. Rhodes, Bonawitz, Shafto, Chen, and Caglar (2015) showed three- to six-year-olds a novel toy that activated when any block was placed on it. They then asked children to pick two blocks to either 1) teach a naïve puppet how the toy really worked, or 2) trick her into thinking that only red blocks made it go. Children reliably selected blocks that would best communicate the pedagogical goal, regardless of whether the goal was to teach or to deceive (Rhodes et al., 2015).

There is also an abundance of work demonstrating that when learning from others, children use the evidence presented to them to make inferences about the knowledgeability of their teachers (see Kushnir and Koenig (in press) for a recent example). Pasquini, Corriveau, Koenig, and Harris (2007) showed children videos of adults naming familiar objects with varying rates of accuracy; children were then asked from whom they would prefer to learn the names of novel objects. Three- and four-year-olds preferred to learn from more accurate teachers, suggesting that children use presented evidence in pedagogical scenarios to update their beliefs about whether teachers are knowledgeable or not. Despite this robust preference for accurate teachers, there has also been work showing that children are able to exonerate previously inaccurate teachers whose past inaccuracies occurred for legitimate reasons (Nurmsoo & Robinson, 2009). Children therefore additionally monitor teachers' epistemic states in conjunction with the evidence they've presented in order to make inferences about their competence.

Together, the works cited in this section suggest that children are developing the ability to reason about evidence in the service of teaching in the early childhood years. However, we are unaware of any work that has investigated the precise relationship between ToM development and children's ability to effectively select pedagogical evidence to teach others. ToM may play an especially important role in supporting this aspect of teaching, because effective evidence selection requires the on-line monitoring of a learner's epistemic state relative to a particular learning goal. The current paper presents a novel experiment that explores the relationship between children's pedagogical evidence selection and ToM development.

Teaching Training and Theory of Mind Study

We investigated the relationship between children's Theory of Mind ability (as measured by a false-belief battery; Wimmer & Perner, 1983; Gopnik & Astington, 1988) and their ability to select evidence to teach another. Assuming this link, we predicted that children with more proficient Theories of Mind would be better at pedagogical evidence selection, and also that training pedagogical skill might lead to improvements in ToM reasoning abilities. To explore this, we assessed children's false-belief understanding before and after training them on two pedagogical tasks. We also assessed children's understanding of numerical conservation; we wanted to be sure that any improvements we saw due to the pedagogical training was specific to ToM abilities and not to other unrelated domains of cognitive development.

We chose to use false-belief tasks to measure ToM; between the ages of three and five, children reliably transition from predicting others' actions based on the veridical state of the world to understanding that others' actions are in fact guided by their (sometimes false) beliefs (Wellman, Cross, & Watson, 2001). Some have argued that implicit false-belief understanding emerges at much earlier ages (between 10 and 15 months), and that apparent developments in ToM between the ages of 3 and 5 years are actually reflections of task demands (Baillargeon, Scott, & He, 2010). Nevertheless, there is ample evidence that the changes that occur in children's ToM understanding during the preschool years are critical: This is the time during which children gain the ability to provide explicit causal explanations for others' actions based on epistemic states (e.g., Bartsch & Wellman, 1989); further, differences in preschoolers' false-belief understanding are predictive of numerous other capabilities, including children's tendency to talk about people in everyday conversation, and their social competence more broadly (see Astington & Jenkins, 1995; Imuta, Henry, Slaughter, Selcuk, & Ruffman, 2016), suggesting an important link between performance on these tasks in early childhood and real cognitive development. We therefore used false-belief tasks to measure ToM abilities.

Methods

Participants

Sixty-one children ($M_{\text{age}} = 47$ months, range $= 39 - 55$ months) were recruited from and tested at local preschools.

Tasks

False-Belief Children's ToM was assessed using two classic false-belief tasks. In the first task, children saw a storybook in which Sally put her cookie in a box and then left the room. While Sally was gone, Anne came in and moved Sally's cookie from the box to a bag. Children were then asked, "When Sally comes back, where will she look for her cookie?" Children earned a point if they correctly reported that Sally would look for her cookie in the box. In the second task, children were shown a crayon box that, it was soon revealed, actually contained some keys. Experimenters asked the children 1) what they thought was inside the box when they first saw it, and 2) what was really inside. Children earned one point if they correctly answered both of these questions. The experimenter then introduced a doll, and asked children the same two questions ("What will the doll think is inside this box? What's really inside?"). Children again received one point for correctly answering both questions. False-belief scores could thus range from zero to three.

Numerical Conservation Control Task To assess children's understanding of numerical conservation, experimenters showed children two parallel rows of ten objects each, both of which were equal in length. Children were asked if row A or B had more objects, or if they were the same. Then, experimenters lengthened one of the rows, and again asked children if row A or B had more objects, or if they were the same. This process constituted one trial; children had to answer both questions correctly on a given trial to earn one point. Experimenters administered two trials; conservation scores could thus range from zero to two.

Pedagogical Training and Test The pedagogical training entailed a novel word learning task and a causal toy activation task. In the novel word learning task, children were told that a novel word (e.g., "Dax") represented the concept they were trying to learn. They were shown a picture of an object with two discrete features (e.g., a *fork* that is *white*), and were told that this picture represented the target concept ("This is a Dax!"). Given the inherent ambiguity in the word's extension, the experimenter explained what the novel word really meant ("Dax means fork."). The experimenter then presented two additional pictures, each of which contained an item that overlapped with exactly one of the original picture's two features (e.g., a white spoon, and a black fork). The experimenter then asked children to teach a confederate what the novel word meant by providing examples using the three pictures, without explicitly telling the confederate what the novel word meant. In order to provide a correct response, children had to present the necessary and sufficient examples to identify the correct rule while ruling out other hypotheses[1].

In the causal toy activation task, children were presented with a novel toy with two distinct mechanisms (e.g., a wheel and a bell). The experimenter first showed children how to activate the toy, causing it to perform some desirable outcome such as lighting up or playing music ("You need to ring the bell and spin the wheel at the same time to make the toy go."). As in the novel word task, children were then instructed to teach a confederate about the toy by providing examples of which combinations of mechanisms did and did not make the toy go. In order to provide a correct response, children had to demonstrate both necessary and sufficient evidence for the confederate to rule out all alternative explanations and correctly infer which mechanism(s) activated the toy.

For both tasks, if children provided insufficient evidence, the confederate prompted the child by musing aloud about the remaining possible explanations. For example, if the child only showed the confederate that operating both mechanisms simultaneously made the toy go, the confederate might say: "Oh, so you showed me both at the same time. It could be that you need to do both at the same time to make it go, or it could be that the wheel by itself could make it go, or that the bell by itself could make it go. Can you teach me?" Note that often children would need to present negative examples to rule out plausible hypotheses (e.g., showing that the wheel by itself did not make it go). The number of prompts children required before providing complete evidence was the primary DV for both pedagogical training tasks; these scores could range from a minimum of zero (i.e., children who provided necessary and sufficient evidence spontaneously) to a maximum of two (i.e., children who required prompting after each demonstration until all evidence had been provided).

There were six different versions of each task: The novel words were *fep, dax, modi, toma, wug,* and *blicket*; the causal toys were *phone, gear toy, helicopter, shadowbox, red airplane,* and *purple*. Some of the novel words represented just one of the two categories to which the example object belonged ("Dax means fork"), while others represented both categories ("Dax means white fork"). Likewise, some causal toys would activate any time one of the mechanisms was operated ("Any time you ring the bell, it makes the toy go"), while others would only activate if both mechanisms were operated simultaneously ("You need to ring the bell and spin the wheel at the same time to make the toy go"). Varying the stimuli in this way ensured that children would have distinct teaching goals on different trials, and would thus have to select evidence that corresponded to the particular teaching goal of a given trial in order to provide a correct response.

Procedure

Children's understanding of false-belief and numerical conservation was assessed on a preliminary testing day. Children who scored fewer than two out of three points on the false-belief task were classified as copy theorists (i.e., those who think that beliefs are always consistent with the world), while children who scored two or more points were classified as perspective theorists (i.e., those who understand that beliefs may vary with perspective, and can thus be false; see Goodman et al., 2006). Copy theorist (CT) children ($N = 40$) were randomly assigned to either the control or the training condition. Over the course of the following six weeks (beginning on the preliminary testing day), children in the training condition ($N = 22$; $M_{\text{age}} = 46$ months) received two training sessions per week on both pedagogical tasks. One version of each task was administered on a given testing session, with the novel word task always being presented first. As there were six versions of both the novel word task and the causal toy task, the experimenter administered the same version of each task across both sessions of a given week. The order in which the different versions of the tasks were presented was randomized across participants. At the end of this six week period, children's understanding of false-belief and numerical conservation were reassessed using the same measures with slightly different stimuli.

CT children in the control condition ($N = 18$; $M_{\text{age}} = 46$

[1] To help children understand the hypotheses under consideration, the confederate announced the full set of hypotheses before the child began teaching (e.g., "I see, Dax could mean fork, or Dax could mean white, or it could mean white fork.").

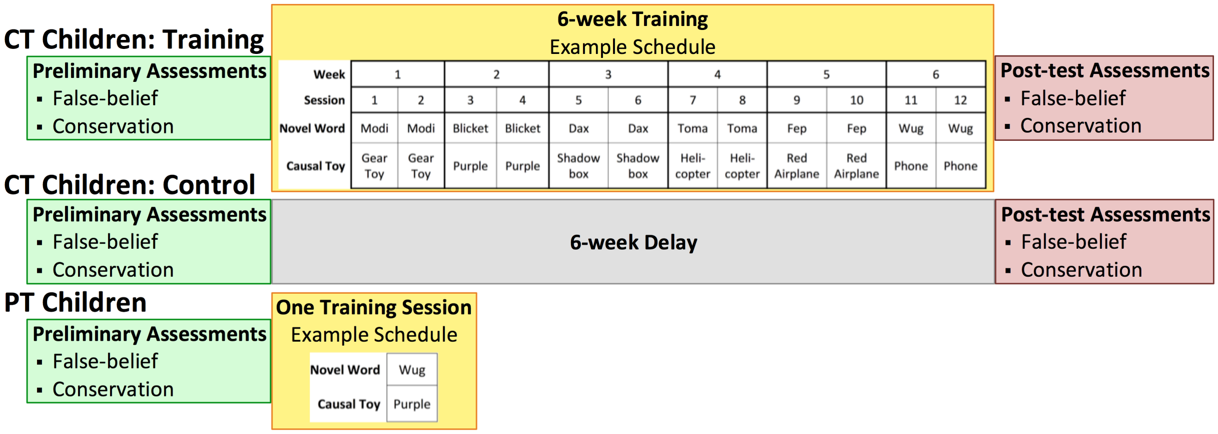

Figure 1: A schematic of our study design, with examples of possible pedagogical training schedules for CT children in the training condition and PT children.

months) received no pedagogical training, and their false-belief and conservation understanding was reassessed after a six-week delay. Perspective theorist (PT) children ($N = 21$; $M_{age} = 49$ months) did not receive longitudinal pedagogical training, since they had little to no room for improvement on the false-belief tasks; instead, they received just one session of the pedagogical tasks on the preliminary testing day, allowing us to measure their initial teaching abilities. The versions of the pedagogical tasks used with PT children were randomized across participants. PT children's false-belief and conservation understanding was not reassessed. See Figure 1 for a schematic of our study design.

Results

One CT child in the training condition did not complete one session of the causal toy task, another CT child did not complete one session of both tasks, and one PT child's numerical conservation abilities were not assessed; these individual data points were treated as missing in subsequent analyses. Otherwise, all children completed all training sessions and assessments. We created a composite pedagogical skill score for each training session by calculating the average number of prompts children required across both tasks in a given session. Lower scores indicated better task performance.

Initial False-Belief & Pedagogical Skill

We first investigated the effects of preliminary false-belief understanding on initial (i.e., non-trained) pedagogical skill. An independent-samples t-test compared CT children in the training condition to PT children on the average number of prompts required on the preliminary testing day. We found that PT children ($M = 1.05$, $SD = .57$) provided complete evidence with significantly fewer prompts than CT children ($M = 1.45$, $SD = .55$), $t(41) = 2.38$, $p = .022$, 95% CI_{diff} = [.06, .75]; see Figure 2A. We also looked at the novel word and causal toy tasks separately: While PT children ($M = .57$, $SD = .68$) significantly outperformed CT children ($M = 1.27$, $SD = .78$) on the causal toy task ($t(41) = 3.17$, $p = .003$, 95% CI_{diff} = [.26, 1.15]), there were no significant differences between the two groups on the novel word task (CT: $M = 1.64$; PT: $M = 1.52$; $p = .584$, 95% CI_{diff} = [-.30, .53]). This disparity may be explained by the seemingly increased difficulty of the novel word task relative to the causal toy task. Indeed, two paired-samples t-tests revealed that both CT and PT children performed better on the causal toy task than on the novel word task on the preliminary testing day (CT: $t(21) = 5.05$, $p < .001$, 95% CI_{diff} = [.56, 1.35]; PT: $t(20) = 2.16$, $p = .042$, 95% CI_{diff} = [.01, .71]); additionally, more children required the maximum of two prompts on the novel word task ($N = 29$) than on the causal toy task ($N = 12$). The novel word task might have been more difficult for children than the causal toy task for several reasons; perhaps children are more generally familiar with toys, or have more experience teaching about toys than about words. Future work could explore the differences between these two tasks.

There are many possible reasons why PT children may have outperformed CT children on the causal toy task, including age or other cognitive factors. To control for this, we ran two between-subjects ANCOVAs, with theorist type (CT vs. PT) predicting performance on the causal toy task; we included preliminary conservation scores as a covariate in one analysis, and age at pre-test in the other. PT children still outperformed CT children on the causal toy task, even when controlling for effects of age ($F(1,40) = 6.11$, $p = .018$) and conservation scores ($F(1,39) = 9.35$, $p = .004$), providing stronger evidence for a direct link between false-belief understanding and teaching ability.

False-Belief Improvement & Pedagogical Skill

Next, we investigated the relationship between overall aggregate performance on the two pedagogical tasks and improvement on the false-belief task from pre- to post-test. Using data from CT children in the training condition, we ran a correlation between false-belief improvement (i.e., pre-test false-belief scores subtracted from post-test scores) and the

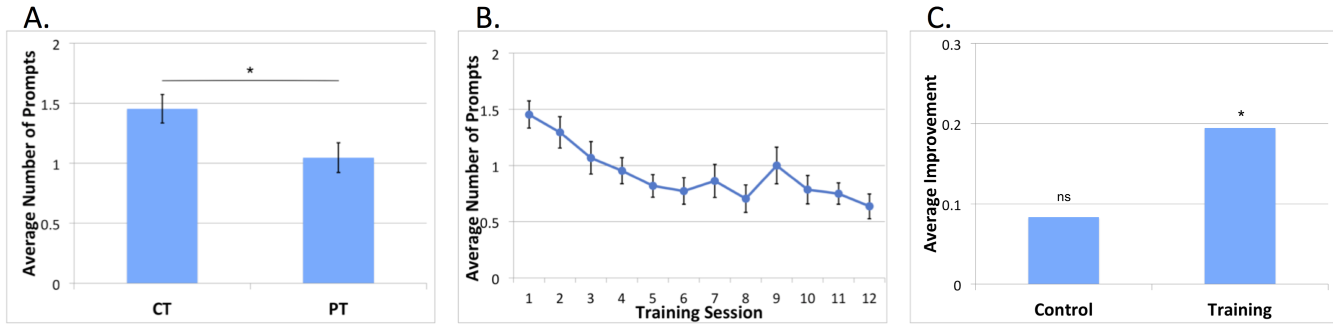

Figure 2: A. PT children provided the confederate with complete evidence after receiving significantly fewer prompts than CT children on the first day of pedagogical testing. B. Children required fewer prompts over the course of the pedagogical training. C. For children who answered all false-belief questions incorrectly at pre-test, those in the training condition significantly improved in false-belief understanding from pre- to post-test, whereas those in the control condition did not. Asterisks denote significance at the $p < .05$ level. All error bars represent two standard errors.

mean number of prompts required across all twelve training sessions. We found a statistically significant negative linear relationship between these two factors, $r(20) = -.43$, $p = .047$. In other words, children who required fewer prompts over the course of the training generally improved more in false-belief understanding from pre- to post-test. Two partial correlations revealed that this finding qualitatively persisted when statistically controlling for average age ($r(19) = -.43$, $p = .054$) and improvement in conservation understanding ($r(19) = -.41$, $p = .063$).

Effect of Training on False-Belief Understanding

Finally, we evaluated the possible effects of pedagogical training on children's false-belief scores. Our first question was whether the training was actually effective in improving children's pedagogical skills. A repeated-measures ANOVA on the mean number of prompts children required on each of the twelve training sessions revealed a significant effect of session ($F(11,220) = 4.96$, $p < .001$), as well as a significant linear trend (i.e., a straight line fit the data at better than chance levels; $F(1,20) = 20.85$, $p < .001$). Children's performance on the pedagogical tasks thus did improve with training (see Figure 2B).

Next, we ran an independent-samples t-test comparing CT children in the training condition ($N = 22$) to those in the control condition ($N = 18$) on false-belief improvement. This direct comparison between training and control participants did not yield significant results ($p = .65$). However, CT children who answered one false-belief question correctly at pre-test had less room for improvement. Indeed, looking only at CT children who answered zero false-belief questions correctly at pre-test, we did observe improved false-belief understanding for children in the training condition ($N = 12$; $M_{\text{improve}} = .19$, $SD = .26$; $t(11) = 2.55, p = .027$), but not for those in the control condition ($N = 8$; $M_{\text{improve}} = .08$; $p = .170$); see Figure 2C. Importantly, conservation scores did not differ for either group between pre- and post-test (Training: $p = .551$; Control: $p = .197$), suggesting that the training targeted ToM without necessarily leading to general improvement in cognitive reasoning. Note that this result does not directly compare training to control children, and should be interpreted with caution. However, coupled with our finding that initial false-belief understanding is related to non-trained pedagogical skill, this may suggest an important link between reasoning about others' minds and pedagogical evidence selection.

Discussion

Past work has shown that children's developing ToM reasoning abilities are related to their pedagogical skill, but has not looked at the precise relationship between ToM development and the ability to select optimal evidence to teach others. Our results suggest that having a more developed ToM is broadly related to being better at evidence selection, even when controlling for age and more general cognitive abilities. Further, we found tentative supporting evidence for the idea that training pedagogical evidence selection may in turn improve children's ToM reasoning abilities. Taken together, our results are consistent with prior work on the relationship between ToM and teaching skills, and provide support for a strong link between pedagogical evidence selection and theory of mind.

Our results speak to existing models of ToM development that postulate genuine conceptual change during the preschool years. Specifically, we found evidence for a link between performance on a false-belief task and the discrete developmental capability of pedagogical evidence selection, suggesting that the changes in false-belief understanding that occur between the ages of 3 and 5 may reflect deeper qualitative changes in children's Theories of Mind. As we noted in the introduction, we recognize that there is a diverse range of perspectives on the course of children's ToM development, and we will not attempt to resolve that debate here. Rather, we simply suggest that our findings cannot be explained in their entirety by false-belief task demands (especially given that our results persist when controlling for effects of age), and may therefore be indicative of some type of conceptual change in ToM during the preschool years.

Our findings also have implications for current theories and models of natural pedagogy and epistemic trust. Shafto et al. (2014) propose a Bayesian model of pedagogical teaching and learning, according to which the evidence that teachers choose to present directly depends on the learner's prior knowledge and the learning goal that the teacher is trying to communicate. This pedagogical model is a special case of the broader model of epistemic trust (Shafto, Eaves, Navarro, & Perfors, 2012; Eaves & Shafto, 2012, in press), which explicitly connects developmental changes in reasoning about others' beliefs to interpretation of evidence selection by others. Our results support these models that link evidence selection and reasoning about other minds. We also extend their findings, showing that this link 1) exists even in young children who have not yet been exposed to formal schooling, and 2) is manifest in their selection of evidence for others.

Along with this prior work, our paper also speaks to broader theories of natural pedagogy, and supports a potential link between the uniquely human ability to teach others and the development of the ability to reason about others' minds; this raises questions about whether these skills may even be evolutionarily intertwined. Whatever the case may be, reasoning about other minds, as conceptualized in the field, is composed of multiple interrelated inference problems. Understanding the role of these social inferences in learning requires investigating how children approach several conjoined problems, as we have done. Our work exemplifies and shows the value of that approach.

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