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Perceptual Access Reasoning (PAR) in Developing a Representational Theory of Mind

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Abstract An important part of children's social and cognitive development is their understanding that people are psychological beings with internal, mental states including desire, intention, perception, and belief. A full understanding of people as psychological beings requires a *representational theory of mind* (ToM), which is an understanding that mental states can faithfully represent reality, or misrepresent reality. For the last 35 years, researchers have relied on false-belief tasks as the gold standard to test children's understanding that beliefs can misrepresent reality. In false-belief tasks, children are asked to reason about the behavior of agents who have false beliefs about situations. Although a large body of evidence indicates that most children pass false-belief tasks by the end of the preschool years, the evidence we present in this monograph suggests that most children do not understand false beliefs or, surprisingly, even true beliefs until middle childhood. We argue that young children pass false-belief tasks without understanding false beliefs by using *perceptual access reasoning* (PAR). With PAR, children understand that seeing leads to knowing in the moment, but not that knowing also arises from thinking or persists as memory and belief after the situation changes. By the same token, PAR leads children to fail true-belief tasks.

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PAR theory can account for performance on other traditional tests of representational ToM and related tasks, and can account for the factors that have been found to correlate with or affect both true- and false-belief performance. The theory provides a new laboratory measure which we label the *belief understanding scale* (BUS). This scale can distinguish between a child who is operating with PAR versus a child who is understanding beliefs. This scale provides a method needed to allow the study of the development of representational ToM.

In this monograph, we report the outcome of the tests that we have conducted of predictions generated by PAR theory. The findings demonstrated signature PAR limitations in reasoning about the mind during the ages when children are hypothesized to be using PAR. In Chapter II, secondary analyses of the published true-belief literature revealed that children failed several types of true-belief tasks.

Chapters III through IX describe new empirical data collected across multiple studies between 2003 and 2014 from 580 children aged 4–7 years, as well as from a small sample of 14 adults. Participants were recruited from the Phoenix, Arizona metropolitan area. All participants were native English-speakers. Children were recruited from university-sponsored and community preschools and daycare centers, and from hospital maternity wards. Adults were university students who participated to partially fulfill course requirements for research participation. Sociometric data were collected only in Chapter IX, and are fully reported there.

In Chapter III, minor alterations in task procedures produced wide variations in children's performance in 3-option false-belief tasks. In Chapter IV, we report findings which show that the developmental lag between children's understanding ignorance and understanding false belief is longer than the lag reported in previous studies. In Chapter V, children did not distinguish between agents who have false beliefs versus agents who have no beliefs. In Chapter VI, findings showed that children found it no easier to reason about true beliefs than to reason about false beliefs. In Chapter VII, when children were asked to justify their correct answers in false-belief tasks, they did not reference agents' false beliefs. Similarly, in Chapter VIII, when children were asked to explain agents' actions in false-belief tasks, they did not reference agents' false beliefs. In Chapter IX, children who were identified as using PAR differed from children who understood beliefs along three dimensions—in levels of social development, inhibitory control, and kindergarten adjustment.

Although the findings need replication and additional studies of alternative interpretations, the collection of results reported in this monograph challenges the prevailing view that representational ToM is in place by the end of the preschool years. Furthermore, the pattern of findings is consistent with the proposal that PAR is the developmental precursor of representational ToM. The current findings also raise questions about claims that infants and toddlers demonstrate ToM-related abilities, and that representational ToM is innate.

I. Perceptual Access Reasoning (PAR)

The study of children's thinking has always included the question of how children come to understand the mind. Understanding the mind poses special challenges to children because words like see, know, remember, and think do not refer to physical objects or actions that adults can point out and name for children. Studying children's understanding of the mind also poses challenges to researchers. When children first talk about seeing, knowing, remembering, and thinking, it is difficult to know what children really understand about those mental activities. In addition, it has taken researchers some time to come to an understanding about what really are the fundamental insights about the mind that children need to acquire. We first briefly review the earlier research tradition on childhood egocentrism that preceded the current theory-of-mind (ToM) approach to studying children's understanding of the mind. Flavell (2004) provides a fuller discussion of links between the two research traditions.

Egocentrism

For a long time, researchers agreed that young children had an egocentric understanding of the mind because young children so often seemed to assume that others saw and knew the same things that they themselves did. If young children had an egocentric understanding of the mind, then the most important thing that children needed to understand was that other people's minds differed from their own. Piaget's (1959; Piaget & Inhelder, 1956) studies revealed many examples of young children's egocentric thinking. A classic example is Piaget's visual perspective-taking task involving a table-top reproduction of three local mountains that cluster together in the center of Geneva. In the lab, children walked around the table to view the model from all sides, and then sat at one side of the table, while another person sat at a different side. Children were shown four photographs, one depicting their visual perspective, and the other three depicting the mountain scene as viewed from each of the other sides of the table. Young children tended to pick the same photograph to indicate what they saw and what the other person saw, suggesting that they assumed that the other had the same visual perspective of the mountain scene as they themselves did, despite viewing it from a different angle.

Similar findings of egocentrism came from studies of perspective-taking regarding knowledge and communication (e.g., Flavell et al., 1968). For example, two children sat on opposite sides of a table, each with an identical pile of blocks of different shapes and colors. A tall screen between them prevented the children from seeing each other's blocks. One child was designated the leader, and assigned the task of building something with the blocks, while at the same time giving the follower directions for how to build the same thing. A typical dialogue might include the following exchange, in which each child has two red blocks, one triangular and one rectangular: Leader, [while picking up the triangular red block] "Now put the red block on top." Follower, [while picking up the rectangular red block] "This one?" Leader, "Yes." In this example, both children appear to assume that the other child knows which red block they meant.

For two decades after Piaget's original studies, researchers attempted to find the age at which children overcome egocentrism (for reviews, see Chandler, 2001; Chandler & Boyes, 1982; Flavell, 1992; Shantz, 1983). The end result was that there was no certain age found when children overcome egocentrism. If visual perspective-taking tasks were pared down to simple judgments of seeing or not seeing, dubbed Level 1 (Flavell, Everett, et al., 1981), or social perspective-taking tasks were pared down to children simply adjusting the level and complexity of their speech to an adult versus a 2-year-old (Shatz & Gelman, 1973), then even 3- to 4-year-olds could appear nonegocentric. At the same time, more complex tasks made older children appear egocentric.

Theory of Mind (ToM)

In trying to determine when children overcome egocentrism, researchers had assumed that children knew their own minds. Two new approaches to studying children's understanding of the mind showed that assumption to be wrong, and that during the preschool years, young children are unaware that their own minds contain representations of things. One new approach was the introduction of appearance-reality tasks (Flavell et al., 1983). In one example, children are shown a fake rock made out of sponge material. When asked what it looks like, all say a rock. But after they touch it and discover that it is really a sponge, they switch and say it looks like a sponge. Once young children discover what the object really is, they can no longer talk about the object's deceptive appearance. The physical object exists in the real world, but the appearance exists in the mind as a mental representation of the physical object. Children's errors on appearance-reality tasks show that young children are out of contact with their own mental representations of how things appear. Thus, egocentrism is not the real problem. Researchers realized that the fundamental insight about the mind that children needed to acquire is not that other people's mental representations of how things appear differ from children's own mental representations. The fundamental insight that young children need to acquire is that people, themselves included, have mental representations that are distinct from reality and that can either faithfully represent reality, or misrepresent reality.

The other new approach was the introduction of false-belief tasks (Wimmer & Perner, 1983). False-belief tasks (described more fully below) also assess the understanding of mental representations, not only in the case of deceptive objects, but also in the more common cases of the contents of familiar containers, and the locations of objects. False-belief tasks are the centerpiece of research on how children develop a ToM. ToM refers to the commonsense view that people have internal, mental states that are the causes of their outward behavior (Fodor, 1978). Desires and beliefs play central roles in ToM. If one knows that a person desires X, and that the person believes that action Y will lead to X, then one can predict that the person will do Y. For example, knowing that Maxi wants his chocolate, and knowing that Maxi believes that his chocolate is in the red cupboard allows one to predict that Maxi will look in the red cupboard for his chocolate. ToM is thought to be closely connected to many uniquely human social abilities to cooperate, compete, communicate, and develop culture. The real power of human ToM stems from the role that belief plays, because beliefs can be either true or false. If one knows that Maxi's chocolate is really in the blue cupboard, and if one knows that Maxi has a false belief that his chocolate is in the red cupboard, then one can make sense of, and even predict Maxi's inevitable search of the wrong cupboard. When a person has a false belief, that person is laboring under a misrepresentation of the real state of affairs; thus, understanding false beliefs allows one to have what is more properly termed a *representational ToM*, or as often referred to in this monograph, *belief reasoning*.

The volume of ToM literature is immense, with active research and debate on many fronts (for overviews see, e.g., Carpendale & Lewis, 2015; Hughes & Devine, 2015; Miller, 2009; Sodian, 2005). This monograph addresses the central question in all of this research: How do children develop a representational ToM? We present herein a new theory about what young children do, and importantly do not, understand about the mind. In presenting the theory, we also discuss research in four related areas, where questions are being asked that were unthought of a generation ago: ToM in human infants and toddlers (Carpendale & Lewis, 2015); ToM in nonhuman primates and other species (Call & Tomasello, 2008; Couchman et al., 2012; Tomasello, 2018); later ToM developments in adolescence (Miller, 2009); and associations between ToM and children's social and academic outcomes (Hughes & Devine, 2015). Areas of potential future applications of the theory would include: ToM and autism spectrum disorder and schizophrenia (Chung et al., 2014; Sprong et al., 2007); ToM and deafness (Hughes & Devine, 2015); and neurological bases of ToM (Carpendale & Lewis, 2015).

The next sections include descriptions of the various false-belief tasks, which are the primary tools that researchers use to study ToM in children, followed by an overview of the current theories about how ToM develops, and the current consensus and controversy about when ToM develops. The remainder of the chapter presents the theory of PAR and describes the overall plan of the monograph.

False-Belief Tasks

In false-belief tasks, children are asked simple questions about someone who has a false belief about something. The questions can be phrased in terms of what the person thinks, or what the person will say or do; the person asked about can be a real person, an imagined person, or a puppet or doll. A meta-analysis of false-belief studies showed that these variations do not change children's answer (Wellman et al., 2001). There are four standard versions of false-belief tasks.

Location False-Belief (Wimmer & Perner, 1983)

The classic example of a location false-belief task is one in which a story is acted out with dolls and two boxes representing kitchen cupboards. The protagonist of the story, Maxi, returns from shopping with his mother and puts his chocolate in a cupboard (A). Maxi then leaves the scene to go outside and play. While he is away, his mother moves the chocolate to cupboard B, and then she leaves as well. Maxi then returns, without having been told by his mother that she moved the chocolate. While Maxi stands between the two cupboards, children are asked where Maxi will look for his chocolate (or alternatively, where Maxi thinks it is), in cupboard A or in cupboard B. If the children understand that Maxi has a false belief about where his chocolate is, they should predict that he will look or think that it is in cupboard A.

Contents False-Belief (Hogrefe et al., 1986)

In the classic example of a contents false-belief task, children are shown a *Smarties* candy box (in the United States, the equivalent would be a bag of M&Ms). They are asked what is inside, to ensure that the box is familiar to them. After they give the expected answer (i.e., Smarties), they are shown that there is actually something else inside instead of candies (e.g., a pencil). The pencil is put back in, and they are asked what a newcomer, who has not looked inside the box, will think is in the box, Smarties or a pencil. If they understand that the newcomer will have a false belief about the contents based on the familiar label, they should say that the newcomer will think that Smarties are inside.

Identity False-Belief (Gopnik & Astington, 1988; Modeled After Flavell et al., 1983)

Children are shown a realistic-looking deceptive object; for example, a fake rock made out of sponge. They are asked what it is to ensure that the deception is effective. After they give the expected answer (a rock), they are allowed to touch it and discover its real, spongy identity. The fake rock is placed back on the table, and they are asked what a newcomer, who has not touched it, will think it is, a rock or a sponge. If they understand that the newcomer will have a false belief about its real identity based on its misleading appearance, they should say that the newcomer will think that it is a rock.

Representational Change (Gopnik & Astington, 1988)

Children are first led to have a false belief about something themselves, then after they are shown the truth, they are asked to recall their false belief from a moment ago. The procedure for the representational change tasks is similar to the procedure used in the contents and identity false-belief tasks. Children are shown the Smarties box or the fake rock, and are asked what they think is inside, or what they think it is. After they give the expected answer, they are shown the real contents (pencil) or real identity (sponge). The test question, however, is about their own earlier false belief. In the contents representational change task, they are asked, “When I first showed you the box, before I opened it, what did you think was inside? Smarties or a pencil?” In the identity representational change task, they are asked, “When I first showed you this, before you touched it, what did you think it was? A rock or a sponge?” If children understand that they had a false belief, they should report that they thought that the box contained Smarties, and that the object was a rock.

Young children fail these seemingly simple tasks. They do not attribute false beliefs to the agents in question, nor to themselves. Instead, young children predict that Maxi will look in cupboard B, where the chocolate really is, rather than where he last saw it. They also predict that someone who sees the familiar Smarties box will say that a pencil is inside, and that someone who sees the fake rock will say that it is a sponge. Young children say that they themselves thought that a pencil was in the box, and that the object was a sponge, despite having thought the opposite in both cases a few moments before. Their answers are amusingly child-like, but they are not trying to be amusing. Their answers bespeak a blindness to the mental representations in each case, as if they take the questions to be about reality (“Where is Maxi’s chocolate?” “What is in the box?” “What is this?”). We will refer to the reasoning that young children use when they fail false-belief tasks as *reality reasoning*.

Current Theories About How ToM Develops

There are three traditional theoretical approaches to explaining how children develop an understanding of ToM, and why children’s responses in false-belief tasks change from reality reasoning to belief reasoning during the preschool years.

Simulation Theory

Classical simulation theory (Goldman, 1992; Harris, 1992) holds that the development of ToM is based on children's awareness of their own mental states. One's own mental states are often vivid and unmistakable, and simulation theorists argue that it is reasonable to assume that young children could be aware, for example, that they are currently seeing a toy in a store window, and that they are currently desiring the toy. Seeing and wanting can feel different if one pays close enough attention to inner experiences while seeing something and while wanting something. Classical simulation theorists hold that young children do pay attention to, or introspect, several different basic mental states, and that they subsequently learn how and when to attribute those mental states to others. They learn by imagining, or simulating, themselves in another person's situation, and then attributing the mental state that they themselves experience during the simulation to the other person. On this view, the development of ToM reflects the development of better perspective-taking skills, and better perspective-taking skills account for the change from reality reasoning to belief reasoning.

Contemporary embodied simulation theory (e.g., Gallese et al., 2004) posits a mirror neuron mechanism by which children could automatically experience another person's mental state while observing the other person's behavior in the situation, without having to rely on effortful introspection or simulation. Embodied simulation theory is still in the early stages of testing and development.

Theory-Theory

Classical theory-theory (e.g., Gopnik & Wellman, 1992) holds that the development of ToM is based on children's construction of theories, or concepts of mental states. Theory-theorists argue that children learn about the mind like they learn about things in the physical world. Children learn about animals, for example, by constructing concepts of different kinds of animals based on the features that distinguish one kind from another. Similarly, theory-theorists hold that children notice the different types of behaviors that people engage in when people are said to be having one mental state versus another. Children build concepts of different kinds of mental states based on the behavioral signals that distinguish one kind from another. Theory-theorists argue that introspection alone is not enough to learn about different mental states (Gopnik, 1993). The argument is that it would be too hard for young children to pay attention to inner experiences of subtle differences among multiple, fleeting mental states that occur simultaneously during on-going behavior. Theory-theory holds that, instead, children pay attention to external behaviors, learn which behaviors signal which mental states, and thereby construct concepts of mental states that apply equally to self and to other. Children first construct concepts of perceptions, desires, and emotions. With only those concepts, children cannot understand beliefs, and so they must resort to reality reasoning in false-belief tasks. On this view, the development of ToM reflects the development of new concepts, and constructing new concepts of knowledge and belief accounts for the change from reality reasoning to belief reasoning.

Contemporary theory-theory (Gopnik & Wellman, 2012; Wellman, 2014) acknowledges a role for introspection, but only after children have acquired concepts of mental states. In this view, behavioral-rule concepts would provide the focus needed for introspecting later on, by directing children's introspective attention to phenomenal experiences while engaged in the behaviors specified by the rule associated with that mental state. Only then would children discover the phenomenal qualities that distinguish one kind of mental state from another. Introspection would play only a secondary role in ToM development.

Modularity Theory

Classical modularity theory (Baron-Cohen, 1995; Leslie, 1987, 1994) holds that the development of ToM is based on an innate ability to automatically interpret people's behavior as reflecting people's mental states. Modularity theorists argue that a specialized neurological system, or module, has evolved in humans to identify and keep track of other's beliefs in order to understand and predict behavior during on-going social interactions. Classical modularity theory also posits the existence of three other specialized modules, for detecting intentions, identifying direction of eye gaze, and sharing attention with others. On this view, the development of ToM reflects both the maturational sequence in which modules come online, as well as the development of general learning and information processing skills that allow children to explicitly reason about the mental states that are the outputs of ToM modules. The module for tracking beliefs is held to come online by the age of 2 years, and immature information processing skills during the preschool years are proposed to account for the change from reality reasoning to belief reasoning.

Contemporary modularity theory (Baillargeon et al., 2010; Carruthers, 2013; Luo & Baillargeon, 2010) holds that the module for tracking beliefs comes online before the age of 1 year. By using novel, nonverbal false-belief tasks adapted for infants, researchers have found a substantial amount of evidence that they argue is in support of an innate human ability to predict the actions of agents who have false beliefs.

Current Consensus and New Controversy About When Representational ToM Develops

With the publication of a meta-analysis of the false-belief literature (Wellman et al., 2001), a consensus formed among most researchers that representational ToM is acquired during the preschool years. The meta-analysis findings revealed that on average in five Western and two Asian industrialized countries, about 50% of children aged 3 years, 8 months passed false-belief tasks. The percentage who passed increased steadily thereafter, to 75% at age 5, and 90% at age 6½. Importantly, the meta-analysis included tests of the effects of several different ways that researchers over the years had modified the standard false-belief tasks in attempts to determine if children below age 3½ years could pass false-belief tasks. The findings revealed that none of the ways of modifying the standard tasks had succeeded in reliably raising young 3-year-olds' false-belief performance up to the level of success that 4-year-olds spontaneously achieved. The persistent failure of young 3-year-olds was seen by most researchers as fatal evidence against modularity theory and the existence of a module for tracking beliefs. The meta-analysis authors' conclusion aptly expresses the majority consensus that still prevails today that "understanding of belief, and, relatedly, understanding of mind, exhibit genuine conceptual change in the preschool years" (Wellman et al., 2001, p. 655).

Four years after the publication of the meta-analysis, however, new evidence appeared in favor of modularity theory, with the publication of findings that 15-month-olds passed novel, nonverbal tasks that mirrored the structure of the verbal false-belief tasks (Onishi & Baillargeon, 2005). As noted above, a substantial amount of evidence from nonverbal false-belief tasks has since accumulated in support of the original findings. The nonverbal tasks are designed to test for implicit understanding of false beliefs, as opposed to explicit, or verbal understanding, which is tested in the standard false-belief tasks. The nonverbal tasks measure infants' looking-time and toddlers' helping behaviors. For example, in looking-time studies, infants might be shown a sequence of events similar to the location false-belief task in which an agent puts a toy in box A and then leaves the scene, and in the agent's absence a different agent arrives, moves the toy to box B, and also leaves. The first agent returns, now with a false belief that the toy is still in box A. Infants know the toy is now in B, but they apparently expect the agent to reach for box A, in accord with the agent's false belief about where it is. The evidence of infants' expectation that the agent will reach for box A is that when the agent reaches toward box B instead (pausing for a few moments so that researchers can measure how long the infant keeps looking at the scene), infants keep looking longer than when the agent reaches toward box A. Presumably, the agent's reach toward B violated infants' expectation, and the unexpectedness of the reach toward B captured their attention for a longer time afterwards.

The issues of whether infants and toddlers have some implicit understanding of false beliefs, and what the nature of that understanding might be, are currently unsettled and the subject of much debate. We refer readers to the special issue of *Cognitive Development* (Sabbagh & Paulus, 2018) devoted to replication studies of implicit false belief in infants and toddlers.

The PAR Challenge

In this monograph, we challenge both the current consensus that representational ToM is largely in place by the age of 5 years, as well as the new evidence in favor of implicit understanding of false beliefs in infants and toddlers. We argue that while there is genuine conceptual change in children's understanding of mind in the preschool years, it is not the development of an understanding of false belief during those short, early years. Rather, we argue that the change that has been detected by false-belief tasks appears to be attributed instead to the development of an intermediate, nonrepresentational ToM. In the studies reported in the current monograph, we observe that most 4- to 5-year-olds are reasoning not about beliefs, but instead about whether someone currently sees or touches something and knows it, and whether the person will act correctly in that situation. We have called this nonrepresentational ToM PAR (Fabricius & Imbens-Bailey, 2000). In the remainder of this chapter, we present the theory of PAR and logical arguments in favor of it. In Chapters II through IX, we present empirical tests of predictions of the theory in true- and false-belief tasks, endeavoring to test as many different predicted consequences of the theory as possible. In Chapter X, we discuss the implications of the findings and suggestions for future research.

To foreshadow what follows, the evidence that we present in this monograph suggests that representational ToM is not acquired until middle childhood. Shifting representational ToM to middle childhood would make for a longer developmental history of representational ToM. A longer history would mean that there is more evidence to be found during the preschool years about how representational ToM develops. Shifting representational ToM further away from infancy and toddlerhood would have profound effects on how we understand the ToM-related abilities in those early periods. Located in middle childhood, it would connect with other developments at that age, giving a broader and more integrated understanding of its nature. Understanding the nature of representational ToM in middle childhood would provide a platform for understanding the new concepts that adolescents might still need to construct.

The Theory of PAR

The central tenet of the theory is that PAR is the developmental precursor of representational ToM. In PAR, children are constrained to reasoning about what someone has perceptual access to in a situation, and to understanding the person's behavior only on that basis. When we say that children are using PAR, we mean that they are using both of the following two rules:

Rule 1: Perceptual access leads to knowing, and lack of perceptual access leads to not knowing.
Rule 2: Knowing leads to acting correctly, and not knowing leads to acting incorrectly.

Rule 1 describes how PAR-users understand the connection between perceptual access and knowing. Young children understand that all five sense modalities give people perceptual access to things (Flavell, Flavell, et al., 1990; Flavell, Green, et al., 1990; O'Neill et al., 1992). We focus on seeing, because in the standard false-belief tasks, the false beliefs come from what the agents see rather than what they hear or feel. The PAR-understanding of seeing is the well-known Level-1 visual perspective-taking conception of seeing, in which children understand that a person does or does not see something depending on whether the person's line-of-sight is obstructed or not (Flavell, Flavell, et al., 1981).

The PAR-understanding of knowing is a nonrepresentational conception of knowing as tied to perceptual access. Consequently, PAR-users do not understand that someone can draw inferences or conclusions that go beyond what the person directly perceives. To understand inferences and conclusions requires an understanding that, like the visual appearances of deceptive objects, they exist in the mind as mental representations, and are derived from thinking about what is perceived. In addition, in PAR there is no attribution of memory to the knower once the situation changes and perceptual contact with the situation is lost. It might seem strange, at first, that children who understand that perceiving causes knowing should also think that knowing ceases when perceiving ceases. But to understand that knowing persists requires an understanding that knowledge is stored as a mental representation.

Rule 2 describes an additional misconception that PAR-users have about knowing; namely, that not knowing is tied to acting incorrectly, rather than to guessing. Young children fail to distinguish between knowing and guessing (Perner, 1991). To understand that someone who does not know where a toy is will guess between two boxes, requires understanding that the person imagines, or has a mental representation of the toy being in one box and also being in the other box. Without being able to understand mental representations, PAR-users cannot understand that someone could consider two unseen states of affairs to be equally likely. PAR-users have a nonrepresentational conception of not knowing, or ignorance, and thus cannot understand that someone who does not see where the toy is, and does not know where it is, would guess randomly. PAR-users can only reason that knowing means getting it right, and if someone does not know, then that person must get it wrong.

In tandem, the two rules allow PAR-users to pass false-belief tasks without attributing false beliefs to the agents in question. The two rules reveal the intrinsic confound between belief reasoning and PAR in all of the standard false-belief tasks.

The Confound in False-Belief Tasks Between PAR and Belief Reasoning

In any 2-option task, including all of the standard false-belief tasks, there are two potential reasons why a respondent would select the correct option A.

One possible reason is that the respondent may know that option A is the right answer. A second reason is that the respondent may know that option B is the wrong answer, and—forced to select a response—the respondent chooses the only other option available, thereby selecting A by default (e.g., Sophian & Wellman, 1983). PAR-users know that the reality-reasoning answer is the wrong answer, and—forced to select a response—choose the belief-reasoning answer by default. Below we describe how PAR works in each of the false-belief tasks that we described earlier (*False-Belief Tasks*).

Location False-Belief

Maxi returns after his mother has, unbeknownst to him, moved his chocolate from cupboard A, where he put it, to cupboard B. When asked where Maxi will look for the chocolate, PAR-users reason only about the current situation, now that Maxi has returned. They do not reason about the prior situation, in which Maxi had put the chocolate in cupboard A, because they do not attribute any memory to Maxi about where the chocolate is after he leaves and loses perceptual contact with the prior situation. They reason instead that in the current situation, Maxi does not see the chocolate in B, he does not know it is in B, and so he will not look in B, but will search incorrectly and fail to get the chocolate. The false belief location (cupboard A) is the only incorrect (i.e., empty) location for Maxi to search. Thus, PAR-users would choose A by default, thereby passing the false-belief task without actually attributing a false belief to Maxi that his chocolate is in A.

Contents False-Belief

When asked what a newcomer will think is in the Smarties box, PAR-users reason only about the contents of the box. They do not reason about the appearance of the box (i.e., the label and pictures on the box), because they do not understand that the newcomer could draw any inference from the appearance about what is inside. They reason instead that the newcomer cannot see the pencil inside, does not know that the box contains a pencil, and so the newcomer will not give the correct answer (that a pencil is inside), but will give an incorrect answer. The false-belief answer (that Smarties are inside) is the only incorrect answer choice. Thus, PAR-users would answer by default that the newcomer would think Smarties are inside, thereby passing the false-belief task without actually attributing a false belief to the newcomer that the box contains Smarties.

Identity False-Belief

When asked what a newcomer will think the fake rock is, PAR-users reason only about the real identity of the object. They do not reason about the rock-like appearance of the object, because they do not understand that the newcomer could draw any inference from the appearance about what the object really is. They reason instead that the newcomer does not touch the object, does not know that it is a sponge, and so the newcomer will not give the correct answer (that it is a sponge), but will give an incorrect answer. The false-belief answer (that it is a rock) is the only incorrect answer choice. Thus, PAR-users would answer by default that the newcomer would think that it is a rock, thereby passing the false-belief task without actually attributing a false belief to the newcomer that the object is a rock.

Representational Change

As described earlier, the representational change task begins like the contents and identity tasks, and all children easily give the expected answers when first asked what they think is in the Smarties box or what they think the fake rock is. After they discover the actual contents and real identity, the box is closed back up and the fake rock is put back on the table out of reach. Children are reminded that when they were first shown the box or the fake rock, they did not see the pencil inside and did not touch the object. The representational change test question immediately follows: Children are asked what they first thought was in the box, or what they first thought the object was. PAR-users reason about themselves like they reason about others. They reason that they did not see the pencil or touch the object, that they did not know that a pencil was inside or that the object was a sponge, and so they reason that they must not have given the correct answers (that a pencil is inside; that it is a sponge). Thus, PAR-users would answer by default that they thought Smarties were inside and that they thought the object was a rock, thereby passing the representational change tasks without actually attributing a past false belief to themselves.

The confound in false-belief tasks between PAR and belief reasoning means that PAR may have been hiding in plain view all these years.

Development of PAR

PAR would give children a deeper understanding of the social world than reality reasoning, albeit one that does not include an understanding of mental representations. PAR could provide the foundation for a certain level of cooperation skills via the ability to identify when someone does not know something and will therefore likely err, and to know when to provide needed information to help the person succeed. By the same token, PAR could underlie simple skills in deception and lying, for example by preventing someone from having perceptual access to a misdeed so that the person will not react to the misdeed. PAR could also provide a foundation for understanding mistakes or accidents as stemming from not knowing, as in, "She didn't see me put my pencil down, and she didn't know it was mine, so when she took it, she was not trying to steal it."

Understanding how children develop their first conceptions of seeing and knowing can provide insight into the nature of PAR, and can also provide suggestions about how children might develop representational ToM later on. Below we discuss our initial findings on 2- to 3-year-olds' developing understanding of seeing and knowing (Gonzales et al., 2018). The study was designed to test whether children used introspection to gain their first insights into seeing and knowing. If introspection provides children's first insights into seeing and knowing, then children should attribute seeing and knowing to themselves before attributing seeing and knowing to others. Few earlier investigators have tested for self-other differences in conceptions of seeing and knowing, and their findings have been inconsistent (see Gonzales et al., 2018 for discussion).

The study included four conditions: Self-see, other-see, self-know, and other-know. The self-conditions provided tests of when children correctly attributed seeing and knowing to themselves, and the other-conditions tested when they attributed those same states to others.

Self-See

The examiner placed two toy animals on the table in front of the child, one to the child's left and the other to the child's right. While the child watched, the examiner gradually lowered a cardboard screen in front of one so that the child could not see that animal but could still see the other. The child was asked two questions, "Right now, do you see the cat?" "Right now, do you see the dog?"

Other-See

The examiner placed two new animals on the table in the same configuration, along with a doll on the opposite side of the table from the child, so that the objects were between the child and the doll. The examiner lowered the screen so that the doll could not see one object but could see the other, while the child could still see both. The child was asked, "Right now, does Jack see the pig?" "Right now, does Jack see the horse?"

Self-Know

The examiner presented two boxes with closed lids and said, "I'm going to show you what is in the white box, but I'm not going to show you what is in the red box." He opened the lid of the white box for the child to see a toy inside, and left the lid of the red box closed. The child was asked, "Do you know what is in the white box?" "Do you know what is in the red box?"

Other-Know

The examiner placed a different doll on the table along with two new boxes, and said, “I am going to show Jane what is in the blue box, but I’m not going to show her what is in the green box.” The child was not shown what was in either box. The child was asked, “Does Jane know what is in the blue box?” “Does Jane know what is in the green box?”

The results showed that by age 2½, most children accurately reported their own visual experiences of seeing and not seeing, and by age 3, most understood when another person does and does not see (reflecting Level 1 visual-perspective taking). By age 3½, most children reported their own experiences of knowing and not knowing, and at 4, most understood when another person does and does not know (reflecting Rule 1 of PAR). Although more research is needed, the finding that children passed the self-versions of both tasks about 6 months before they passed the other-versions suggests that children were introspecting their own states of seeing and knowing. An important aim for future research is to test for other evidence of children’s ability to introspect, and for the possible role of introspection in the development of representational ToM. We will return to the issue of the development of belief reasoning in Chapters IX and X.

The finding that children were able to introspect seeing about a year before they were able to introspect knowing, suggests a possible factor that might direct their introspective attention more to seeing than knowing. It is easier for parents to tell when children see something than when children know something. Thus, parents could conceivably label the child’s mental states of seeing and not seeing more reliably than the child’s mental states of knowing and not knowing. Parents’ more accurate references to the child’s seeing than knowing could presumably focus the child’s introspective attention more reliably toward phenomenal experiences of seeing than knowing, helping the child to learn about seeing before learning about knowing.

More generally, the results tentatively suggest a transactional model of ToM development, in which children and their parents co-construct concepts of mental states. Possible transactional sequences in such a model might include the following: The child’s experience of being in a certain mental state could elicit parents’ labeling of that mental state; parents’ labeling could facilitate the child’s introspection; the child’s learning from introspection could lead the child to explicitly talk and ask questions about mental states; parents’ responses could give the child further information about causes and consequences of mental states. There are frequent calls from researchers, but few responses as yet, for transactional models of ToM development. There is a growing sense that transactional models are needed in order to broaden the traditional views that mental state concepts are either implanted by natural selection, or constructed from the child’s solitary introspection and reflection (Carpendale & Lewis, 2015; Hughes & Devine, 2015).

Logical Arguments for PAR

PAR can account for performance in false-belief tasks. Below we briefly discuss how PAR can also account for (a) performance on the other traditional tests of ToM, (b) the factors that correlate with and affect performance on false-belief tasks, and (c) performance on other tasks that assess understanding of mental representations.

Other Traditional Tests of ToM

Appearance-Reality Tasks

These tasks test children's understanding of their own mental representations that are caused by misleading visual appearances of objects (Flavell et al., 1986). Children pass these tasks at the same rates that they pass false-belief tasks.

Appearance-reality tasks use two types of objects. One type includes fake objects, such as an artificial rock made out of sponge material. Children are allowed to explore the object to discover its real identity. Afterwards, while children are looking at it but not touching it, they are asked two questions, "What is it really? A rock or a sponge?" and "What does it look like to your eyes right now? Like a rock or like a sponge?" As discussed above, fake objects are also used in the identity false-belief task, where the question is, "What will someone who only looks at it and doesn't touch it think it is?" PAR can apply in the same way to both tasks. Children know it is a sponge because they have touched it, and so the question, "What does it look like to your eyes now?" could mean to PAR-users, "if you do not touch it," in which case they would reason that if a person, themselves included, does not touch it they will not know that it is a sponge, and they will be wrong. In both tasks, rock is the only wrong option and so PAR-users would answer correctly by default, without attributing a mental representation of a rock to themselves or to another person.

The other type of objects includes objects that change appearances, such as a white card that is made to look blue by holding a transparent blue filter over it. Children are given repeated demonstrations of moving the filter back and forth over the card. Afterwards, while children are looking at the card through the filter, they are asked two questions, "What color is the card really? White or blue?" and "What color does the card look like to your eyes right now? White or blue?" Flavell et al. (1989) concluded that many 5-year-olds who answered both questions correctly were not reasoning about the visual appearance of blue when they viewed the card through the filter, a conclusion that the PAR account would agree with. Flavell et al.'s evidence showed that children were reasoning instead about two different colors at two different times: The color before, without the filter, and the color now, with the filter. Children commonly justified their correct answers by explaining, "When you lift [the filter] up it will be white and when you put it down it will be blue." The PAR account would simply add that children were reasoning about what they see and know at the two different times, by using Rule 1 of

PAR: "Now I see blue, so I know it's blue." "Before I saw white, so I knew it was white."

Level-2 Visual Perspective-Taking Tasks

These tasks test children's understanding of mental representations that are caused by the different visual perspectives that the child and another person presently have of the same object (Flavell, Everett, et al., 1981). For example, the child and an adult sit on opposite sides of a table so that a picture of a turtle that is lying flat on the table between them appears as right side up to one and upside down to the other. Children are asked how the object looks to each of them. They are pretrained ahead of time with the pair of contrastive terms to be used in the task (e.g., "right side up or upside down;" "on its feet or on its back"). Children pass these tasks somewhat earlier than they pass false-belief tasks.

The PAR account is that children do not attribute a mental representation of a visual perspective to either themselves or the adult. The PAR account would be that children are focusing on Rule 2 of PAR: "I know the turtle is on its feet, so I get it right;" "He doesn't know the turtle is on its feet, so he gets it wrong." "On its back" is the only wrong option, and so PAR-users could answer correctly by default. Perhaps pretraining on the contrastive terms leads children to assume that there will be one right answer and one wrong answer; in that case, when they themselves get it right, they could assume that the adult, who is over there, gets it wrong. A way to test the PAR account would be to include an object that looks the same from both viewpoints. PAR-users would answer that it looks different to the adult because the adult gets it wrong. We are aware of only one study that used such an object (a vertical cylinder) in a Level-2 task (Flavell, Everett, et al., 1981). Almost all of the 5½-year-olds said the cylinder looked the same to both viewers, but only half were able to justify why it looked the same, whereas all were able to justify why an asymmetrical object (a tangle of wire) looked different to the two viewers. It is likely that the pretraining could have produced an association between looking the same and the vertical cylinder, which could have led to correct answers, but which would not have led to correct justifications.

Second-Order False-Belief Tasks

These tasks are used to test advanced ToM involving recursive reasoning about beliefs. In second-order stories, the protagonist has a false belief about what a second character believes about the situation. Pared down to essentials, a story might be that John and Mary are told by the driver of an ice cream truck that the truck will be at the playground later. John and Mary agree to meet the truck at the playground. They each go home to wait, but on the way, Mary learns that the truck will be at the school instead. John believes that Mary believes that the truck will be at the playground, but his belief about Mary is false. Children are asked, "Where does John think Mary will go?"

Second-order tasks include only two response options; thus, recursive reasoning about the protagonist's belief about the other's belief is confounded with recursive use of PAR. The PAR account is that children are not reasoning about beliefs. PAR-users would reason, "John doesn't know that Mary knows that the truck will be at school, so John will be wrong about Mary." Playground is the only wrong option and so PAR-users would answer correctly by default. Some researchers use second-order task procedures that emphasize the protagonist's ignorance (e.g., "Remember, John doesn't know that Mary found out where the truck will be."). Emphasizing the protagonist's ignorance could elicit PAR in children who were in transition to PAR. Increased PAR use would explain the findings that emphasizing the protagonist's ignorance leads to substantial increases in correct performance on second-order false-belief tasks (Coull et al., 2006).

Factors That Correlate With and Improve Performance on False-Belief Tasks

Correlates of False-Belief Performance

Researchers have long found that executive function skills and language development correlate with performance on false-belief tasks (e.g., Burnel et al., 2020). Both factors conceivably have roles to play in the development of PAR. A theoretical implication of a developmental delay between PAR and later belief reasoning would suggest that the executive function and language skills that are related to the development of PAR might not be the same skills that are related to the later development of belief reasoning.

Executive function skills include inhibition, flexibility, and working memory. Inhibitory control would allow PAR-users to inhibit a reality response while they engage in PAR. Flexibility in shifting between tasks or rules would allow PAR-users to decide if the situation has changed and whether or not to engage in a new round of PAR for the new situation (see Chapter II). Working memory would allow PAR-users to maintain information about perceptual access (PAR Rule 1) while using it to predict or explain someone's behavior (PAR Rule 2).

Language development, including mental state vocabulary, clearly has a role to play in PAR. In addition, the syntactic structure of complement sentences, in which a proposition expressing a fact follows a cognition verb (e.g., think, believe, know) or a communication verb (e.g., say, tell), is used in PAR Rule 1 (e.g., "He doesn't know that his sister moved it.").

Factors That Improve False-Belief Performance

The meta-analysis of the false-belief literature revealed five factors that improved performance on false-belief tasks (Wellman et al., 2001). Four of the factors could, in principle at least, elicit PAR in children who were in transition to PAR, and thereby improve performance. Asking "Where will Maxi look first for his chocolate?" rather than the standard question, "Where will Maxi look for his chocolate?" could elicit PAR by implying that Maxi will get it wrong (see Chapter II). Conspiring with the child to trick the protagonist could also elicit PAR by implying that the protagonist will get it wrong. Actively engaging the child in moving the object during the protagonist's absence could elicit PAR by emphasizing that the situation has changed (see Chapter II). Explicitly stating the false belief, as in, "Linda thinks her cat is in the garage, but it's really in the living room. Where will she look?" could elicit PAR by suggesting that Linda gets it wrong (see Chapter VIII). (The fifth factor, object-not-present, involves removing the object from the scene before the protagonist returns, which would elicit random responses in children who use reality reasoning, because there is no reality location).

Other Tasks That Assess Understanding of Mental Representations

Other tasks that assess understanding of mental representations are often not passed until about 6–8 years of age, which is consistent with the PAR account that belief reasoning is acquired during middle childhood. We will point out a few examples; the volume edited by Astington et al. (1988) provides several other examples.

Pretending

Pretending requires a mental representation of what one is pretending to be or do. Thus, children's early pretend play has been suspected of being the first context in which children might understand mental representations. However, 3- to 5-year-olds rarely use the word pretend in natural play contexts unless it is to direct others' actions. When talking about their own actions, they tend to say, for example, "I'm a kangaroo," rather than, "I'm pretending I'm a kangaroo" (Hall et al., 1995). Lillard (2001) reviewed the experimental literature on children's understanding of pretense, and concluded that only about 25% of 4-year-olds understood the mental representational aspects of pretense (e.g., that pretending to be a kangaroo requires knowing about and thinking about kangaroos), and that the rate of understanding rose only about 15% per year until age 8.

Awareness of Thinking

Young children's unawareness of mental representation is not confined to its role in pretense. Flavell et al. (1995) studied children's understanding of thinking, and found that 3- to 5-year-olds were largely unaware of mental representations in the broadest sense of merely having thoughts about things. Compared to 7- to 8-year-olds, young children were much less likely to understand when someone, including themselves, was thinking, or what they are thinking about, despite strong behavioral and situational cues. After two dozen studies of various aspects of thinking (attentional focus, inner speech, controllability and uncontrollability, unconsciousness, thought-emotion and thought-action links), Flavell and Flavell (2004) concluded that “a family of fundamental intuitions concerning the what and when of thinking … are in the process of acquisition during the late preschool and middle-childhood years” (p. 451).

Other Examples

Lags between passing false-belief tasks in early childhood, and understanding mental representation in middle childhood can be found in other areas. Examples include children’s understanding of what emotion a protagonist with a false belief will have, their reasoning about counterfactuals, and their abilities to maintain lies and persuade others.

Understanding that a person’s emotions stem from the person’s false belief requires belief reasoning about the content of the person’s belief. Without being able to represent what a person falsely believes, PAR-users could be expected to fall back on the objective situation to predict the person’s emotion, and “claim for example that even if Little Red Riding Hood mistakenly thinks there is only her grandmother waiting for her inside the cottage and that she knows nothing about the wolf, she will still feel afraid” (Harris et al., 2014, p. 106).

Counterfactual reasoning requires mentally representing a different outcome of a past event. “Only by 6 years of age do children show compelling evidence that they can imagine an alternative version of a past event for which the outcome is known, and reason about how the present would look had that alternative past event actually transpired” (Gautam et al., 2019, p. 6).

Maintaining lies and persuading others both require, at a minimum, reasoning about the content of the person’s belief; young children have limited skills in both areas (Bartsch, London, et al., 2007; Talwar & Lee, 2008).

Representations of the Self

Mental representations of the self, including autobiographical memory and self-concepts, show quantitative and qualitative changes around age 8. Genuine, lasting autobiographical memories, in which children richly and spontaneously describe the self as having experienced chronologically sequenced events, are rare much before the age of 8 (Nelson et al., 1983). Young children’s recall of past events, such as their first day of kindergarten, tends to be generic, script-like, and not especially personal (e.g., “You hang up your coats. Then you have snack.”). Their recall is also relatively short-lived. In carefully controlled conditions, 8-year-olds produced longer descriptions of events that they had experienced up to four months earlier than 4- and 6-year-olds produced (Bauer & Larkina, 2019).

Self-concepts in early childhood include primarily positive behavioral characteristics and basic emotions about external things. In middle childhood, new abilities are acquired to self-criticize, have emotions of pride and shame, and express higher-order generalizations about the self in statements such as, "I'm not bad, I just do bad things sometimes." Until recently, it was widely accepted that children were unable to mentally represent specific behaviors as higher-order representations of traits in others (Rholes & Ruble, 1984), or as higher-order representations of their own global selves or self-esteem "until about the age of 8, normatively" (Harter, 2012, p. 17). Motivated in large part by young children's ability to pass false-belief tasks, considerable debate has arisen about the nature of early self-concepts, involving definitional issues, simplification of tasks, and interpretation of findings. It is too early to know how the debate will end, but the theory of PAR could provide, as Cimpian et al. (2017) note that Piaget's theory once did, "a strong reason to expect a qualitative shift between early and middle childhood in the representational abilities relevant to reasoning about oneself as an individual" (p. 1788).

The Current Studies

> *About 20 years ago, when asked in a meeting what can be done in observational studies to clarify the step from association to causation, Sir Ronald Fisher replied: "Make your theories elaborate." What Sir Ronald meant, as subsequent discussion showed, was that when constructing a causal hypothesis one should envisage as many different consequences of its truth as possible, and plan observational studies to discover whether each of these consequences is found to hold* (Cochran, 1965, p. 252).

Overview

The most immediate potential contribution of this monograph, in our view, is the contribution of the belief understanding scale (BUS) for distinguishing PAR from belief reasoning that is described and employed in Chapter X. The BUS involves a simple, two-task battery of one standard contents false-belief task and one contents true-belief task. PAR theory makes the counter-intuitive prediction that at the same time that children begin to pass false-belief tasks, children should begin to fail true-belief tasks (see Chapter II). The BUS tests for the existence of PAR.

The theory of PAR is not in its final form, and the empirical studies of the theory that are presented in this monograph do not provide a definitive test of the theory. The studies we report were the means by which we developed the BUS, and by which we tested it along the way. Those tests included obtaining important assurances that presenting the false-belief task before the true-belief task does not produce carry-over effects. Now that we have developed the BUS, it can be used in longitudinal studies which would allow tests of whether all children do in fact use PAR before acquiring a representational ToM in middle childhood.

Each study presented in this monograph provides a test of a specific prediction derived from PAR theory. Some of the tests are weaker than others due to small sample sizes or the possibility of undetected order effects, and all but one of the studies lack controls for children's language development and families' demographic factors, both of which are known to affect false-belief performance. Most importantly, viable, non-PAR alternate explanations of individual studies are possible.

However, presenting all the tests, weak and strong, together in this monograph serves two purposes. Showing how each test relates to PAR theory clarifies the constructs and causal explanatory mechanisms of the theory. Showing how the overall pattern of the findings across studies fits comfortably within PAR theory provides some assurance of the viability of the theory. Readers will make their own decisions about how to weigh the overall pattern of findings against the strengths and weaknesses of individual studies.

Our hope with this monograph is to offer a challenge that stimulates further empirical tests and further development of the theory. The challenge is timely because PAR theory addresses two pressing issues confronting the field today. PAR theory provides a principled explanation of the previously unrecognized and unexplained findings in the true-belief literature of young children failing various true-belief tasks (Chapter II). PAR theory also forces a reconsideration of ToM-related abilities in infants and toddlers, and raises a serious hurdle for recent claims that representational ToM is innate.

Chapter Summaries

Our approach within each study as well as across the whole set of studies uses the strategy (referenced in the quote at the beginning of this section) of pattern-matching to test causal hypotheses (Campbell, 1966). In pattern-matching, the theoretical predictions derived from the target theory and from the alternative theory are matched to evidence from many different sources, or studies. The degree to which the overall pattern of evidence across the different studies matches one set of theoretical predictions rather than the other set provides stronger evidence than the findings from any one study. Sabbagh and Paulus (2018, p. 2) succinctly articulate the pattern-matching strategy: "We assume that when a phenomenon varies systematically with theoretically meaningful variables, and generalizes across putatively trivial variables, the more likely it is to capture something important about the human mind."

Evidence in support of PAR requires the following types of evidence from young children: Evidence of failing true-belief and false-belief tasks (Chapters II and III); specific evidence of PAR Rule 1 and PAR Rule 2

(Chapters IV and V); evidence that attributions of true beliefs and false beliefs pose similar difficulties (Chapter VI); evidence that children's justifications and explanations do not contradict the theory (Chapters VII and VIII), and evidence that distinguishing PAR from belief reasoning has predictive validity (Chapter IX).

Below we briefly describe the approach we used in each chapter to test the predictions derived from the target PAR theory versus the predictions derived from the alternate theory that young children understand false belief. Table 1 provides a summary of the chapters.

In Chapter II, we provide evidence that children fail true-belief tasks by reporting a secondary analysis of the published true-belief literature. Consistent with predictions derived from PAR theory, performance varies systematically with five meaningful procedural variations. True belief performance should not vary if children understood false belief.

In Chapter III, we provide evidence that children fail false-belief tasks by reporting seven replication studies using 3-option false-belief tasks. Consistent with PAR theory, children's correct performance did not generalize

TABLE 1
THEORETICAL PREDICTIONS

Chapter	If Children Use PAR	If Children Understand False Belief
II	True-belief performance should vary systematically with age and meaningful procedural variations	True-belief performance should not vary
III	False-belief performance on 3-option tasks should vary with superficial procedural variations	False-belief performance on 3-option false-belief tasks should not vary
IV	The lag between understanding ignorance and understanding false belief should be longer than previous findings	The lag should be similar to previous findings
V	Children should make similar judgments about protagonists with false beliefs and protagonists with no beliefs	Children should make different judgments
VI	Added processing demands should disrupt true-belief as well as false-belief performance	Added processing demands should not disrupt true-belief performance
VII	Children should pass false-belief and fail true-belief tasks, and give the same justifications in both cases	Children should pass both false-belief and true-belief tasks
VIII	Children should give similar explanations of the behavior of protagonists with false beliefs and protagonists with no beliefs	Children should give different explanations
IX	The belief understanding scale should account for more variance than the false-belief task in inhibitory control, social skills, and kindergarten adjustment	The false-belief task should account for more variance

Note. PAR = perceptual access reasoning.

across superficial procedural variations. False-belief performance should generalize if children understood false belief.

In Chapter IV, we provide evidence that children use Rule 1 of PAR by testing the developmental lag between understanding ignorance and understanding false belief, using 3-option false-belief tasks to remove the confound between PAR and belief reasoning. Consistent with PAR theory, the lag was longer than has been found previously. The lag should be short if children understood false belief.

In Chapter V, we provide evidence that children use Rule 2 of PAR by comparing false-belief tasks to no-belief tasks, in which the false-belief information is removed and the protagonist has no belief. Consistent with PAR theory, most children predicted that protagonists would get it wrong in both cases. If children understood false belief, they should predict that the protagonist with no belief would guess, while the protagonist with a false belief would get it wrong.

In Chapter VI, we provide evidence that children have similar difficulties attributing true beliefs and false beliefs by testing for a default attribution of true belief. Consistent with PAR theory, added processing demands produced similar disruption of true-belief and false-belief performance. If children had a default attribution of true belief, added processing demands should disrupt false-belief performance more than true-belief performance.

In Chapter VII, we provide evidence that children's justifications do not contradict PAR theory by comparing children's justifications of their answers in false-belief tasks and true-belief tasks. Consistent with PAR theory, many children passed the false-belief and failed the true-belief tasks, and then gave the same justifications in both cases by referring to the protagonists getting it wrong. If children understood false belief, they should pass both tasks.

In Chapter VIII, we provide evidence that children's explanations do not contradict PAR theory by comparing children's explanations of protagonists' actions in false-belief tasks and no-belief tasks. Consistent with PAR theory, many children gave the same explanations in both cases by referring to the protagonists getting it wrong. If children understood false belief, they should give different explanations in the two cases.

In Chapter IX, we provide evidence that distinguishing PAR from belief reasoning has predictive validity by using the BUS to account for individual differences among children. In accord with PAR theory, the BUS accounted for more variance than the false-belief task in individual differences in inhibitory control, social skills, and kindergarten adjustment after controlling for children's language skills and gender, and families' socio-economic status (SES).

Wellman et al. (2001, p. 672) admonish, "Conceptual change accounts require … a general multimethod approach to construct validity: a variety of tasks, all conceptually similar but varying in their task specifics, should lead to similar developmental changes." We heeded their requirement to use all three tasks (location, contents, and identity), not only in the false-belief versions of those tasks, but also in the true-belief versions, as well as in new, no-belief versions.

Each chapter begins with a preface that introduces and illustrates the conceptual issue that the chapter addresses. The preface also provides a brief, idealized example of the new tasks that were used in the studies reported in that chapter. All examples are location tasks, with Maxi as the protagonist.

II. Secondary Analyses of the True-Belief Literature

Maxi puts his chocolate in cupboard A.
Then Maxi watches his mother move it to cupboard B.
Maxi leaves and then returns.
Where will Maxi look for his chocolate?

The example above is a true-belief location task with a return condition, which refers to the fact Maxi leaves and returns before the child is asked about Maxi's belief. Several other true-belief conditions have been used at various times by researchers, and those different conditions play a prominent role in the analyses reported in this chapter. True-belief tasks, however, have seldom been used by researchers because they had judged these tasks to be too easy. That is, researchers have assumed that children who passed false-belief tasks should be able to pass true-belief tasks. True-belief tasks do not require children to understand that Maxi has a belief that contradicts reality. In this chapter, we show that true-belief tasks are not so easy, at least some of them. The evidence comes from analyzing all the previous true-belief studies together. Researchers have been unaware of the systematic errors that young children make on true-belief tasks. PAR theory predicts those errors. The most striking example of a successful PAR-prediction is that as children get older, they do worse on true-belief tasks before they do better.

Introduction

PAR theory makes the counter-intuitive prediction that at the point in development when children begin to pass false-belief tasks, they should begin to fail true-belief tasks because both reflect the development of PAR. As children later develop belief reasoning, their performance on true-belief tasks should reverse direction, resulting in a U-shaped developmental pattern for true-belief tasks. Fabricius et al. (2010) found the predicted pattern in three studies of 3- to 7-year-olds using three versions of true-belief tasks (i.e., location, contents, and identity). In the true-belief contents and identity tasks, the predictions that 5-year-olds would fail were especially counter-intuitive. These tasks began like false-belief tasks. The child saw that an M&Ms bag contained a pencil, and that a fake rock was a sponge. They became true-belief tasks when the child watched the experimenter remove the pencil and put real M&Ms in the bag, and remove the fake rock, place a real rock on the table, and say, "I have a real rock here. Feel it." Children erred by predicting that someone who did not look in the bag or touch the rock would get it wrong, and say there was a pencil in the bag, and the rock was a sponge.

The vast majority of true-belief studies use location tasks, and enough such studies now exist for a secondary analysis of the literature to test for the U-shaped developmental pattern. In addition, several other predictions exist about which location task conditions should interfere with PAR, and which conditions should tend to elicit PAR. A test of these predictions is needed as well, because there is no standardized true-belief location procedure, and there have been few tests of whether procedural variations affect performance.

Primary Conditions

Three primary conditions have been used in enough true-belief location studies to be included as factors in a secondary analysis. We will refer to these conditions as *protagonist movement*, *highlight*, and *object movement* conditions.

Protagonist Movement

Protagonist movement refers to what the protagonist does after acquiring the true belief. There are three protagonist movement conditions: stay, leave, and return. As noted in the example above, Maxi leaves and then returns, making it a return condition. If Maxi left and did not return, so that the test question is asked in his absence, it would be a leave condition. If Maxi did not leave, so that the test question is asked immediately after he watched his mother move his chocolate to cupboard B, it would be a stay condition.

As discussed by Fabricius et al. (2010) and by Hedger and Fabricius (2011), these are important distinctions. PAR theory specifies that children analyze only the current situation to determine whether the protagonist has perceptual access. Maxi watches during the hiding phase as his mother moves the chocolate from cupboard A to cupboard B, and children using PAR would conclude that Maxi sees and knows. However, if Maxi left and returned, his return would prompt children to see it as a new situation, and to engage in a new round of PAR about this second situation (i.e., "Maxi doesn't see the chocolate, so he doesn't know where it is, so he'll be wrong").

If Maxi stays, there would be no indication to the child that the situation has changed. The theory specifies that when children do not decide that the situation has changed, they simply default to the conclusion they came to during the hiding phase, that Maxi sees and knows, and they will answer that he will get it right in stay conditions. Technically speaking, at the moment the chocolate is hidden Maxi is out of perceptual contact with it. But it would make no sense for children to immediately reapply PAR and conclude that he now does not know where it is, and that he will get it wrong if he goes to get his chocolate now. There would be no incentive in real social situations to reapply PAR to momentary breaks in perceptual access, and doing so would take effort and attention away from ongoing social interactions. PAR theory entails that return conditions are more likely to elicit PAR than stay conditions; thus, children will be more likely to answer correctly in stay than in return conditions.

Some studies have used leave conditions. Predictions are less clear for the leave condition relative to the stay and return conditions. It is an empirical question whether simply leaving indicates that the situation has changed. In the only direct, but partial test of the impact of these contrasting conditions, Friedman et al. (2003) predicted, and found, that more children answered correctly in leave than return conditions. (The researchers did not include a stay condition.) Friedman's findings suggest that the return condition was more likely to indicate that the situation had changed than the leave condition, and thus was more likely to elicit PAR.

Highlight

Highlighting refers to reminding the child immediately before asking the test question (i.e., with no intervening questions) that the protagonist had earlier seen the object placed in the true-belief location. Friedman et al. (2003) referred to this reminding procedure as one that highlights the protagonist's prior perceptual access. They argued, and we agree, that highlighting would interfere with reasoning about the protagonist's current lack of perceptual access by drawing the child's attention away from the fact that the protagonist does not now see the object in the true-belief location. PAR theory entails that highlighting the protagonist's prior perceptual access will interfere with PAR, in which case, like in stay conditions, children will simply default to their initial conclusion that the protagonist sees and knows; thus, children will be more likely to answer correctly in highlight conditions than in no-highlight conditions.

Use of highlighting has been noted by reviewers. Friedman et al. (2003) noted that "Clements and Perner (1994) highlighted the observer's perceptual access by asking children whether the observer had seen the object being moved to the [true-belief] location. This question was asked after the observer left the room, and shortly before the true-belief question was asked" (p. 510). They also reported that Surian and Leslie (1999) had highlighted the protagonist's prior perceptual access. Fabricius et al. (2010) noted that Wimmer and Perner (1983, p. 109) had told children immediately before asking the test question that Maxi "still remembers where he had put the chocolate," and that Roth and Leslie (1991, p. 324) embedded highlighting within the test question: "Remember, the monkey saw everything, where does the monkey think the chocolate is?" Consistent with the PAR prediction, in most of the highlight conditions all children answered correctly.

Question

Question refers to whether the test question asks where the protagonist will look for the object, or where the protagonist will look first. The phrase, look first, is one example of many types of statements that can imply different meanings (Grice, 1989). Children have to work out what the adult might mean by look first. It originated in false-belief tasks to address a concern that children might misinterpret the standard form of the question (e.g., "Where will Maxi look for his chocolate?") to refer to where he will need to go to get it (Lewis & Osborne, 1990; Siegal & Beattie, 1991). Look-first questions improve false-belief performance, and researchers assumed that it helps children work out that the test question refers to the protagonist's false belief, not to reality. However, Fabricius et al. (2010) pointed out that for children beginning to acquire PAR, "the word 'first' may have encouraged perceptual access reasoning by implying that the first search would be wrong" (p. 1414; see also Surian & Leslie, 1999). PAR theory entails that look-first questions will be more likely to elicit PAR than look questions; thus, children will be more likely to answer correctly in true-belief look conditions than in true-belief look-first conditions.

Consistent with PAR, Rai and Mitchell (2004) found that 5-year-olds' true-belief performance fell from .85 correct in their no-highlight/look/stay condition to .25 correct in the no-highlight/look-first/stay condition. In three studies reported by Oktay-Gür and Rakoczy (2017), Study 1 records the question as, "Where will the protagonist look for the X?" (p. 31). However, the actual wording was, "Wo wird sie nach … zuerst suchen?" ("Where will she look first for…?") (German personal communication from H. Rakoczy, May 13, 2018). Study 2 records that, "The procedure … remained the same as in Study 1" (p. 34). Study 3 records that a new question was asked that removed the word first, "What does she think where the … is" (p. 36) ("Was glaubt sie, wo … ist?") (Personal communication from H. Rakoczy May 13, 2018).

Secondary Conditions

There are two secondary conditions, aspectuality and object movement, that have not been used in enough studies to be included as factors in the overall secondary analysis; for these, we conducted preliminary tests.

Aspectuality

In *aspectuality* conditions, the object has two identities, or aspects, and the protagonist either knows about only one aspect, or knows about both aspects. Understanding the aspectuality of the protagonist's belief involves reasoning about what he does and does not know, and consequently what he believes about the object under different aspects. It is the essence of disguise. With a representational ToM, one can understand that a protagonist who does not know that Clark Kent is Superman can have a true belief that Superman is flying over the building, and a false belief that Clark Kent is still in the phone booth, even though they are the same person. Understanding the aspectuality of beliefs "makes no sense" (Perner et al., 2015, p. 87; see also Rakoczy, 2017) without having a representational ToM. Thus, it is incumbent on PAR theory that children who use PAR cannot reason about the aspectuality of beliefs.

Russell (1987) was the first to use an aspectuality task, in which the child was told that a burglar had red hair, and that the protagonist did not know that. The test question was, "Can we say that [protagonist] is looking for a man with red hair?" Consistent with PAR, most 5- and 6-year-olds and half of 7-year-olds failed the task and answered yes. Other researchers have obtained similar findings (Apperly & Robinson, 1998, 2001, 2003; Kamawar & Olson, 1999, 2009, 2011; Sprung et al., 2007).

Researchers have been concerned that aspectuality tasks might be too verbally sophisticated for young children, and have attempted to simplify the task and question children are asked. Hulme et al. (2003) confirmed Russel's (1987) findings with a nonverbal test question that asked children to select the picture that showed what the protagonist was thinking. Others have attempted to simplify the task by making it a false-belief location task. To continue with the Superman example, in the aspectuality false-belief location task, the protagonist first watches Clark Kent enter the phone booth, and then the protagonist leaves. In his absence, the child watches Clark Kent emerge briefly, change into Superman, and then go back into the phone booth. So only the child knows that Clark Kent is Superman. The protagonist returns, watches Superman fly into the other location, and stays in the scene while the child is asked the false-belief question, "Where will [protagonist] look for Clark Kent?" Children who use belief reasoning would understand that the protagonist has a false belief that Clark Kent is still in the phone booth. However, this task reinstates the confound between belief reasoning and PAR. It can be solved by PAR in the same way that other false-belief tasks can: "He didn't see Clark Kent fly into the other location, so he doesn't know Clark Kent is there, so he will get it wrong and say he is in the phone booth."

As elsewhere, the true-belief version removes the confound. In this case, the protagonist watches, as does the child, while Clark Kent changes into Superman. Children who use belief reasoning understand that after watching Superman fly into the other location, this protagonist will have a true belief that Clark Kent is in the other location. Those who use PAR will fail the aspectuality true-belief location task because they reason the same way as in the false-belief version: "He didn't see Clark Kent fly into the other location, so he doesn't know Clark Kent is there, so he will get it wrong and say he is in the phone booth."

All aspectuality true-belief location studies used stay conditions. As noted, in stay conditions with standard tasks, children should tend to default to their initial conclusion that the protagonist sees and knows. In stay conditions with aspectuality tasks, however, children should be more likely to engage in another round of PAR because the question is about the aspect (e.g., "Clark Kent") that the protagonist did not see move to the second location. The PAR theory entails that in stay conditions, aspectuality tasks are more likely to elicit PAR than standard tasks; thus, children will be more likely to answer correctly in standard than in aspectuality tasks.

Object Movement

The second infrequent procedural variation involves inconsequential object movement. During the protagonist's absence, the object is temporarily removed from, but then put back into the true-belief location. Fabricius et al. (2010) hypothesized that object movement, "much like the protagonist's return … would prompt children to see this as now a new situation and to use perceptual access reasoning about this new situation" (p. 1404). The PAR theory entails that inconsequential object-movement indicates that the situation has changed, and is more likely to elicit PAR than no object-movement; thus, children will be more likely to answer correctly in no object-movement than in object-movement conditions.

The three primary and two secondary procedural variations that should affect use of PAR allowed an additional prediction; namely, the U-shaped developmental function will be present in all conditions that elicit PAR, and not in conditions that do not elicit PAR. To summarize, the following are hypotheses derived from PAR:

Hypothesis 1. A U-shaped pattern should be found between age and performance in all conditions. Furthermore, the pattern should vary depending on whether conditions do or do not elicit PAR. Specifically:

IA. A U-shaped pattern should be found between age and performance in all conditions that elicit PAR.

IB. No association should be found between age and performance in conditions that do not elicit PAR.

Hypothesis 2. The following conditions should affect use of PAR:

2A. Protagonist Movement: Return should elicit more PAR than Stay.

2B. Highlighting: Highlighting should interfere with PAR.

2C. Question: Look-First should elicit more PAR than Look.

2D. Aspectuality: Aspectuality tasks should elicit more PAR than standard tasks.

2E. Object Movement: Movement should elicit more PAR than No Movement.

Method

Literature Search and Inclusion Criteria

To be conservative and work against our hypotheses, we limited our search to published studies. We reasoned that unpublished studies would be more likely to include true-belief conditions with higher error rates, because true-belief conditions have often been used as controls. Thus, we conducted a thorough and systematic search for published studies that reported true-belief data. We began with publications already known to us, and searched their reference sections to find earlier studies, repeating this process for each newly identified study. We used the "cited by" function in Google Scholar for each identified study to find later studies, using that process iteratively as well. We also searched online databases of published studies using Google Scholar and the key words "true belief," and "theory of mind," and again followed the references and subsequent citations. The search was completed during June, 2017, and no studies published after this time were included in the analyses.

The search initially yielded 33 publications that included true-belief data. Many included multiple studies and conditions. We defined conditions as either separate age groups or different variants of true-belief task procedures. The individual conditions were assessed to determine if they met the following five criteria to be included in the secondary analyses: (1) a true-belief location task; (2) a question about the protagonist's belief rather than knowledge; (3) an explicit response (verbal, pointing, or acting-out of the protagonist's search), rather than an implicit eye-gaze or helping response; and (4) typically developing children not selected on the basis of having passed a false-belief task. This yielded 76 conditions with 1,324 children aged 32 to 127 months from 19 publications.

Coding

Table 2 shows the coding, *N*, mean age (months), and proportion of children passing the true-belief task in each condition. If there was more than one trial, the dependent variable is the average proportion of correct responses. The first two authors coded the five conditions. For protagonist movement, stay meant the protagonist either stayed in the scene, or left and returned prior to watching the object move to the true-belief location. Leave and return meant the protagonist left after the object moved; the test question was asked while he was away in leave, and after he returned in return. Look questions included where the protagonist will either look for the object or think it is. Highlighting had to precede the test question with no intervening questions. We requested and obtained clarification of procedures and English translations of test questions from four sets of authors.

Table 3 shows that the 76 conditions include at least one condition in 11 of the 12 cells of the cross-tabulation of the three primary procedural

TABLE 2

Procedural Variations, Number (N), Mean Age (Months), and Proportion Passing the True Belief (TB) Task in Each Condition

	Condition	Primary Procedural Variations					
Publication	Study/Authors' Condition Label/Age Group	Highlight	Question	Movement	N	Age	Pass TB
Wimmer and Perner (1983)	Study 3/x-x/4 years[a]	Yes	Look	Return	12	52	1.00
	Study 3/x-x/5 years[a]	Yes	Look	Return	12	64	1.00
Roth and Leslie (1991)	Study 2/3 years	Yes	Look	Stay	28	40	0.89
	Study 2/5 years	Yes	Look	Stay	25	65	0.84
Clements and Perner (1994)	Action prediction standard/older 2 years	Yes	Look	Leave	11	32	0.73
	Action prediction standard/young 3 years	Yes	Look	Leave	11	37	1.00
	Action prediction standard/older 3 years	Yes	Look	Leave	11	41	1.00
	Action prediction standard/young 4 years	Yes	Look	Leave	11	49	1.00
	Action prediction look first/older 2 years	Yes	Look first	Leave	11	32	1.00
	Action prediction look first/young 3 years	Yes	Look first	Leave	11	37	1.00
	Action prediction look first/older 3 years	Yes	Look first	Leave	11	41	1.00
	Action prediction look first/young 4 years	Yes	Look first	Leave	11	49	1.00
Surian and Leslie (1999)	Study 1/see standard	Yes	Look	Return	18	46	1.00
	Study 1/see look first	Yes	Look first	Return	14	45	0.83
Garnham and Perner (2001)	Study 1/anticipate-protagonist	Yes	Look	Leave	44	39	0.96
	Study 2/anticipate-protagonist	Yes	Look	Leave	24	39	0.79
	Study 2/reflect matt move	Yes	Look	Leave	24	39	0.92
Mitchell et al. (1999)	Study 1/opaque/3 years	No	Look	Stay	20	46	0.80
	Study 1/opaque/4 years	No	Look	Stay	20	58	0.90
	Study 1/opaque/5 years	No	Look	Stay	20	73	0.85
	Study 1/opaque/6 years	No	Look	Stay	20	83	0.70
Garnham and Ruffman (2001)	Verbal prediction	No	Look first	Leave	32	37	0.72
Ruffman et al. (2001)	3 years	No	Look	Leave	52	42	0.92
	4 and 5 years	No	Look	Leave	33	52	0.85
Friedman et al. (2003)	Study 2/no return	No	Look	Leave	34	55	0.74

(Continued)

TABLE 2. (*Continued*)

	Condition	Primary Procedural Variations					
Publication	Study/Authors' Condition Label/Age Group	Highlight	Question	Movement	N	Age	Pass TB
	Study 2/return	No	Look	Return	29	55	0.69
Rai and Mitchell (2004)	Study 2/do next	No	Look	Stay	19	65	0.84
	Study 2/look first	No	Look first	Stay	20	65	0.25
Lohmann et al. (2005)	Seen switch	No	Look first	Stay	43	44	0.63
	Tell	No	Look first	Stay	50	44	0.62
	No switch[a]	No	Look first	Return	52	44	0.81
Kikuno et al. (2007)	Study 3/discriminative/3 years	No	Look	Stay	29	42	0.97
	Study 3/discriminative/4 years	No	Look	Stay	29	54	0.93
	Study 3/unseen aspect/3 years	No	Look	Stay	28	42	0.89
	Study 3/unseen aspect/4 years	No	Look	Stay	28	54	1.00
Fabricius et al. (2010)	Study 1/3 years[a]	No	Look	Leave	29	40	0.86
	Study 1/4 years[a]	No	Look	Leave	28	54	0.54
	Study 1/5 years[a]	No	Look	Leave	29	66	0.79
	Study 1/6 years[a]	No	Look	Leave	22	75	0.86
	Study 2	No	Look	Return	18	65	0.67
Low (2010)	Study 1/3 years	No	Look	Leave	12	44	1.00
	Study 1/4 years	No	Look	Leave	12	55	1.00
	Study 2/3 years	No	Look	Leave	18	42	1.00
	Study 2/4 years	No	Look	Leave	18	52	1.00
	Study 3/3 years	No	Look	Leave	21	40	1.00
	Study 3/4 years	No	Look	Leave	21	52	1.00
Rubio-Fernandesz and Geurtz (2013)	Study 1/control true belief	No	Look	Stay	12	46	0.83
Rubio-Fernandesz and Geurtz (2016)	Study 1/TB control	No	Look	Stay	20	43	1.00
Perner et al. (2015)	Predication—present	No	Look	Stay	51	67	0.86
	Individuation—present[b]	No	Look	Stay	51	67	0.45
Rakoczy et al. (2015)	Study 1/extensional property	No	Look	Stay	20	59	0.75

(*Continued*)

TABLE 2. (*Continued*)

Publication	Condition	Primary Procedural Variations					
	Study/Authors' Condition Label/Age Group	Highlight	Question	Movement	N	Age	Pass TB
	Study 1/extensional sortal	No	Look	Stay	20	59	0.60
	Study 1/TB property	No	Look	Stay	20	59	0.80
	Study 1/TB sortal[b]	No	Look	Stay	20	59	0.65
Oktay-Gür and Rakoczy (2017)	Study 1/TB standard	No	Look first	Stay	23	55	0.52
	Study 1/TB aspectuality[b]	No	Look first	Stay	23	55	0.54
	Study 2/TB standard/3-years	No	Look first	Stay	9	39	0.67
	Study 2/TB standard/3.5 years	No	Look first	Stay	11	44	0.64
	Study 2/TB standard/4 years	No	Look first	Stay	14	54	0.21
	Study 2/TB standard/5 years	No	Look first	Stay	9	66	0.22
	Study 2/TB standard/6 years	No	Look first	Stay	13	70	0.15
	Study 2/TB standard/8-years	No	Look first	Stay	10	102	0.40
	Study 2/TB standard/10 years	No	Look first	Stay	12	127	0.83
	Study 2/TB aspectuality/3 years[b]	No	Look first	Stay	5	39	1.00
	Study 2/TB aspectuality/3.5 years[b]	No	Look first	Stay	15	44	0.53
	Study 2/TB aspectuality/4 years[b]	No	Look first	Stay	12	54	0.42
	Study 2/TB aspectuality/5 years[b]	No	Look first	Stay	10	66	0.10
	Study 2/TB aspectuality/6 years[b]	No	Look first	Stay	12	70	0.17
	Study 2/TB aspectuality/8 years[b]	No	Look first	Stay	10	102	0.40
	Study 2/TB aspectuality/10 years[b]	No	Look first	Stay	10	127	0.80
	Study 3/standard/3 years	No	Look	Return	20	44	0.76
	Study 3/standard/4 years	No	Look	Return	41	53	0.70
	Study 3/standard/6 years	No	Look	Return	40	78	0.80
	Study 3/aspectuality/3 years[b]	No	Look	Stay	20	45	0.75
	Study 3/Aspectuality/4 years[b]	No	Look	Stay	41	53	0.68
	Study 3/aspectuality/6 years[b]	No	Look	Stay	40	78	0.74

[a]Object movement condition.
[b]Aspectuality task.

TABLE 3
Number of Conditions in Each Combination of Primary and (Secondary) Procedural Variations

		Protagonist Movement		
Highlight	Question	Stay	Leave	Return
No	Look	20 (5 aspectuality)	13 (4 object movement)	5
	Look first	19 (8 aspectuality)	1	1 (object movement)
Yes	Look	2	7	3 (2 object movement)
	Look first	0	4	1

variations. Most conditions included no highlight ($N = 59$) versus a highlight ($N = 17$); a look question ($N = 50$) versus a look-first question ($N = 26$); and a protagonist who stayed in the scene after acquiring the true belief ($N = 41$), versus one who left and did not return ($N = 25$) or one who returned ($N = 10$).

Results

Hypothesis 1

Figure 1 shows the best-fitting linear and quadratic lines in the scatterplots of all 76 conditions with increasing age (months). In Figure 1a, the dependent variable is the mean proportion correct in each condition, and in Figure 1b, those means are transformed via the logit transformation. Where the proportion is 1.00, we changed it to 0.996 so that a logit score could be calculated. The logit is the natural logarithm of the odds of correct versus incorrect responses. The logit values of 0, 1, 2, and 3 correspond to mean proportion correct values of 0.50; 0.73; 0.88; and 0.95, respectively. The logit ranges from negative to positive infinity, thus eliminating the restricted range of proportion data.

Hypothesis 1 was confirmed in an analysis of covariance (ANCOVA) of proportion correct (logit) with only the linear and quadratic effects of age entered as covariates. The negative linear and quadratic lines in Figure 1b provided significant independent fits, $F(1, 73) = 13.27$, $p = .001$, and $F(1, 73) = 8.52$, $p = .005$, respectively. The mean proportions correct (Figure 1a) were 0.87 for 3-year-olds (mean ages 32 to 47 months; $N = 32$ conditions); 0.76 for 4-year-olds (48–59 months; $N = 23$); 0.53 for 5-year-olds (60–71 months; $N = 12$); and .71 for children aged 6 years and older (72–127 months; $N = 9$). This indicates increasing use of PAR from 3 through 5 years of age, and beginning use of belief reasoning after 6 years.

Hypothesis 2D

The prediction is that, in stay conditions, children will be more likely to answer correctly in standard than in aspectuality tasks. Three studies

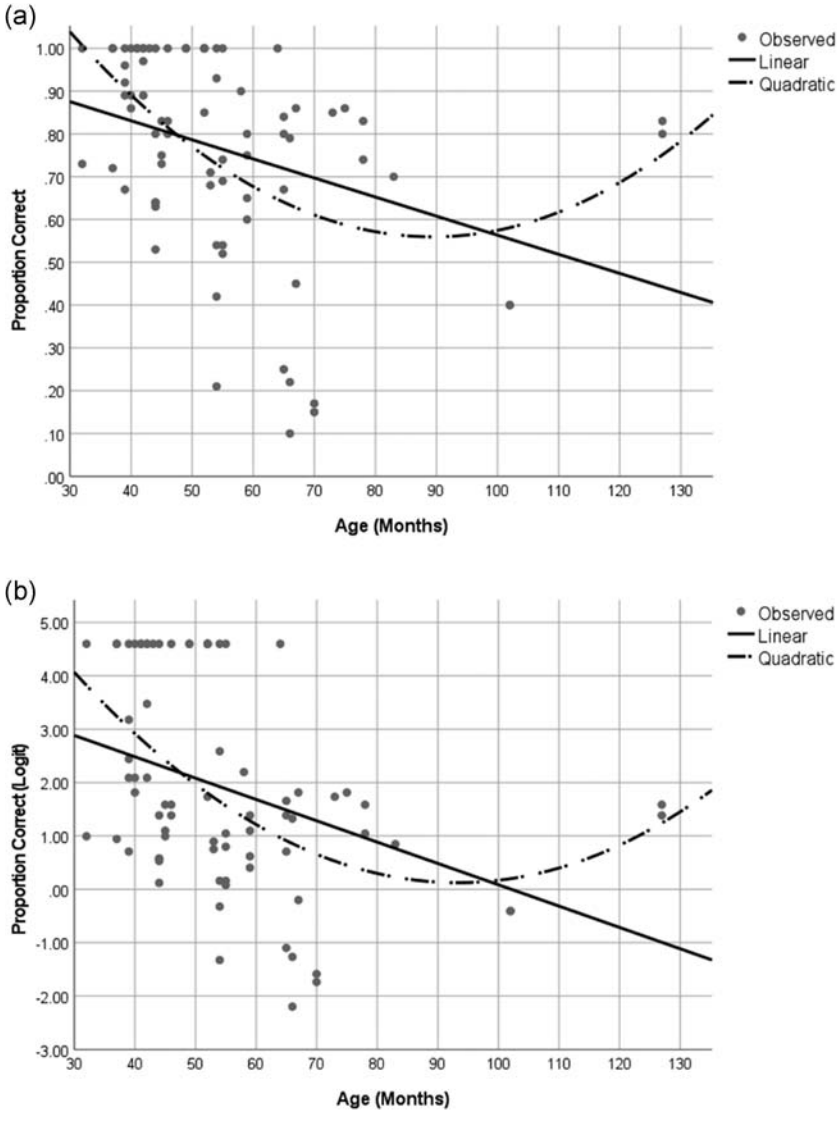

FIGURE 1.—Scatterplots of conditions with increasing age showing best-fitting linear and quadratic lines. Each condition is represented by its mean proportion correct (a), and by those scores transformed by the logit transformation (b).

(13 conditions) used aspectuality tasks: Perner et al. (2015, individuation), Rakoczy et al. (2015, Study 1 true-belief sortal), and Oktay-Gure and Rakoczy (2017, true-belief aspectuality in all three studies). These conditions occurred in two cells of Table 3 (no-highlight/look/stay, and no-highlight/look-first/stay). Both cells also included conditions with standard objects, which allowed within-cell, like-to-like tests of the aspectuality of beliefs. These and all following tests were ANCOVAs of proportion correct (logit) controlling for linear and quadratic effects of age.

Hypothesis 2D was confirmed in the no-highlight/look/stay cell. Five conditions included aspectuality tasks and 15 included standard tasks, and a one-way ANCOVA revealed a significant effect of Task, $F(1, 16) = 5.89$, $p = .027$, estimated at age 58 months. For standard tasks, the marginal mean proportion correct (logit) = 2.05, and for aspectuality tasks, $M_{\text{logit}} = 0.76$ (corresponding to M proportion correct = .88 and .68, respectively). This finding indicates that aspectuality tasks elicited more PAR than standard tasks.

Hypothesis 2D was not confirmed in the no-highlight/look-first/stay cell. Eight conditions included aspectuality tasks, and 11 included standard tasks, and a one-way ANCOVA revealed no effect of Task, $F(1, 15) = 1.04$, $p = .324$, estimated at age 67 months. The overall $M_{\text{logit}} = 0.03$ ($M = 0.50$). This finding indicates that a look-first question elicited PAR to similar degrees in standard tasks and in aspectuality tasks.

Hypothesis 2E

The prediction is that children will be more likely to answer correctly in no-object-movement than in object-movement conditions. Two studies (Fabricius et al., 2010, Study 1; Wimmer & Perner, 1983, Study 3) provided six object movement conditions, in two cells of Table 3. Hypothesis 2E was confirmed in the no-highlight/look/leave cell. There were four conditions with object movement and nine without, and a one-way ANCOVA revealed an effect of object movement, $F(1, 9) = 10.22$, $p = .011$, estimated at age 51 months. With no movement, $M_{\text{logit}} = 3.98$, and with movement, $M_{\text{logit}} = 1.07$ ($M = 0.98$ and 0.74, respectively). This finding indicates that object movement elicited more PAR than no movement.

Hypothesis 2E was not confirmed in the highlight/look/return cell. In two conditions with object movement and one without, all children answered correctly. This suggests that highlighting interfered with PAR to the degree that all children were correct regardless of object movement.

One study (Lohmann et al., 2005) included one object-movement condition in the no-highlight/look-first/return cell, but there were no other conditions in that cell for a comparison.

Hypotheses 2A, 2B, and 2C

The five aspectuality task conditions in the no-highlight/look/stay cell, and the four object-movement conditions in the no-highlight/look/leave cell (but not in the other two cells) would lower performance in those cells, and thereby distort the analysis of the three primary conditions (i.e., Hypotheses 2A, 2B, and 2C). Thus, we removed those conditions. Also, as noted above, it is an empirical question whether the leave condition indicates that the situation has changed more so than the stay condition. In preparation for the following analyses, we tested whether stay and leave differed in each of the first three rows of Table 3 (i.e., at each of three levels of highlight × question).

A 3 (levels of highlight × question) × 2 (protagonist movement: stay, leave) ANCOVA revealed an effect of the three levels, $F(2, 45) = 11.21$, $p < .001$, but no effects of protagonist movement or interaction (Fs < 1). Thus, for simplicity, we collapsed stay and leave conditions.

A 2 (highlight) × 2 (question) × 2 (protagonist movement) ANCOVA on the 67 remaining conditions revealed linear, $F(1, 57) = 24.58$, $p < .001$, and quadratic, $F(1, 57) = 22.31$, $p < .001$, effects of age; main effects (estimated at 55 months) of highlight, $F(1, 57) = 9.32$, $p = .003$, and question, $F(1, 57) = 7.46$, $p = .008$; and a significant three-way interaction, $F(1, 57) = 13.06$, $p = .001$. The main effect of highlight confirmed Hypothesis 2B: Children were more likely to answer correctly in highlight ($M_{\text{logit}} = 3.01$) than in no-highlight conditions ($M_{\text{logit}} = 1.40$). This indicates that highlighting interfered with PAR relative to no-highlighting. The main effect of question confirmed Hypothesis 2C: Children were more likely to answer correctly in look ($M_{\text{logit}} = 2.92$) than in look-first conditions ($M_{\text{logit}} = 1.49$). This indicates that look-first questions elicited more PAR than look questions.

Hypothesis 2A (i.e., return should elicit more PAR than stay/leave) was tested in the context of the three-way interaction, illustrated as protagonist movement × question in Figure 2a (with no-highlight), and in Figure 2b (with highlight). Hypothesis 2A was confirmed in Figure 2a among look conditions. The simple effect of protagonist movement (estimated at 54 months) was significant, $F(1, 25) = 5.19$, $p = .032$. Performance was higher in the 24 stay/leave ($M_{\text{logit}} = 2.70$; $M = 0.94$) than in the 5 return conditions ($M_{\text{logit}} = 1.29$; $M = 0.78$). This indicates that return elicited more PAR than stay/leave, when the procedures involved no-highlighting and look questions.

Hypothesis 2A was not confirmed in Figure 2a among look-first conditions. The simple effect of protagonist movement (at 64 months) was not significant, $F(1, 17) = 0.38$, $p = .548$. Performance did not differ between the 20 stay/leave and one return conditions. The overall $M_{\text{logit}} = 0.38$ ($M = 0.59$). This indicates that look-first elicited PAR in stay/leave to a similar degree that it was elicited in return, with no-highlighting.

Hypothesis 2A was also not confirmed in Figure 2b among look conditions. The simple effect of protagonist movement (at 45 months) was not significant, $F(1, 8) = 1.38$, $p = .273$. Performance did not differ between the nine stay/leave and three return conditions. The overall $M_{\text{logit}} = 3.61$ ($M = 0.97$). This indicates that highlighting interfered with PAR in return to the degree that PAR was not elicited any more frequently in return than in stay/leave, when the procedures involved look questions.

Finally, Hypothesis 2A could not be tested in Figure 2b among look-first conditions due to lack of variance. In all four leave conditions (there were no stay conditions), all children answered correctly. This suggests that in leave conditions, highlighting outweighed look-first and interfered with PAR. In the one return condition, 45-month-olds performed well ($M = 0.83$). This might suggest that the combination of protagonist return and look-first outweighed highlighting to some degree and elicited some PAR.

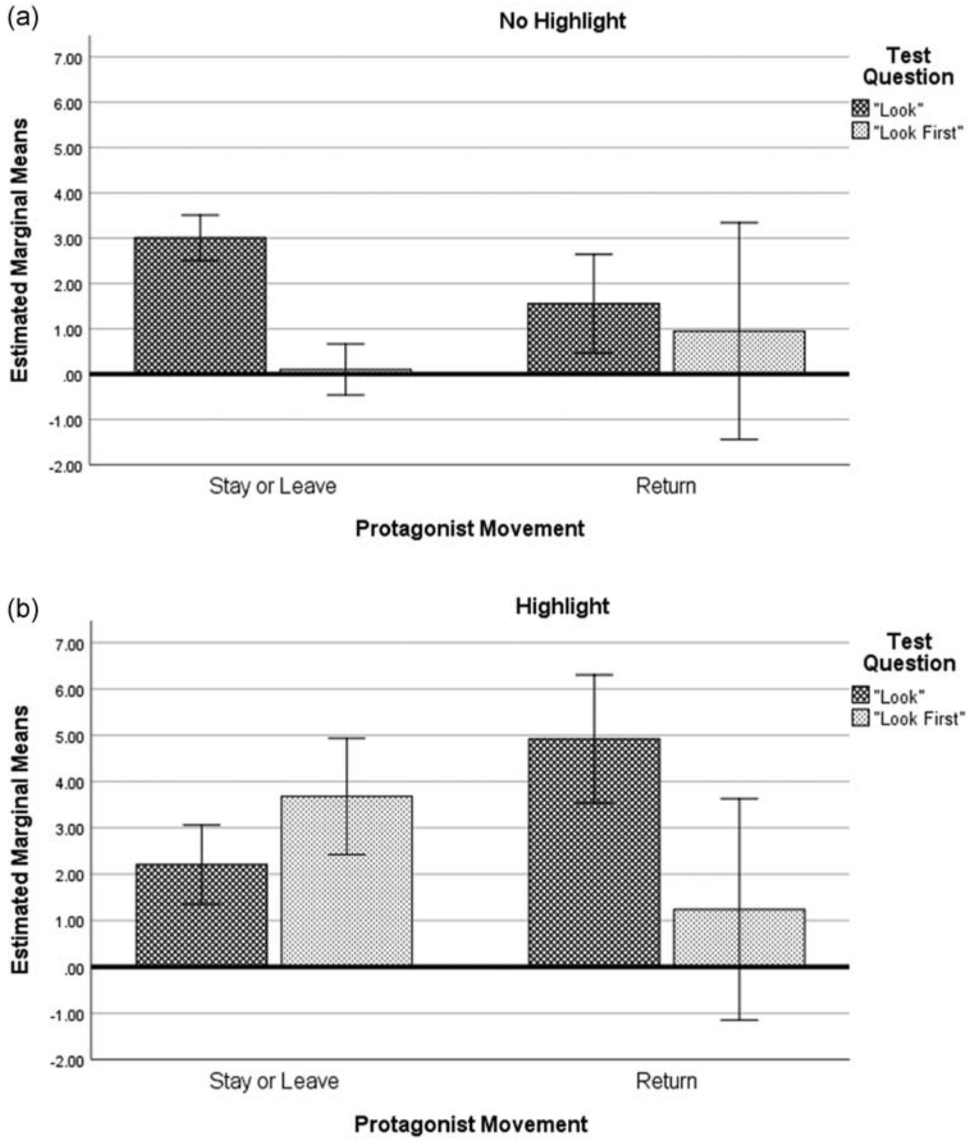

FIGURE 2.—Three-way interaction of protagonist movement × test question with no-highlight (a) and with highlight (b) on mean proportion correct transformed by the logit transformation. Error Bars = 95% CI. CI = confidence interval.

Hypothesis 1A

We tested Hypothesis 1A separately for each question, look and look-first. Separate tests ensured that the U-shaped pattern was not driven by only one type of question. There were similar numbers of conditions that used each question, although look-first conditions spanned a larger age range.

Look Question

The previous analyses revealed that the following 14 conditions in the first row of Table 3 (no-highlight/look) elicited PAR: Aspectuality tasks in stay ($N = 5$), object movement in leave ($n = 4$), and return ($N = 5$). These conditions spanned 40–78 months of age. An ANCOVA with only the linear and quadratic effects of age entered as covariates confirmed Hypothesis 1A. The linear, $F(1, 11) = 9.72$, $p = .010$, and quadratic, $F(1, 11) = 10.00$, $p = .009$, lines provided significant independent fits.

Look-First Question

The previous analyses also revealed that all 21 conditions in the second row of Table 3 (no-highlight/look-first) elicited PAR. These conditions spanned 37–127 months. Hypothesis 1A was also confirmed for these conditions. The linear, $F(1, 18) = 34.34$, $p < .001$, and quadratic, $F(1, 18) = 33.90$, $p < .001$, lines provided significant independent fits.

Hypothesis 1B

The remaining 41 conditions did not elicit PAR. These conditions included stay and leave in no-highlight/look (24), and all of the highlight conditions (17). These conditions spanned 32–83 months. Hypothesis 1B was confirmed for these conditions. The linear, $F(1, 38) = 0.123$, $p = .728$, and quadratic, $F(1, 38) = 0.014$, $p = .906$ did not provide significant fits.

Discussion

True-belief tasks have mostly been used to control for lower-level strategies, and fewer than two studies have appeared per year on average, with few tests of procedural variations. Consequently, the literature cannot support a comprehensive, exploratory meta-analysis to test, and control for, a wide range of procedural variations; true-belief tasks other than location tasks; year of publication; nationality of sample; etc. Instead, this secondary analysis focused on the several theoretical predictions of the PAR theory for true-belief location tasks.

All of the predictions were confirmed. The true-belief location studies published between 1983 and 2017 show a U-shaped developmental pattern (Hypothesis 1), and that pattern is confined to those conditions hypothesized, and found, to elicit PAR (Hypotheses 1A and 1B). The effects of all the conditions were in the predicted directions. The protagonist's return (Hypothesis 2A), and inconsequential object movement in the protagonist's absence (Hypothesis 2E) were both hypothesized to elicit PAR by signaling that the situation had changed. Pesch, et al. (2020) recently also found poor performance in a true-belief return condition among 4½-year-olds

($M = 0.50$) and 5½-year-olds ($M = 0.65$), consistent with the U-shaped pattern. The look-first question (Hypothesis 2C) was hypothesized to elicit PAR via the implicature (Grice, 1989) that the protagonist will get it wrong. Aspectuality tasks (Hypothesis 2D) were hypothesized to elicit PAR in yet another way; namely, even though the protagonist stays in the scene, he sees the object move under one aspect, but the question refers to the other aspect that he did not see move. Reasoning about the aspectuality of beliefs is beyond the capabilities of children who use PAR because it requires understanding that the same object can be mentally represented differently depending on the information one has been granted (e.g., that Superman can be represented as Clark Kent or not; that Maxi's chocolate can be represented as in location A or B; that a Smarties container can be represented as containing Smarties or a pencil; or that a fake rock can be represented as a rock or as a sponge). Finally, highlighting the protagonist's prior perceptual access (Hypothesis 2B) was hypothesized to interfere with PAR about the protagonist's current lack of perceptual access, leaving children to default to their initial conclusion that the protagonist sees and knows, and to answer correctly that he will get it right.

The conditions interacted in ways that were not specifically predicted, but that did not contradict PAR theory. The interactions suggested differential weighing of the conditions. In the studies that highlighted the protagonist's prior perceptual access, children almost always answered correctly. In the studies that did not highlight, performance decreased when the protagonist returned, when the question was look-first, or when there was inconsequential object movement.

The meta-analysis of false belief (Wellman et al., 2001) found that in false-belief location tasks, the only condition to interact with age involved look-first. Past age 4, look-first increasingly improved performance with age relative to look. Wellman and Cross (2001) point out that the effect of look-first does not reveal early belief reasoning, in which case it should boost false-belief performance in younger children more than in older children. Similarly, if look-first elicited a low-level strategy such as reporting where the object was placed first (Rai & Mitchell, 2004; Surian & Leslie, 1999), it should also boost performance in younger children. PAR theory provides an explanation for why look-first should increasingly improve false-belief performance with age; that is, children past age 4 become increasingly likely to use PAR to work out the conversational implicature that the protagonist will get it wrong.

The current findings offer some clarity to recent issues that have arisen in the context of true belief. Regarding protagonist movement, the findings fail to support Oktay-Gür and Rakoczy's (2017) contention that children fail true-belief stay conditions. In the 24 standard, no-highlight/look conditions with no object movement, mean correct performance was estimated at 54 months of age to be 0.94 in stay/leave, and these conditions constituted the majority of the 41 conditions that did not elicit PAR and did not show linear or quadratic effects of age.

Regarding aspectuality, Perner et al. (2015) and Rakoczy et al. (2015) have been engaged in a debate about whether 4- to 6-year-olds' success on aspectuality false-belief location tasks indicates that they understand the aspectuality of belief. The debate hinges on whether children also pass aspectuality true-belief location tasks. By providing like-to-like comparisons of aspectuality and standard conditions in terms of protagonist movement and test question while controlling for age, the results confirm the Perner et al. (2015) finding (see Table 2) that children are less likely to answer correctly in aspectuality true-belief (individuation-present) conditions than in standard true-belief (prediction-present) conditions. The results also reveal that Oktay-Gür and Rakoczy's (2017) contrary findings of no difference are actually due to use of look-first in their Studies 1 and 2, and confounding of aspectuality and protagonist movement in Study 3.

Perner et al.'s (2015) contention is that 5- to 6-year-olds' failure on aspectuality true-belief location tasks means that they have only a localized difficulty reasoning about the aspectuality of belief that does not extend to reasoning about false belief in standard tasks. One problem with that argument is that children's difficulty with true belief is not confined to objects with a dual aspect. A second problem is that belief reasoning is confounded with PAR in false-belief tasks, to which we turn in the next chapter.

III. Three-Option False-Belief Tasks

Maxi puts his chocolate in cupboard A.
Then Maxi watches his mother move it to cupboard B.
Maxi leaves, and his mother moves it to cupboard C.
Maxi returns.
Where will Maxi look first for his chocolate?

In the above example, when Maxi returns, he has a false belief that his chocolate is in cupboard B. The chocolate is really in cupboard C. What is the point of including the third cupboard, A, in the story? At first glance, it might seem to be irrelevant.

Maxi has no reason to believe his chocolate is in A when he comes back. Belief-reasoners would know that Maxi has a false belief that it is in B. Reality-reasoners would say that Maxi will look in C. Who would pick A? PAR-users. They reason that Maxi will get it wrong, and so they can be expected to choose randomly between the two empty cupboards, A and B. The critical addition of the third cupboard allows PAR-users to be distinguished from belief reasoners, but only if care is taken to make sure that the false-belief option (cupboard B) and the irrelevant option (cupboard A) are equally salient. Otherwise, PAR-users could be more likely to pick the false-belief option simply because it stands out more. In this chapter, we show how easy it is to make PAR-users switch between choosing the irrelevant option and the false-belief option.

Introduction

The confound between belief reasoning and PAR in false-belief tasks can be removed by adding an irrelevant option, which the protagonist has no reason to believe. Children who use either approach will avoid the reality option, but those who use PAR will choose randomly between the two wrong options (false-belief and irrelevant), while those who use belief reasoning will choose only the false-belief option. In three studies, Fabricius and Khalil (2003) added irrelevant options to six false-belief tasks: Location; contents; identity involving a sponge (reality) that looked like a rock (false belief) and that a story character, Yogi, had pretended to be his car (irrelevant); representational change versions of contents and identity; and a novel task (plate task) in which the protagonist saw one object (false-belief) on a plate, and then in his absence the child chose to replace the false-belief object with one of two other objects (reality and irrelevant). In each task, the experimenter asked a yes-or-no test question about each option (e.g., "Will she think it is a sponge?" "… a rock?" "… Yogi's car?").

Performance was similar on all the tasks. On average, .61 of the 132 5-year-olds avoided the reality option in the 3-option tasks, and .64 passed the standard 2-option false-belief tasks that were also administered. However, only about half of the .61 who avoided the reality option (e.g., sponge) responded in accord with belief reasoning by endorsing only the false-belief option (rock). The rest said yes to only the irrelevant (Yogi's car) option, or to both the irrelevant and false-belief options, or to neither, all of which are plausible patterns for PAR. On average, only .20 of 5-year-olds passed both or failed only one of four or five 3-option tasks. Only when viewed through the lens of PAR theory, does it make sense that children who pass 2-option false-belief tasks would also say that someone who sees a fake rock will think it is Yogi's car, and that someone who sees a real rock will think it is a sponge (Chapter II). Notably, children did no better reporting their own prior false beliefs in the representational change tasks. It also did not make a difference if the 3-option tasks were presented before or after the 2-option tasks; if children were questioned about the false-belief and reality options before the irrelevant option was introduced; or if they were questioned about the false-belief option first, second, or third.

In contrast, Perner and Horn (2003) found that 4½-year-olds seldom, if ever, chose the irrelevant option. They asked children a single open-ended test question (e.g., "What will she think it is?") in 3-option location, contents, and novel neutral box (similar to the plate task described above) tasks. It is possible that a single open-ended question might not ensure that children who use PAR give equal consideration to the false-belief and irrelevant options. Inadvertent salience differences between false-belief and irrelevant options could bias children who use PAR toward one or the other. Salience differences could result from superficial procedural differences. Perner and Horn used only one version of each of their three tasks, and thus it is unknown whether their procedures might have inadvertently given greater salience to the false-belief option than the irrelevant option.

In this chapter, we report all seven replication studies that we ran using Perner and Horn's tasks and their single open-ended test question. Across the seven studies, we varied superficial features of the tasks unsystematically, much like what might happen if different labs devised their own versions of each 3-option task.

Children who use PAR do not distinguish between false-belief and irrelevant options; thus, PAR theory entails that with a single question, superficial procedural variations across tasks can result in substantial variation across tasks in the proportion of 4½-year-olds who choose the false-belief option among the subset who avoid the reality option. Children who use PAR will avoid the reality option, so the theory further entails that these same superficial procedural variations will not affect choices of the reality option.

Three predictions follow: (1) Perner and Horn found that among the 4½-year-olds who avoided the reality option, .90 of them chose the false-belief option over the irrelevant option. The first prediction is that the rate of .90 will not replicate when varying superficial task procedures. (2) Perner and Horn asked a look-first question in their location task, and found that the proportion of children who avoided the reality option was higher in their location task (.75) than in their typical box (.43) and neutral box (.38) tasks. The task effect was likely due the look-first question, which is known to elicit better performance than a look question in location tasks (Wellman et al., 2001). We also used a look-first question in our location tasks. Thus, our second prediction was that the task effect on avoiding the reality option should replicate. (3) Choices of the false-belief option over the irrelevant option among those who avoid the reality option should be independent across tasks.

Perner and Horn's studies likely had low power (Ns = 21 and 23). Thus, their findings might not replicate in a same-age, same-N replication study simply because of low power. We do not rely on counting how many of our replication studies found a higher proportion than chance (.50) of children who choose the false-belief option among those who avoid the reality option. Braver et al. (2014) showed via simulations how low power renders such a strategy misleading, because the probability of achieving significance is equal to power, and with low power, even a true effect will often fail to achieve $p < .05$.

Perner and Horn argued from the overall pattern of their results. We followed their lead by using a slight variation of standard meta-analysis, the *continuously cumulating meta-analytic approach* (CCMA; Braver et al., 2014). In CCMA, one combines the data from replication studies to increase power and to test for heterogeneity of effect sizes across the studies. In our analyses of the three tasks, significant heterogeneity in the form of study effects on the proportion of children who chose the false-belief option among the subset who avoided the reality option, combined with a wide range of observed proportions, would be strong evidence that the original findings do not replicate. If, instead, the differences across studies are descriptively small and statistically nonsignificant, and the pooled proportion is on the order of the pooled proportion that Perner and Horn observed (i.e., .90), that would be strong evidence that the original findings do replicate. We pooled the data from our seven replication studies and from Perner and Horn's two studies. Braver et al. showed that with seven replication attempts, Ns of 25 per study, and moderate effect size, the CCMA approach provides ample power to detect a real effect.

Method

Participants

Information about participants' age and gender in each study is provided in Table 4. The following information about recruitment applies also to the

TABLE 4

Number of Children Who Chose Each Option

Study and Tasks	Participants: N (# Boys), Age M and (Range) in Years; Months	Two-Option Tasks		Three-Option Tasks		
		False Belief	Reality	False Belief	Irrelevant	Reality
Perner and Horn (2003) (Experiment 1, Session 2)	$N = 23$ (10), $M = 4;8$ (4;1–5;1)					
Location		18	5	19	0	4
Perner and Horn (2003) (Experiment 2)	$N = 21$ (9), $M =$ * (4:1–5:0)					
Location		14	7	13	1	7
Typical box		8	13	6	3	12
Neutral box		8	13	8	0	13
Study 1	$N = 21$ (11), $M = 4:6$ (4:1–4:11)					
Location		12	9	9	3	9
Typical box		12	9	4	5	12
Neutral box		8	13	7	0	14
Study 2	$N = 21$ (9), $M = 4:5$ (4:0–4:11)					
Location		11	10	10	1	10
Typical box		12 (12)[a]	9 (9)	2 (2)	6 (6)	13 (13)
Neutral box		9	12	5	4	12
Study 3	$N = 26$ (10), $M = 4:4$ (4:1–5:0)					
Location		19	6	13	7	5
Typical box				5 (3)	3 (2)	9 (7)
Neutral box				11	0	12
Study 4	$N = 21$ (7), $M = 4:6$ (4:0–5:0)					
Location		11	5	7	5	3
Typical box				3 (3)	6 (5)	10 (8)
Neutral box				10	0	8

(Continued)

TABLE 4. (*Continued*)

Study and Tasks	Participants: N (# Boys), Age M and (Range) in Years; Months	Two-Option Tasks		Three-Option Tasks		
		False Belief	Reality	False Belief	Irrelevant	Reality
Study 5	$N=25$ (14), $M=4{:}4$ (4:0–4:11)					
Location		13	11	1	13	11
Typical box				0	5 (5)	16 (13)
Neutral box				5	0	16
Study 6	$N=19$ (8), $M=4{:}6$ (3:9–5:2)					
Location				10	2	6
Neutral box				6	4	9
Study 7	$N=33$ (19), $M=4{:}5$ (3:9–5:1)					
Location				6	18	9
Typical box		20 (17)	13 (8)	13 (13)	3 (2)	14 (14)
Neutral box				16	1	13

Note. Empty cells indicate tasks that were not included in the study.
*Ages not reported.
[a]Numbers in parentheses are the children who passed the memory control question in our adaptations of Wellman and Liu's (2004) contents false-belief task procedures.

studies reported in Chapters IV–VIII, and will not be repeated in those chapters. Children were recruited from university-sponsored preschool and daycare centers in a large southwestern United States metropolitan area. All parents whose children were in the targeted age range were invited, via letters distributed by school or center staff, to give consent for their children to participate. The centers traditionally have high rates of parent consent. All children whose parents consented were invited to participate, and all were native English-speakers. Sociometric data were not collected. Procedures were approved by the university Institutional Review Board (IRB).

For Study 7 only, IRB approval was also obtained for posting all the video recordings of the data collection on Databrary (Fabricius, 2014).

Procedures

Our Study 1 was designed as an exact replication of Experiment 2 of Perner and Horn (2003). The reason why Experiment 1 was not replicated is that it included only location tasks, which were identical to those in Experiment 2. In both of our Studies 1 and 2, children were assessed in two sessions one week apart by the same experimenter, who was not blind to the hypotheses. In each session there were three tasks (either the 2-option or the 3-option version of each task) presented in random order. In our Studies 3–7, children were assessed in one session by research assistants who were blind to the hypotheses, and the tasks were given in random orders. In all studies, if children failed a control question, they were reminded of the information and re-asked the question until they answered correctly. If experimenters failed to ask or re-ask a control question, or if the response to the test question was "I don't know," idiosyncratic, or no response, the child's response to the test question on that task was scored as missing.

Simplified versions of the procedural variations across studies, blocked by tasks, are presented below. Task scripts for each study, including the control questions that children were asked, are provided in Supporting Information, *Chapter III: Task Scripts in each Study*.

2-Option Location Tasks

Perner and Horn; Studies 1, 2, and 5. Protagonist puts the object in the false-belief location and leaves. The second character enters and moves the object to the reality location and leaves. Control questions are asked. Protagonist returns and child is asked where protagonist will look first.

Studies 3 and 4. Same as Study 1, except that the child is reminded of the story events before control questions are asked.

3-Option Location Tasks

Perner and Horn; Study 1. Protagonist puts the object in the false-belief location and leaves. The second character enters and moves it to the irrelevant location, chosen by the child. The second character changes her mind, moves it to the reality location, and leaves. Control questions are asked. Protagonist returns, and child is asked where he will look first.

Study 2. Same as Study 1, except that protagonist asks the second character to put the object away, she chooses the false-belief location, protagonist watches her, and leaves; the second character removes the object, and child moves it to the irrelevant location.

Study 3. Protagonist finds the object in the irrelevant location, removes it, puts it in the false-belief location, and leaves. The second character enters and moves it to the reality location. The child is reminded of the story events. Control questions are asked. Child is asked where protagonist will look first when he returns.

Studies 4 and 7. Same as Study 3, except that protagonist watches the second character put the object in the irrelevant location, and then watches her move it to the false-belief location, and leaves.

Study 5. Same as Study 4 without the reminder of the story events.

Study 6. Same as Study 2, except that the second character moves the object to the irrelevant location, and the child is reminded of the story events. Control questions are asked.

2-Option Typical Box Tasks

Perner and Horn; Study 1. Child is shown a Whoppers candy box, and asked what he or she thinks is inside. The second character enters and shows the child that the reality object is inside. Child is given the reality object to hold for a moment (an inadvertent departure from Perner and Horn's procedure). The second character replaces the reality object and leaves. Control questions are asked. Newcomer enters. The child is asked what newcomer thinks is in the box.

Studies 2 and 7. We used Wellman and Liu's (2004) contents false-belief task with an open-ended test question in Study 2, but, inadvertently, we asked their forced-choice question in Study 7. Child is shown a Whoppers box, asked what he or she thinks is inside, and is shown that the reality object is inside. Newcomer enters. The child is asked what newcomer thinks is in the box. Control question is asked.

3-Option Typical Box Tasks

Perner and Horn; Study 1. Child is shown a Band-Aids box, and asked what he or she thinks is inside. The second character enters and shows the child that the irrelevant object is inside. Child is given the irrelevant object to hold for a moment (an inadvertent departure from Perner and Horn's procedure). The experimenter produces the reality object, and the second character puts it in and leaves. The irrelevant object is removed. Control questions are asked. Newcomer enters. The child is asked what newcomer thinks is in the box.

Studies 2, 3, and 7. We used Wellman and Liu's (2004) task modified for three options. The child removes the irrelevant object from the Band-Aid box, and puts in the reality object. Protagonist enters and the task proceeds as in the 2-option version.

Study 4. Same as Study 2, except that the child is reminded of the story events. Control questions are asked.

Study 5. Same as Study 4 without the reminder of the story events.

2-Option Neutral Box Tasks

Perner and Horn; Study 1. Protagonist puts the false-belief object in a nondescript box and leaves. The second character enters and removes the false-belief object. The experimenter produces the reality object and the second character puts it in and leaves. The false-belief object is removed. Control questions are asked. Protagonist returns. The child is asked what protagonist thinks is in the box.

Study 2. Two objects are displayed, and child names them. The child chooses the false-belief object, names it, and puts it in the box while protagonist watches. Protagonist leaves. The second character enters, removes the false-belief object, puts in the reality object, and leaves. The false-belief object is removed. Control questions are asked. Protagonist returns. The child is asked what protagonist thinks is in the box.

3-Option Neutral Box Tasks

Perner and Horn; Study 1. Protagonist puts the false-belief object in a nondescript box and leaves. The second character enters and removes the false-belief object. Two new objects are produced. Child chooses the irrelevant object, and the second character puts it in. The second character changes her mind, removes the irrelevant object, puts the reality object in, and leaves. The false-belief object and the irrelevant object are removed. Control questions are asked. Protagonist returns. The child is asked what protagonist thinks is in the box.

Study 2. Three objects are displayed, and child names them. The child chooses the false-belief object, names it, and puts it in the box while protagonist watches. Protagonist leaves. The second character enters, removes the false-belief object, and wants to put one of the remaining objects in the box. The child chooses the irrelevant object, names it, and puts it in. The second character changes her mind, removes the irrelevant object, puts the reality object in, and leaves. The false-belief object and the irrelevant object are removed. Control questions are asked. Protagonist returns. The child is asked what protagonist thinks is in the box.

Study 3. Same as Study 1 except the child names the false-belief object, and gives it to protagonist; the child names the two new objects; the child names the irrelevant object after choosing it; the second character puts it in; children are reminded of the story events before control questions are asked; the false-belief object and the irrelevant object are removed.

Study 4. Same as Study 2 except children are reminded of the story events before control questions are asked.

Study 5. Three objects are displayed, and the child names them. The child chooses the irrelevant object, names it, and protagonist watches the child put it in the box. Protagonist changes his mind and removes the irrelevant object. The child chooses the false-belief object, names it, and protagonist watches child put it in the box. Protagonist leaves. The second character enters, removes the false-belief object, puts the reality object in, and leaves. The false-belief and the irrelevant objects are removed. Control questions are asked. Protagonist returns. Children are asked what protagonist thinks is in the box

Study 6. Same as Study 5 except that protagonist does not change his mind; the second character enters, removes the irrelevant object, and wants to put one of the remaining objects in the box; the second character changes her mind after protagonist leaves; and children are reminded of the story before the control questions are asked.

Study 7. Same as Study 2, except that the false-belief and the irrelevant objects are removed after the control questions are asked.

Results

Children initially answered .82 to .98 of the control questions correctly in each 3-option task in each study, except in the Neutral Box task in Study 5 (.67) and Study 7 (.70). Table 4 shows the number of children who chose each option. We show only the Session 2 data from Perner and Horn's Experiment 1 because we did not have the within-participant data. The parentheses show the number of children in the typical box tasks who passed the memory control question from Wellman and Liu's (2004) contents false-belief task. Results did not differ if we excluded those who failed the memory control question, so we included them.

We used logistic, multilevel, random-effects models for each task to test for study effects. Random-effects models, rather than fixed-effects, are appropriate because, with the exception of Study 1, our studies were not direct replications, and the results from previous studies did not determine the procedural changes in subsequent studies. Random-effects models are less powerful, and thus work against finding study effects. We used M-Plus version 7.4 (Muthén & Muthén, 2010) with a restricted maximum likelihood (MLR) estimator. We conducted likelihood ratio tests on hierarchal models in which the baseline model was single-level, intercept-only, and the alternative model was two-level with a variance estimate for random intercepts for study. Significant Level 2 (i.e., study-level) variance and a large interclass-correction (ICC) would indicate that the studies differ, and that the effect does not replicate.

The first prediction was that the pooled proportion of children that Perner and Horn observed (i.e., .90) who chose false-belief over irrelevant among those who avoided reality would not replicate across studies with superficial procedural differences. The prediction was supported. There were significant study effects in all three tasks. For children who avoided the reality option, we coded whether they chose the false-belief (coded 1) or the irrelevant option (0). The random-intercepts models significantly improved fit over the baseline single-level models: Location, $\chi^2(1) = 39.73$, $p < .001$, $ICC = .50$; typical box, $\chi^2(1) = 3.84$, $p = .050$, $ICC = .19$; neutral box $= \chi^2(1) = 7.48$, $p = .006$, $ICC = .56$. The pooled proportions were: Location = .64 (range = 0.07–1.00), typical box = .52 (0.00–0.81), and neutral box = .88 (56–1.00).

The second prediction was that the task effect on avoiding the reality option that Perner and Horn observed would replicate. This prediction was also supported. There were no significant study effects in any task on avoiding the reality option. We coded whether children avoided the reality option by choosing either the false-belief or the irrelevant options (coded 1), or choosing the reality option (0). The random-intercepts models did not improve fit over the baseline single-level models: Location, $\chi^2(1) < .01$, $p = .964$, $ICC < .01$; typical box, $\chi^2(1) < .01$, $p = .984$, $ICC < .01$; neutral box, $\chi^2(1) = .03$, $p = .867$, $ICC = .01$. The pooled proportions were: Location = .68 (range = 0.83–0.52), typical box = 0.43 (0.53–0.24), and neutral box = 0.44 (0.56–0.24). The nearly nonoverlapping ranges of pooled proportions of avoiding the reality option between the location and the two box tasks strongly replicates the task effect in Perner and Horn. The third prediction was that among the children who avoided the reality option, choices of the false-belief option over the irrelevant option should be independent across tasks. This prediction was supported in each pair of tasks. Table 5 shows the number of children with each pattern of choices of the irrelevant versus the false-belief options in each pair of 3-option tasks, summed over study. Choices of the false-belief option were not significantly related across location and typical box ($N = 45$, Fisher's exact $p = .376$); location and neutral box ($N = 55$, $p = .416$); and typical and neutral box ($N = 36$, $p = .492$).

In contrast, there was significant consistency in avoiding the reality option between each pair of 3-option tasks: location and typical box ($N = 125$, $p < .001$); location and neutral box ($N = 147$, $p = .001$); and typical and neutral box ($N = 119$, $p < .001$). The pooled proportion of children in each pair of tasks who avoided the reality option in both or neither were .63, .62, and .76, respectively.

Discussion

The findings supported all three predictions derived from PAR theory. First, we did not replicate the original finding (Perner & Horn, 2003) that .90 of 4½-year-olds who avoided the reality option chose the false-belief option. Instead, the proportions ranged from 0.00 to 1.00 when superficial procedures were varied across seven replication studies, reflecting significant study effects. Second, we replicated the original finding that more children avoided the reality option in the location task than in the box tasks. This finding showed that the effect of the superficial procedural variations was confined to the predicted variable. Third, choices of the false-belief option versus the irrelevant option were not significantly related across all pairs of tasks. The current findings, with a single open-ended test question, combined with Fabricius and Khalil's (2003) findings, with a separate test question for each option, provide a successful multimethod test of PAR theory.

In the current studies, the mean proportions of children who chose the false-belief option among those who avoided the reality option in the location and typical box tasks were .64 and .52, respectively. Neither is close to the original finding of .90. The proportion of false-belief option choices in the typical box condition was at chance (.50), even though the false-belief option has inherent salience; namely, the picture on the box.

TABLE 5

Number of 4½-Year-Olds in Each Pattern of Choices of Irrelevant and False-Belief Options in 3-Option False-Belief Tasks

	Location Task			Location Task			Typical Box Task	
Typical Box Task	Irrelevant	False-Belief	Neutral Box Task	Irrelevant	False-Belief	Neutral Box Task	Irrelevant	False-Belief
Irrelevant	9	14	Irrelevant	1	6	Irrelevant	0	2
False-belief	12	10	False-belief	16	32	False-belief	16	18

Study-level effects would normally prompt researchers to search for an explanation (Braver et al., 2014), but in this case the variation is predicted to be due to superficial salience differences between the false-belief and the irrelevant options that are not theoretically relevant. The unsystematic way that we varied superficial procedural features makes it unlikely that explanations will be apparent, with one possible exception. In the location tasks, children were more likely to choose the false-belief option when the sequence in which the object was placed in the false-belief (FB), irrelevant (I), and reality (R) locations was FB-I-R than I-FB-R. In the five FB-I-R studies, the proportion of children who chose the false-belief option among those who avoided the reality option was .90 (range = 1.00–0.75). In the four I-FB-R studies, the proportion was .39 (0.65–0.07). This difference could be explained by the look-first test question prompting children to not only avoid the reality location but also choose the location where the object was placed first. Pesch et al. (2020) recently found similar results using 3-option false-belief tasks. In their location task, they used the I-FB-R order, and the proportion of 4½-year-olds who chose the false-belief option among those who avoided the reality option was .32.

We could find no salience explanations for the study-level effect in the typical box tasks. Few superficial procedural variations were even possible within the confines of the standard task. The effect was weaker, giving a bit more assurance that the mean (.52) represents the true state of affairs, and that the variation might largely reflect random fluctuations and, in the case of Study 7, perhaps regression to the mean. In their 3-option contents task, Pesch et al., (2020) found a higher proportion (.76) than we did of 4½-year-olds who chose the false-belief option among those who avoided the reality option. It might have been difficult for children to remember the irrelevant option object in Pesch et al.'s contents task (which included both 3-option and 4-option versions), as suggested by their finding that working memory was associated only with performance on the 3-option contents task.

The findings for the novel neutral box task were mixed. The significant study-level effect coupled with the chance-level performance in Studies 2 and 6 indicates failure to replicate the original finding. However, performance in the other six studies was essentially perfect. In Studies 2 and 6, the protagonist might have been equally associated with the false-belief object and the irrelevant object because all three objects were initially on the table, and the protagonist only watched as the false-belief object was put in the box. In the other six stories, the protagonist might have been more strongly associated with the false-belief object than the irrelevant object in two different ways: Only the false-belief object was initially on the table, and the protagonist put it in the box (Perner & Horn, Study 1, and Study 3), or the experimenter talked about the protagonist watching the false-belief object (Study 4, during reminding the child of the story; Study 7, during control questions with the false-belief object remaining on the table; and Study 5, during the protagonist changing his mind about the irrelevant object).

IV. Understanding Ignorance and Understanding False Belief

Maxi puts his chocolate in cupboard A.
Then Maxi watches his mother move it to cupboard B.
Maxi leaves, and his mother moves it to cupboard C.
Maxi returns.
Does Maxi know where his chocolate is?
Where does Maxi think his chocolate is?

Around 4 years of age, children make a curious error. They will say that Maxi does not know where his chocolate is, but then say that he will look in the right place for it. According to PAR theory, the curious error results from children using PAR Rule 1 ("Maxi doesn't see, so he won't know.") before they incorporate PAR Rule 2 ("Maxi doesn't know, so he will get it wrong."). According to PAR theory, the lag between the rules shows that the rules are, in fact, separate concepts that children need to acquire and coordinate. The theory predicts that there should be a second lag, between coordinating the two PAR rules and the later development of belief reasoning. In this chapter, we tested for that second lag. We used 3-option false-belief tasks with two questions, like the example above. The first question identifies the children who use PAR Rule 1. The second question identifies the children who use belief reasoning.

Introduction

A long-standing consensus in the ToM literature is that once children understand ignorance, it takes only a short time before they understand false belief. The consensus is based on consistent findings from studies using 2-option false-belief tasks (Flavell, Flavell, et al. 1990; Friedman et al., 2003; Hogrefe et al., 1986; Roth & Leslie, 1998; Sullivan & Winner, 1991; 1993; Surian & Leslie, 1999; Wellman & Liu, 2004). A short lag would contradict the PAR theory that representational ToM is acquired in middle childhood, which entails a substantial and meaningful lag between correctly attributing ignorance via Rule 1 of PAR (i.e., not seeing leads to not knowing) and understanding false belief.

All of the previous studies cited in the paragraph above used 2-option false-belief tasks to assess the lag between children's understanding of ignorance and their understanding of false belief. Each child is asked two questions. The first question that children are asked is the measure of understanding ignorance, and it is called the knowledge question (e.g., “Does Maxi know where his chocolate is?”). The second question is the measure of understanding false belief, and it is the standard belief question (e.g., “Where does Maxi think his chocolate is?”). Wellman and Liu (2004) compiled the findings from all of the 22 previous studies that had included knowledge and belief questions. For each study, they calculated a difference score, which was the proportion of children who passed the knowledge question, minus the proportion who passed the belief question. A positive difference score would indicate a developmental lag between understanding ignorance and understanding false belief; a difference score of zero would indicate no lag; and a negative difference score would indicate a lag in the opposite direction, suggesting that children understood false belief before ignorance. Wellman and Liu found that across the 22 studies, the difference scores ranged from −.19 to .44. The mean of the difference scores was .15, which was reliably above zero, and which reflected a small developmental lag in passing the knowledge question before the belief question.

From the perspective of PAR theory, the confound between PAR and belief reasoning in 2-option false-belief tasks means that the above mean difference score of .15 cannot be assumed to reflect the lag in understanding ignorance before false belief; it could as well indicate a lag between using Rule 1 of PAR and Rule 2 of PAR (i.e., not knowing leads to acting incorrectly). Wellman and Liu (2004) reported that all but one of the previous studies included either 3½- or 4½-year-olds, which is when a lag between Rule 1 and Rule 2 would most likely occur.

Assessing the lag between children’s understanding of ignorance and understanding of false belief requires a belief question that can identify the children who use belief reasoning. PAR theory predicts that when the belief question requires children to understand false belief and not simply to use PAR, then the difference between the proportion of children who pass the knowledge question and the proportion who pass the belief question will be substantially larger than .15.

It is important to begin by noting that in the current chapter, we present only one study. In Chapter II, our conclusions came from analyses of the true-belief literature, and in Chapter III, our conclusions came from seven replication studies. With only one study in the current chapter, however, we are very cautious about offering conclusions. Even if we find a difference score substantially larger than .15, and outside the range of previous difference scores, our findings could be an outlier. In our view, the most important contributions that the current chapter makes on its own are to show how PAR theory applies to the question at hand, to demonstrate a method that can be used to test the theory’s predictions, and to stimulate the future studies that are needed to test whether the current findings replicate. The remaining chapters also present only one study each, and so the same caveat applies to their individual contributions as well.

To return to the question at hand, constructing the belief question presents a challenge in meeting two objectives: The question needs to disentangle PAR and belief reasoning, while being similar in linguistic complexity and measurement sensitivity to the knowledge question. To meet the two objectives, we used 3-option versions of contents, identity, and location tasks. We first asked a knowledge question about the reality option (e.g., "When she comes back, will she know it is in the yellow box?"), followed by a belief question about the remaining false-belief and irrelevant options (e.g., "When she comes back, will she think it is in the orange box or the purple box?"). The knowledge and belief questions have two response options each; thus, the likelihood of guessing correctly was .50 for each question. We note here that Wellman and Liu (2004) coded the 3-option tasks of Fabricius and Khalil (2003) as 2-option tasks, by using only the reality and false-belief questions. Their coding inadvertently re-introduced the confound, because on the belief question, children who use PAR will sometimes choose the false-belief option by chance. The criterion we used in the current chapter to identify the children who were likely using belief reasoning was answering the belief question correctly in all three of the tasks we used. Thus, we compared the proportion of children who answered the knowledge question correctly in all three tasks to the proportion who answered the belief question correctly in all three tasks.

Answering three questions correctly meant that we assessed consistent understanding of ignorance and consistent understanding of false belief, rather than the earliest understanding. The counter-PAR argument could be that even if a larger difference is found between the knowledge questions and the belief questions, it might only mean that there is a longer lag in achieving consistent understanding of false belief than consistent understanding of ignorance, and that the earliest onsets of the two concepts could still be close in time. The false belief meta-analysis (Wellman et al., 2001, p. 675) weakens this counter-argument because:

Children are quite consistent on multiple false-belief tasks of the same type … on average, children gave identical responses to two or more similar false belief trials 84% of the time. Consistency was correlated with age … but even in conditions in which children's mean age was less than 44 months … the mean [consistency] was .81.

The counter-argument is further weakened because the mean age of our participants was 60 months, and as noted above, older children are more consistent than are younger children. The counter-argument could be modified to argue that consistency will be hampered when the task presents three options, but in the current study both the knowledge and the belief questions are presented in the same 3-option tasks, and so that would not account for a substantially larger difference. Thus, the counter-argument would need to be that consistent belief reasoning is more difficult to achieve when the choice is between false-belief and irrelevant options (e.g., Band-Aids and key), than when, in the 2-option tasks, it is between false-belief and reality options (Band-Aids and pencil). But the difficulty should be the other way around due to reality reasoning. The more difficult choice should be between false-belief and reality options.

It is important that the false-belief and irrelevant options are made as equally salient as possible. The forced-choice belief question helped ensure that children considered both options before answering. Furthermore, in the contents and identity tasks, as described below, the child had both options in view when the test questions were asked.

If children were pulled by reality to step outside the bounds of the forced-choice belief question (e.g., "Will she think it is in the orange box or purple box?") and chose the reality option ("She'll think it's in the yellow box."), the experimenter accepted and recorded those reality responses. Other children might have wanted to choose the reality option but might have been dissuaded by the forced-choice belief question. Those children would presumably choose randomly between false-belief and irrelevant, and thus not be likely to be mis-classified as belief reasoners.

Method

Participants

Participants included forty 5-year-olds (54–66 months; $M = 60$; 17 boys).

Procedures

In all tasks, if children answered any pretest control questions incorrectly, the story was repeated once, and the questions were asked a second time. All retained children answered correctly either initially or after the single repetition. Four additional children were not retained because they repeatedly failed one or more pretest control questions. On the 2-option and the 3-option location tasks, the control questions were asked once again at the end of the task. These posttest control questions ensured that children had retained the story narratives while answering the test questions. The tasks were presented in four random orders, with the constraint that the 2-option and 3-option location tasks were separated by at least one other task. Specific stories and questions used in each of the tasks follow.

3-Option False-Belief Contents Task

The child was shown a candy box that contained a pencil, and watched as the pencil was removed and replaced by stickers. The pencil remained on the table while the test questions were asked, but was screened by a barrier so that the newcomer (Elmo) could not see it. The placement of the screen allowed the child to see the pencil (irrelevant) and the picture of candy (false-belief) on the box while the test questions were asked. Control questions were "What kind of box is it?" "What was inside the box first?" and "What is inside the box now?" The knowledge question was, "If Elmo stood right there and just looked at the box, would he know it's stickers in the box?" The belief question was, "If he stood right there and just looked at the box, what would he think is in the box, candy or a pencil (counterbalanced)?"

3-Option False-Belief Identity Task

The child was shown and allowed to manipulate a sponge that was painted to look like a rock. A character (Yogi) was shown riding the fake rock and pretending that it was his car. The experimenter briefly moved Yogi and the fake rock around the table, accompanied by car noises. The experimenter left Yogi sitting on the fake rock, stationary and silent, while the test questions were asked, which allowed the child to see a reminder of the pretend car (irrelevant) and the rock-like appearance (false-belief) of the object. The newcomer (Big Bird) Control questions were "What is it really?" and "What does Yogi like to pretend this is?" The knowledge question was, "If Big Bird just looked at it, and didn't touch it, would he know it's a sponge?" The belief question was, "If he just looked at it, and didn't touch it, what would he think Yogi is sitting on, on a rock or on his car (counterbalanced)?"

3-Option False-Belief Location Task

Three different-colored boxes were arranged in a semi-circle in front of the child. A doll (Anna) was portrayed as playing with her toy. After she finished playing with her toy, she put it in the irrelevant box (left side or right side counterbalanced) and returned to the center of the array. Anna then retrieved the toy and played with it again. When she was finished this time, she put it in the false-belief box (center) and left the scene. After she left, her father entered and moved it to the reality box. Control questions were "Where did Anna put the toy first?" "Where did Anna put the toy next?" and "Where did dad move it to?" The knowledge question was "When Anna comes back, will she know it is in [reality]?" The belief question was "When she comes back, will she think it is in [false-belief] or [irrelevant] (counterbalanced)?"

2-Option False-Belief Location Task

Children watched Maxi put his chocolate in the red cupboard and leave. In his absence, his mother moved it to the blue cupboard. Control questions were "Where did Maxi put the chocolate?" "Where did Maxi's mom move the chocolate?" and "Did Maxi see her put it there?" The belief question was, "When he comes back, will he think his chocolate is in the red cupboard or the blue cupboard (counterbalanced)?

Results

On the pretest control questions in all tasks, at least .90 of children answered each question correctly the first time it was asked. On the posttest control questions in the location tasks, at least .97 answered each question correctly.

In Table 6, the first column shows that .78 (31/40) of 5-year-olds passed the 2-option false-belief task, which is typical for that age (Wellman et al., 2001). Also shown are the mean proportion of correct answers to the knowledge questions in the three 3-option tasks, and the mean proportions of choices of the three options in response to the belief questions in the three 3-option tasks. In each 3-option task, the proportion of children who chose the false-belief option was similar to the overall mean: Contents (.60), identity (.68), and location (.65).

To calculate the difference score between understanding ignorance and understanding false belief, we determined the proportion of children who answered all three knowledge questions correctly, .80 (32/40), and subtracted the proportion who answered all three belief questions correctly, .33 (13/40). The difference score equaled .47, which is substantially larger than the mean difference score (.15), and outside the range (−0.19 to 0.44) of difference scores found in the previous studies in which belief reasoning was confounded with PAR (Wellman & Liu, 2004).

TABLE 6

Proportion of 5-Year-Olds who Passed the 2-Option Task, Mean (*SD*) Proportion of Correct Answers to the Three Knowledge Questions, Mean (*SD*) Proportion of Responses to the Three Belief Questions, and Four Patterns of Performance

		Pattern			
False-Belief Task	All Participants ($N = 40$)	Belief Reasoning ($N = 12$)	PAR ($N = 9$)	Consistent Rule 1, Inconsistent Rule 2 ($N = 11$)	Inconsistent Rule 1 ($N = 8$)
2-Option	.78	1.00	1.00	0.45	.63
3-Option					
Knowledge question	.93 (.16)	1.00	1.00	1.00	.63
Belief question					
False-belief option	.64 (.31)	1.00	0.59	0.36	.54
Irrelevant option	.24 (.26)	0.00	0.41	0.40	.21
Reality option	.12 (.19)	0.00	0.00	0.24	.25

Note. PAR = perceptual access reasoning.

The purpose of this study was to assess the lag between understanding the two concepts at a group level, not to reliably classify individuals who used belief reasoning versus PAR. That would require more than three tasks. Nevertheless, we explored the data to attempt to identify patterns of performance. Twelve children fit the pattern of consistent belief reasoning, and their data are shown in the second column in Table 6. They answered all three knowledge questions correctly, passed the 2-option task, and chose the false-belief option on all three tasks.

Nine children fit the pattern of consistent PAR (third column in Table 6). They answered all three knowledge questions correctly, passed the 2-option task, chose false-belief on two or fewer of the three tasks, and never chose the reality option.

Eleven children fit the pattern of consistent use of Rule 1, and inconsistent use of Rule 2 (fourth column in Table 6). They answered all three knowledge questions correctly, and each of them either failed the 2-option task, or chose the reality option once or twice in response to the three belief questions.

The remaining eight children showed inconsistent use of Rule 1 (fifth column in Table 6). All failed one or two knowledge questions.

Finally, we checked to see if our efforts to make the false-belief and irrelevant options equally salient seemed to have succeeded or not. If the options were equally salient, then the children who did not fit the consistent belief reasoning pattern (i.e., the children in columns 3 through 5 in Table 6) should choose randomly between the false-belief and irrelevant options. The mean rates of choices of false-belief (.49, $SD = .23$) and irrelevant (.35, $SD = .25$) did not differ significantly, $t(27) = 1.759$, $p = .090$, although the balance was tilted toward false-belief. That could indicate some inconsistent belief reasoning, or it could result from small salience differences affecting the forced-choice between false-belief and irrelevant options.

Discussion

Our findings revealed a difference of .47 at 5-years of age between the proportion of children who answered all three knowledge questions correctly and thus appeared to understand ignorance (.80), and the proportion who answered all three belief questions correctly and thus appeared to understand false belief (.33). The difference score found here is over three times the size of the mean of the difference scores found in the previous studies in which belief reasoning was confounded with PAR (Wellman & Liu, 2004). Our finding of a larger difference score is certainly in need of replication; additional studies, employing methods by which PAR and belief reasoning can be un-confounded, would provide a better estimate of the true difference between children's understanding of ignorance and their understanding of false belief.

We used 3-option false-belief tasks to remove the confound between PAR and belief reasoning. The knowledge and belief questions in our 3-option tasks were similar in form to the questions used in the previous studies. The knowledge question assessed whether children knew that a protagonist was ignorant (e.g., that the protagonist would not know that the fake rock with Yogi sitting on it was a sponge, without touching it). The belief question assessed whether children attributed the correct false belief (rock) to the protagonist, versus the irrelevant, unjustified belief (Yogi's car). The latter answer would indicate that children simply thought the protagonist would get it wrong. The belief question presented a choice between false-belief and irrelevant options, and thus was structurally similar to the belief question in 2-option tasks, which presents a choice between false-belief and reality. We used three different tasks. Comparing the proportion of children who answered all three knowledge questions correctly to the proportion who answered all three belief questions correctly reduced the likelihood that correct answers to the belief question would be inflated by some PAR children guessing correctly. This meant that we assessed consistent understanding, rather than initial understanding, of each concept. The literature shows that even 3½-year-olds who pass 2-option false-belief tasks do so with a high level of consistency (Wellman, et al., 2001). This makes it difficult to explain why, if they understood false belief, only .33 of 5-year-olds consistently answered the belief questions correctly on three false-belief tasks, while .80 consistently answered the knowledge questions correctly on those tasks. PAR theory provides a principled explanation: The small difference in 2-option tasks with younger children records the lag between acquiring PAR Rule 1 and acquiring PAR Rule 2, while the larger difference at age 5 records the lag between PAR Rule 1 and understanding false belief.

Finally, there was very preliminary evidence suggesting that PAR Rule 1 is distinct from PAR Rule 2. Eleven children, over .25 of the sample of 5-year-olds, consistently answered the knowledge question correctly, showing that they had a good grasp of Rule 1. However, those same children did not consistently avoid choosing the reality option. This finding may suggest that children can acquire Rule 1 before Rule 2, and perhaps that the rules develop somewhat independently and need to be actively brought together and coordinated by the child. PAR theory holds that when the Rule 1 concept of knowing is working in coordination with the Rule 2 link to behavior, it gives the child a working, nonrepresentational ToM. PAR Rule 1 occupies the role that will be filled by the new concept of belief when children acquire a representational ToM.

If 3-option false-belief tasks were the only way to distinguish belief reasoning from PAR, the field would be at a stalemate, because any attempts to reduce concerns about performance factors on those tasks could always be questioned. Fortunately, true-belief tasks do not confound belief reasoning and PAR, and do not make more processing demands than 2-option false-belief tasks. In Chapter V we test PAR theory with a different 2-option task.

V. Taking the False Belief Out of False-Belief Tasks

Maxi comes into the kitchen and then leaves.
His mother comes in and puts his chocolate in cupboard A.
Then she moves it to cupboard B, and then she leaves.
Maxi returns to get some chocolate.
Where will Maxi think his chocolate is?

It is just as important to understand what people are likely to do when they have no beliefs, as it is when they have false or true beliefs. We should avoid betting on what people will do when we know they are guessing. In the example above, Maxi does not have a belief about where his chocolate is. Understanding that Maxi will guess where his chocolate is, and that he is as likely to go to either cupboard, requires understanding that Maxi mentally represents both cupboards as possibly containing the chocolate. Without an understanding of mental representation, PAR-users must resort to PAR Rule 2 (knowing leads to acting correctly; not knowing leads to acting incorrectly) if they are to have any way, other than by reality reasoning, to understand and predict ignorant Maxi's actions. In the current chapter, we used no-belief tasks like the one above to test whether children used PAR Rule 2, or whether they understood that ignorant agents would guess.

Introduction

A few researchers have studied young children's reasoning about ignorance, and their findings suggest that children used PAR Rule 2. Ruffman (1996) used a task in which the protagonist did not know what was in a nondescript box. Ruffman found that 4- to 7-year-olds were biased toward predicting that an ignorant protagonist would give the wrong answer rather than sometimes guess correctly. Chen et al. (2015) used tasks in which protagonists did not know which location held a desired object, or did not know which choice of object was correct. Chen et al. also found that 4½- to 6½-year-olds were biased toward predicting that ignorant protagonists would get it wrong. Chen et al. asked a follow-up justification question, and most children who predicted that the ignorant protagonists would get it wrong justified their answers by referring to the protagonists not knowing or not seeing.

The only challenge to these two confirmations of the PAR theory comes from Friedman and Petrashek (2009), who unexpectedly found the opposite result; namely, that 4- and 5-year-olds tended to predict that a girl who did not know which box her dog had hidden under would get it right. The authors struggled to explain why these children, who had all passed a false-belief screening task, would have responded as if they were using reality reasoning. The authors did not ask a justification question (i.e., "Why will she look there?") that might have provided insight into children's reasoning. The story that children were told did not recount the protagonist's interactions with the dog to make it clear that she was ignorant; they were simply told, "Sally doesn't know." Children might have assumed that the girl would look in the right place because they were told, "Her dog likes to hide under these boxes," which implies that it was a familiar occurrence. In addition, Rai and Mitchell (2004) and Symons et al. (1997) found that 5-year-olds failed false-belief tasks in which the protagonist sought an entity that was a volitional being that decided where to go.

We constructed no-belief location and no-belief contents tasks that were analogues of false-belief location and false-belief contents tasks, by removing the false-belief information. We also added two important procedures. First, it is important to identify those who used reality reasoning, because a mixture of PAR and reality reasoning responses in aggregate could look like the random responding expected of children who use belief reasoning. Thus, we selected children who passed false-belief tasks, as did both Chen et al. (2015) and Friedman and Petrashek (2009).

Second, one forced-choice prediction question could mask belief reasoning. To guard against this possibility, Chen et al. added a follow-up alternative possibility question ("What about the other one? Is it likely that [protagonist will choose the other option]?"). Most of the 6-year-olds, but not the younger children, recognized that the protagonist was also likely to select the other option. We used a different procedure to allow children to express their understanding that an ignorant protagonist will be equally likely to choose each option; namely, the betting procedure invented by Ruffman et al. (2001). In this procedure, children distribute tokens to each option to indicate how likely they think the protagonist will be to choose each one. Ruffman et al. (2001) found that the betting procedure was valid in false-belief tasks for children as young as 3½ years of age.

Method

Participants

Participants included 25 4½-year-olds (47–59 months; $M = 54$, 19 boys), 22 5½-year-olds (60–71 months; $M = 64$, 17 boys), and 29 6½-year-olds (72–86 months; $M = 79$, 12 boys). Data from one 6½-year-old who participated were excluded due to experimenter error in the testing procedure.

Procedures

Each child received a betting training task, followed by two no-belief tasks in counterbalanced order, followed by two false-belief tasks in counterbalanced order. This order ensured that the false-belief tasks would not influence performance in the no-belief tasks (Chen et al., 2015; Friedman & Petrashek, 2009). In all belief tasks, the prediction question was asked before the betting question. Ruffman et al. (2001) found no difference depending on which question was asked first. Children were asked pretest control questions before the prediction question. If they answered any pretest control questions incorrectly, the story was repeated once and the questions were re-asked. The control questions were asked once again between the prediction and betting question. These posttest control questions ensured that children had retained the story narratives while answering the betting question.

In addition, 10 of the 4½-year-olds received Ruffman et al.'s (2001) probabilities task at the end of the session, the purpose of which was to ensure that the youngest children could express certainty and uncertainty in response to the betting question. We decided to include this task after testing had begun, but the children who received it were the same age as the other 4½-year-olds, and the performance of the two groups on the other tasks was similar. Specific stories and questions used in each of the tasks follow.

Betting Training Task

We followed the procedures of Ruffman et al. (2001) with one exception (below). The training prepared children to bet tokens to express certainty about their answers in the belief tasks, and serves two important functions. First, it ensured that they understood that when they were uncertain, they should place tokens on both answers, and when they were certain they should place all their tokens on one answer. Second, it motivated them to win tokens by demonstrating that tokens bet on the correct answer would be doubled, and tokens bet on the incorrect answer would be lost. In all tasks, children received 10 new tokens to bet each time.

Children were first shown a sheet that contained images of tokens and were told that it represented how many tokens other children had won in previous games. Children were encouraged to try to win as many tokens as possible. A die was placed under one of two cups while the cups were screened from view. The screen was then removed, and children were told to place their tokens next to the cup that the die was under and if they did not know which cup the die was under to place their tokens next to both cups. In the Ruffman et al. procedure, children received two trials with transparent cups and two trials with opaque cups, and if a child had either won or lost all of the first three training trials the experimenter fixed the fourth (opaque) trial so that each child had an experience of both winning and losing. One child in their study failed to place tokens next to both opaque cups on at least one opaque trial and was eliminated. We gave all children a fifth, opaque trial that was rigged so that the child had to fail (both cups were empty), and we also gave children up to three extra opaque trials if they failed to place tokens next to both opaque cups on at least one trial. Fourteen children required one or more of the extra opaque trials, and seven children were eliminated for failing to place tokens next to both opaque cups (three 4½-year-olds, one 5½-year-old, and three 6½-year-olds).

No-Belief Location Task

This task was designed to mirror the standard false-belief location task with one important difference: The protagonist (Sarah) never saw where her toy was placed. Children watched Sarah come into the kitchen and then leave. While Sarah was out of the room her father entered with a toy and placed it in one of the cupboards (Location A), but then moved it to another cupboard (Location B) before leaving. Children were asked control questions: "Remember when Dad was here, where did Dad put the toy away first [CQ1]? Then Dad cleaned up, right? Where did Dad put the toy next [CQ2]? Did Sarah see him put it there [CQ3]?" The prediction question followed: "When Sarah comes back to get her toy and stands right here, where will she think her toy is?" Children were re-asked the control questions, followed by the betting question: "Now show me with these tokens where Sarah will think her toy is when she comes back to get it and stands right here. Put your tokens next to the cupboard where she will think her toy is. If you don't know where she will think it is, put tokens next to both cupboards. You can put the same number next to both or a different number. So, put your tokens next to the cupboard where she will think it is."

No-Belief Contents Task

This task was designed to mirror the standard false-belief contents task with one important difference: The container was a plain white box. Children were shown two objects (e.g., toy car and dinosaur) and were asked to put one in the box. Children were asked the following control questions: "Which one did you put inside the box [CQ1]? Which one did you NOT put inside the box [CQ2]? The other object was removed, and the prediction question followed: "Let's pretend I have a friend named Johnny and he came in and didn't see what you put in the box. When he first looks at the box, before he opens it, will he think there is a car or a dinosaur inside?" Children were re-asked the control questions, after which the experimenter put a picture of a car and a picture of a dinosaur on the table, and asked the betting question: "Now show me with these tokens what Johnny will think is inside when he first looks at the box, before he opens it, a car or a dinosaur? Put your tokens next to the toy he would think is in the box. If you don't know what he will think, put tokens next to both toys. You can put the same number next to both or a different number. So, put your tokens next to the toy he will think is in there."

Five children responded, “I don’t know” in the no-belief tasks (two in both the location and contents tasks; two in location only; and one in contents only). One child responded, “Something other than car [the reality option]” in no-belief contents. All were asked to guess.

False-Belief Location Task

Children watched as Maxi placed his chocolate in one cupboard and left to go outside to play. Maxi’s mom then came in to clean the kitchen, saw that the original cupboard was dirty, took the chocolate out and put it into the other cupboard, and then left. Children were asked the following control questions: “Remember when Maxi was here, where did Maxi put the chocolate [CQ1]? Then Maxi left, right? And his mom took the chocolate out, right? Where did Maxi’s mom move the chocolate to [CQ2]? Did Maxi see her put it there [CQ3]?” The prediction question, the re-asked control questions, and the betting question were framed in the same way as in no-belief location.

False-Belief Contents Task

Children were shown a box of Band-Aids and asked what was inside. If children failed to say “Band-Aids,” they were given a series of prompts (“What does the box look like it will have inside?” “Can you guess what will be inside?” “What kinds of things come in a box like this?”), until they gave the correct response. The experimenter revealed that a key was inside, gave it to the child to handle for a moment, and then replaced it in the box. Children were asked two control questions: “What kind of box is it [CQ1]? What is inside the box [CQ2]?” The prediction question, the re-asked control questions, and the betting question were framed in the same way as in no-belief contents.

Probabilities Task

We based our procedures on Ruffman et al. (2001). In three trials (10–0, 9–1, and ambiguous, given in that order), children were first reminded of the goal of winning tokens and the rule that if they put tokens next to the correct answer they would win the same number, and if they put tokens next to the wrong one they would lose those tokens. In the first two trials, children were shown a cup with 10 blocks and told to see how many were inside. In 10–0, all were the same color; in 9–1, nine were one color and one was the other color. The experimenter then put up a screen and took one block out of the cup and placed it in a box, then removed the screen. Children were asked to show, by distributing tokens between a picture of a red block and a picture of a green block, what color they thought was inside the box. In the ambiguous trial children were not allowed to see the color of the one block that was inside the box. Instead, there was a lid on the box and children were told that there was a block inside, but were not told what color.

Results

On all tasks, the first time each pretest control question was asked, at least .88 of 4½-year-olds, and .93 of 5½- and 6½-year-olds answered it correctly. All children passed the pretest control questions by the second presentation. On the posttest control questions, all children answered correctly.

The mean proportions of correct answers to the two different false-belief tasks at each age were .60 ($SD = .46$), .75 ($SD = .40$), and .78 ($SD = .39$), respectively. At these ages, generally .67, .80, and .90 pass false-belief tasks (Wellman et al., 2001). The great majority of 4½-, 5½-, and 6½-year-olds (.84, .86, and .90, respectively) gave consistent answers to the two different false-belief tasks, which justifies basing the analyses below on the children who passed both false-belief tasks versus those who failed one or both. At each age, 13, 15, and 21 children, respectively, passed both false-belief tasks.

Prediction Question

For the children who passed both false-belief tasks, a 3 (age group) one-way ANOVA on the proportion of no-belief tasks in which they predicted the protagonist would get it wrong showed no significant age differences, $F(2, 46) = .522$, $p = .597$. The overall mean was .76 ($SD = .36$), which was significantly greater than chance (.50), $t(48) = 5.025$, $p < .001$. This finding is consistent with PAR, and replicates Chen et al.'s (2015) findings.

For the children who failed one or both false-belief tasks, a similar ANOVA also showed no significant age differences, $F(2, 24) = 0.662$, $p = .525$. The overall mean was .26 ($SD = .38$), which was significantly less than chance (.50), $t(26) = 3.323$, $p = .003$. This finding is consistent with reality reasoning, and also replicates Chen et al.'s (2015) findings.

Betting Question

Using betting as the dependent measure produced similar findings. For the children who passed both false-belief tasks, a 3 (age group) one-way ANOVA on the proportion of tokens children bet that the protagonist would get it wrong in the two no-belief tasks showed no significant age differences, $F(2, 46) = 1.227$, $p = .303$. The overall mean was .64 ($SD = .37$), which was significantly greater than chance (.50), $t(48) = 2.668$, $p = .010$. This finding is consistent with PAR, and replicates Chen et al.'s (2015) findings.

For the children who failed one or both false-belief tasks, a similar ANOVA also showed no significant age differences, $F(2, 24) = 0.674$, $p = .519$. The overall mean was .24 ($SD = .33$), which was significantly less than chance

(.50), $t(26) = 4.072$, $p < .001$. This finding is consistent with reality reasoning, and also replicates Chen et al.'s (2015) findings.

Probabilities Task

The probabilities task provided a validity check that children could express their feelings of certainty by betting. We replicated Ruffman et al.'s (2001; Study 1) findings indicating that the betting question does tap into young children's feelings of certainty. In the complete certainty (10–0) trial, the mean proportion of tokens that 4½-year-olds bet on the dominant color was .95 ($SD = .16$), and in the 9–1 trial, which represented only a slight difference in certainty, they bet significantly less on the dominant color (.56, $SD = .35$), $t(9) = 2.623$, $p = .028$. The decrease in 9–1 was not due to a simple matching strategy of distributing the tokens to match the mixture of colors they saw in the cup, because in the ambiguous trial, in which children never saw the color of the single block in the box, they also bet significantly less on the color that had been dominant in 10-0 ($M = 0.63$), $t(9) = 2.776$, $p = .022$. In Ruffman et al. (2001), the respective means on these tasks for 4½-year-olds (.97, .54, and .44) were similar to the above findings.

Additional Analyses

We explored the betting data in two ways for some preliminary insight into the likely proportion of individuals who used belief reasoning. We first looked for the pattern of performance one would presumably expect from individuals with a firm grasp of false belief. They should pass both false-belief tasks with complete certainty by betting all 10 tokens on the false-belief option in both tasks, and be less certain that the protagonist would get it wrong in the no-belief tasks. Among 6½-year-olds, .52 (15/29) fit this pattern, whereas among the two younger ages combined, only .15 (7/47) fit this pattern.

The second method yielded almost identical findings. We looked for the pattern of performance one would presumably expect from individuals who had at least some sensitivity to false belief. They should simply bet in the correct direction; namely, more tokens that the protagonist would get it wrong in the false-belief tasks than in the no-belief tasks. A few children at each age (5 at 4½, 2 at 5½, and 1 at 6½) bet in the incorrect direction. This behavior appeared to represent chance responding. If indeed these responses were based on chance, we would expect that similar numbers of responses that appeared to be correct were actually the result of chance rather than of understanding. To take this possibility into account, we subtracted the number who bet in the incorrect direction from the number who bet in the correct direction, leaving .59 (17/29) of 6½-year-olds who thus appeared to show some sensitivity to false belief. At the two younger ages combined, only .17 (8/47) showed some sensitivity to false belief calculated in this manner.

Discussion

PAR is hypothesized to include two distinct reasoning processes, both of which children use when they pass 2-option false-belief tasks. Chapter IV provided evidence that children use PAR Rule 1 (seeing leads to knowing; not seeing leads to not knowing) before they pass false-belief tasks. The current study provides evidence of use of PAR Rule 2, (knowing leads to getting it right; not knowing leads to getting it wrong).

According to the theory, PAR-users have a nonrepresentational ToM, in which PAR Rule 1 provides a concept of knowing, and PAR Rule 2 provides a heuristic for predicting behavior. Rule 2 is hypothesized to operate on a nonrepresentational concept of knowing, and so should not be able to predict that an ignorant agent will represent unseen choices as equally likely and guess randomly between them, just as it should not be able to predict agents' behavior on the basis of true beliefs or false beliefs. The current findings are in accord with PAR theory, and they replicated those of Chen et al. (2015) by using different methods. The findings showed that 4½- to 6½-year-olds who passed both false-belief tasks—most of whom, PAR theory would hold, used PAR—were biased at all three ages toward predicting that an ignorant protagonist would get it wrong, in accord with PAR Rule 2. Children who failed one or more false-belief tasks were biased in the no-belief tasks toward predicting the protagonist would get it right, consistent with reality reasoning in both tasks.

Chen et al. asked a follow-up alternative possibility question, and in response, few 4½-, about half of 5½-, and most 6½-year-olds recognized that the protagonist was also likely to select the other option. However, although most 6½-year-olds gave justifications in which they mentioned not knowing or not seeing, fewer than half offered either of the two types of justifications that Chen et al. coded as random choice (e.g., "He guessed") and appropriate reasoning (e.g., "Maybe he likes that one better"). The infrequency of these two types of justifications also seems to indicate an unawareness of guessing.

We used a different follow-up procedure, the betting question, in which children distributed tokens to each option to indicate how certain they were that the protagonist would choose each one. Ruffman et al. (2001) validated the betting procedure with children younger than the participants in the current study. The betting procedure would easily allow children to express a judgment that the ignorant protagonist would be equally likely to choose each option, by simply placing equal numbers of tokens next to each option. Nevertheless, the findings with the betting question were the same as with the prediction question (i.e., the bias that the ignorant protagonist would get it wrong was present at all ages to a similar degree). Only about .50 of 6½-year-olds showed more certainty that a protagonist with a false belief would get it wrong than a protagonist who was ignorant, an indication that they likely used belief reasoning.

The data presented in Chapters II and V are consistent with predictions of PAR theory in both types of 2-option tasks, true-belief and no-belief. The evidence from 2-option tasks is important because it is immune to potential concerns about computational or inhibitory demands in 3-option tasks. Evidence from true-belief tasks would appear to challenge the theory that true belief is the default attribution of an innate *theory of mind mechanism-selection processor* (*ToMM-SP*; Leslie, 2000a). PAR can be directly tested against ToMM-SP, by trading on the role that response inhibition plays in ToMM-SP, to which we turn in Chapter VI.

VI. Are True Beliefs Easier to Understand Than False Beliefs?

Maxi looks for a place to put his piece of fish.
He sees a sick kitten in box A.
Then Maxi watches the kitten crawl into box B.
He knows he should not put the fish with the kitten because it will make the kitten's tummy sore.
Maxi leaves to get his piece of fish.
Maxi returns with the fish.
Which box will Maxi go to with the fish?

PAR is a nonrepresentational ToM, which means that during the time that children use PAR, they should have no understanding of false beliefs or true beliefs. The evidence in Chapters II and III suggested that attributions of true beliefs and false beliefs posed similar difficulties for children. However, most of the research has focused exclusively on understanding why children fail false-belief tasks. All three major theoretical approaches (theory-theory, simulation theory, and modularity theory) point in one way or another to the fact that children have to ignore what they themselves know in order to attribute a false belief. PAR theory holds instead that it is not false belief that poses difficulty; it is belief. In the present chapter, we used the task in the example above. The task was designed by modularity theorists to test whether children automatically attribute true beliefs, but struggle to attribute false beliefs. We made only one change to the original task, reflected in the line, "Maxi returns with the fish."

Introduction

One of the most difficult issues in understanding ToM development is explaining how children acquire concepts of mental states (Leslie, 2000b). PAR theory describes the initial concepts of seeing and knowing as mentalistic but not representational, and we have sketched an account in Chapter I of how those concepts might be coconstructed between parents and children. That leaves the task of explaining how children acquire new concepts of representational mental states. One approach to the concept learnability problem is to avoid it by proposing that all mental state concepts are innate. The classic and most well-developed model of an innate ToM is Leslie's (2000a) ToMM-SP model. ToMM automatically generates mental states as candidate explanations when the child confronts some behavior, such as Dad walking into the kitchen saying, "Where are my keys?" and opening a drawer.

In response, the 3-year-old's ToMM automatically generates candidates for what Dad does and does not want, see, know, and believe, to mention a few. One set of candidates would include: Dad wants keys, doesn't see them, knows they are in the kitchen, and has a true belief they are in the drawer. But ToMM's job is not done until it generates all the possible candidates, including Dad doesn't want keys, sees them, doesn't know they are in the kitchen, and has a false belief they are in the drawer. All candidates are equal in ToMM's blind eye, and thus the next task is to select among these possibilities.

Selection is the job of the selection processor (SP), and it works by hypothesis-testing, learning, and inhibiting. The child has to learn which mental states someone is likely to have in a given situation (e.g., Does Dad want keys or not?). For just these four states, there are 16 combinations of present versus absent, true versus false. The problem gets worse because to learn about which mental states someone is likely to have, the child has to learn which states are likely to precede, and cause, which other states (e.g., Does seeing lead to knowing, or does knowing lead to seeing?). There are 24 permutations of want, see, know, and believe, and in each of the 24 orders, there are three potential causal links to learn about (e.g., Does wanting lead to seeing or not?), yielding eight patterns of cause or not cause. That yields 3,072 possibilities for the child to test against Dad's observed behavior to arrive at a workable mentalistic account of what Dad is up to. This would tax the hypothesis-testing mechanisms in any learning system.

The SP does get some help in at least figuring out what someone might believe. The help exists in the form of its own innate bias to give greater salience to true beliefs than false beliefs. As Leslie et al. (2005, p. 48) explain:

A true-belief is always more highly valued by SP and is selected by default. A true-belief default is ecologically valid because, at least about mundane matters, people's beliefs usually *are* true. We can go a little further than this. For a basic belief-attributing system—one whose business concerns simple everyday beliefs—the true-belief attribution *ought* to be the default (emphases in original).

The true-belief default does not just lend a helping hand. It is central to ToMM-SP because it explains why, if both true and false beliefs are innately generated candidates, children supposedly only fail false-belief tasks. The argument is that attributing false beliefs requires children to have developed sufficient inhibitory control in order for SP to be able to inhibit the true-belief default in false-belief tasks, so that the SP can select a false belief for Maxi.

To provide evidence of the true-belief default, Leslie and Polizzi (1998; Leslie et al., 2005) developed what they called avoidance-desire tasks. Avoidance-desire tasks are like standard true- and false-belief location tasks, but whereas in the standard tasks, Maxi wants to go to the cupboard that contains a desired object, in avoidance-desire tasks, Maxi wants to avoid the cupboard that contains an undesired object. Children who are trying to figure out what Maxi will do cannot automatically assume that Maxi desires the object. Children have to inhibit something they would otherwise automatically default to in reasoning about both Maxi's desire (i.e., that he has a positive desire for the object) and his belief (i.e., that he has a true belief about where the object is). The example at the beginning of this chapter is an avoidance-desire true-belief task with a return condition.

Leslie et al.'s (2005) logic and method are as follows: Children who pass standard false-belief tasks at the younger ages are as yet presumably capable of inhibiting only one action or decision of theirs. In standard false-belief tasks, that one inhibition is used to inhibit the true-belief default that would otherwise lead them to fail the false-belief task.

Avoidance-desire false-belief tasks require children to also inhibit the positive-desire default, and children will not be able to do both. They will be able to carry out only one of the two required inhibitions. Let us say that in an avoidance-desire false-belief task Maxi has a false belief that the undesirable object is in A, but it is really in B. The correct answer is that Maxi will look in B. If children only inhibit the true-belief default, they will grant Maxi his false belief that the object is in A, but, forgetting that Maxi wants to avoid it, they will say he will look in A, and they will fail the task. If children only inhibit the positive-desire default, they will grant Maxi his desire to avoid the object, but, forgetting that Maxi has a false belief that the object is in A, they will also say he will look in A, and they will also fail the task.

Avoidance-desire true-belief tasks require children to only inhibit the positive-desire default, and so children who pass standard false-belief tasks will be able to pass avoidance-desire true-belief tasks. Thus, Leslie and colleagues selected children who passed a standard false-belief location task, and gave them avoidance-desire versions of true-belief and false-belief location tasks to show that the true-belief default makes true belief easier than false belief. Specifically, the pattern predicted by ToMM-SP for children who are selected on the basis of having passed a standard false-belief location task, is that they should also pass a standard true-belief location task and an avoidance-desire true-belief location task, but they should fail an avoidance-desire false-belief location task.

The basic outline of the avoidance-desire task that Leslie and Polizzi (1998) came up with starts with Sally wanting to put a piece of fish in a box for later. Inspecting two boxes, she discovers a sick cat in one. Children are told that she does not want to put the fish in that box because it will make the cat sicker. She leaves to get the fish, and while she is away, the cat crawls into the other box. So, when Sally returns, she has a false belief that the cat is still in the first box. Children are asked where she will put the fish. As predicted, children who passed the standard false-belief task seldom passed the avoidance-desire false-belief task (Cassidy, 1998; Leslie & Polizzi, 1998; Leslie et al., 2005).

The companion avoidance-desire true-belief tasks that Leslie et al. (2005) used were always true-belief stay conditions. In their avoidance-desire true-belief stay condition, Billy is in a similar predicament with a food item and a sick animal, and he also does not want them to get together. Billy also leaves the room to get some meat, in this case, but the sick puppy waits until Billy returns to crawl into the other box. So Billy sees him crawl into the second box, then Billy stays in the room, and children are immediately asked where Billy will put the meat. As predicted, children performed very well on the avoidance-desire true-belief task with a stay condition (Leslie et al., 2005).

It is unclear why Leslie et al. (2005) always used stay conditions in the avoidance-desire true-belief tasks. An avoidance desire should also not disrupt performance in true-belief return conditions, if children have a true-belief default. We used an avoidance-desire true-belief return condition, depicted in the example at the beginning of the chapter. The only difference in the return condition, compared to the stay condition, is that Billy leaves, not before, but after watching the puppy crawl to the other box. The puppy does not move while Billy is away, and so Billy returns with his true belief intact. Then children are asked where Billy will put the meat.

Leslie et al. (2005) selected children who passed a standard false-belief task, because they were presumably capable of one inhibition of the true-belief default when necessary. We know from Chapter I that children often fail true-belief return conditions, even without added processing demands. To provide a fair test of reasoning about true and false beliefs, we selected children who passed both a standard false-belief task and a standard true-belief return task. We observed how those children's reasoning about protagonists' beliefs fared under the added processing demands.

Method

Participants

Participants included nineteen 4½-year-olds (49–57 months; $M = 53$; 8 boys), seventeen 5½-year-olds (60–71 months; $M = 64$; 3 boys), ten 6½-year-olds (72–81 months; $M = 76$; 6 boys), and three 7½-year-olds (89–94 months; $M = 91$; 2 boys).

Procedures

Each child received four tasks: Avoidance-desire false-belief, avoidance-desire true-belief return condition, standard location false-belief, and standard location true-belief return condition. Each of the 24 possible task orders was assigned to one or two of the 49 children. We followed the original procedures for the avoidance-desire false-belief task almost verbatim (Leslie & Polizzi, 1998; Leslie et al., 2005). Below we give the essential features of that task, and then describe briefly how the other tasks compare to it.

Avoidance-Desire False-Belief Task

The story is enacted with a two-room playhouse with dolls and animal figures. The front walls are removed. There is no door between the rooms, so the protagonist has to walk outside one room to get into the other. One room has two boxes. Sally is in the other room with her piece of fish that she wants to put in a box for later. She leaves the fish in the first room and goes into the second room and looks in each box.

Sally sees a cat in one box, and is told that the cat is sick and that if she puts the fish in the box with the cat, it will make the cat's tummy sore. Sally goes back to the first room to get the fish, and while she is in the other room the experimenter moves the cat to the other box, explaining to the participant child that the cat is crawling to the other box, and that Sally did not see the cat do that. Children are asked control questions to ensure that they understand Sally's desire to avoid giving the fish to the cat, remember the cat's movements, and know that Sally did not see the cat move. If children failed any control question, they were reminded of that part of the story and re-asked the question. Sally comes back with the fish, and children are asked which box Sally will go to with the fish.

Avoidance-Desire True-Belief Return Task

The task is the same as the avoidance-desire false-belief task except that before Billy leaves to get a piece of meat, he sees a puppy crawl to the other box, and the experimenter points out that Billy sees that. Billy then goes to get the meat. Children are asked control questions. Billy returns with the meat, and children are asked which box Billy will go to with the meat.

Standard False-Belief Location and True-Belief Location Return Tasks

Both tasks follow the standard Maxi task procedures, except that in both tasks, the protagonists do not put the objects in the cupboards themselves. Instead, the protagonists ask their parent to do it for them, and the protagonists watch. In the avoidance-desire tasks, the protagonists do not put the animals in the boxes, and we desired to avoid having the protagonists in the standard tasks more actively involved with the false-belief location than the protagonists in the avoidance-desire tasks.

Results

On all tasks, the first time each control question was asked, the proportion of children who answered correctly was at least .90. All children passed the control questions by the second presentation.

We first examined performance in the standard tasks in order to select the children who passed both the true-belief and false-belief tasks. Table 7 shows the number of children in each pattern of passing versus failing the

TABLE 7
Number of Children (Pooling 4½- to 7½-Year-Olds) in Each Pattern of Performance and Reasoning Strategy on Location Tasks

	False-Belief	
True-Belief	Pass	Fail
Pass	22[a]	13[b]
Fail	9[c]	5[d]

Note. PAR = perceptual access reasoning.
[a]Belief reasoning.
[b]Reality reasoning.
[c]PAR.
[d]Switching = reality reasoning on false-belief task and PAR on true-belief task.

standard true-belief and false-belief tasks. The patterns are labeled with the reasoning strategy they represent: 22 children passed both, which is consistent with belief reasoning, and these children's response patterns were examined in the primary analyses.

Performance in the avoidance-desire tasks of the 22 children who passed both standard tasks was as follows: On the avoidance-desire false-belief task, only .64 (14/22) passed, in accord with Leslie et al.'s (2005) findings and prediction of disrupted reasoning about false belief with added processing demands. However, children fared no better when reasoning about true belief. On the avoidance-desire true-belief return task, only .68 (15/22) passed. Among the 22 belief reasoners, 12 passed both avoidance-desire tasks, 5 failed both, only 3 passed the true-belief version and failed the false-belief version, and 2 were in the opposite pattern, McNemars $p = 1.00$.

Discussion

We tested the proposition of a true-belief default that is central to the innatist position developed by Leslie (1987; 2000a). The true-belief default is used to explain why young children fail false-belief tasks if they have an innate concept of belief. Using Leslie et al.'s (2005) logic, there was no evidence of a true-belief default. Children identified as using belief reasoning, on the basis of passing standard true-belief and false-belief tasks, often failed both tasks when required to reason about an avoidance-desire. Leslie et al. (2005) found that the additional processing demands did not disrupt true-belief performance, but their evidence came from only one type of true-belief condition, in which the protagonist stayed in the room after acquiring the true belief. The current study employed a true-belief return condition, in which the protagonist left the room after acquiring the true belief, and then returned. If a true-belief default is "ecologically valid," and ought to be part of a ToM mechanism "whose business concerns simple everyday beliefs"

(Leslie et al., 2005, p. 48), then it ought to apply when someone leaves and returns to a room.

Behavioral and neurological studies of adults have also found evidence contrary to a true-belief default. Back and Apperly (2010) found that response times on both true- and false-belief location trials covaried with task demands, and they concluded that adults did not attribute true beliefs automatically. Döhnel et al. (2012) incorporated some improved true- and false-belief controls into fMRI procedures, and found common functional activity for true- and false-belief reasoning. Both studies used true-belief stay conditions, despite available developmental evidence that the reasoning in return conditions, as opposed to stay conditions, is more similar to the reasoning in false-belief tasks (Fabricius et al., 2010; Friedman et al. 2003). Thus, similarities in adult reasoning appeared even though the true-belief stay controls likely biased the findings toward differences. Both studies also found some residual differences between true-belief and false-belief reasoning, which we anticipate will attenuate in return conditions.

VII. Judgment-plus-Justification in False-Belief and True-Belief Tasks

Maxi puts his chocolate in cupboard A.
Then Maxi watches his mother move it to cupboard B.
Maxi leaves and then returns.
Where does Maxi think his chocolate is?
Why does Maxi think his chocolate is there?

The above example is the same true-belief task from Chapter II, but with an added justification question at the end. Why should we ask children to justify why they think that Maxi will have a true belief? The reason is that some researchers in the past asked children to justify why they thought that Maxi would have a false belief. The children who passed the false-belief tasks gave what sounded like perfectly good justifications that referred to Maxi's false belief. PAR theory holds, however, that children pass false-belief tasks without understanding false beliefs. PAR theory leads us to suspect that children's justifications only sound like references to false belief, but are really references to Maxi getting it wrong and looking in the empty cupboard. PAR theory predicts that children should give the same justifications to their incorrect answers in true-belief tasks. The same justification could not refer to Maxi's false belief in one case, and his lack of true belief in the other case. In the current chapter, we compare children's justifications of their correct answers in false-belief tasks to their justifications of their incorrect answers in true-belief tasks.

Introduction

If children pass false-belief tasks, and also justify their answers by clearly indicating that the protagonist has a false belief, it would show that the children used belief reasoning, not PAR. Two early studies (Clements & Perner, 1994; Wimmer & Weichbold, 1994) reported that large majorities of 3½- to 4½-year-olds who passed a false-belief location task also gave correct justifications for why Maxi will look in location A. It was rare for these young children to say that Maxi thinks his chocolate is in location A, or doesn't know that it was moved to B, but they did commonly say, "Because that's where he put it." That justification sounded like a shorthand reference to Maxi's false belief that his chocolate is still in A, and it was counted as a correct justification.

The primary justification that young children gave in the two studies above ("Because that's where he put it") has face validity as a reference to Maxi's false belief, but its construct validity has not been tested. PAR theory suggests that "Because that's where he put it" could instead be shorthand for "That's where Maxi put it, but it's not there now because his mom moved it." In other words, the justification could be an implicit reference to the fact that location A is now the empty, wrong location. For simplicity, in the current chapter we will refer to justifications of the form, "Because that's where he put it," as [protagonist] put it in A.

In the current study, we tested the construct validity of [protagonist] put it in A, by examining its use in both false-belief and true-belief tasks. Clements and Perner (1994) also used true- and false-belief tasks, but they only reported justifications in the false-belief task. We tested older children (5½-year-olds) than those in the two previous studies to see if they still primarily used [protagonist] put it in A, or if their justifications more clearly referred to beliefs.

The tasks we used were the same false-belief location and true-belief location return condition tasks used in Fabricius et al. (2010, Study 2). In both cases, the protagonist puts it in location A, and the parent puts it in location B. In the false-belief task, Maxi puts his chocolate in A and leaves, and his mom moves it to B in his absence. In the true-belief task, Sarah puts her toy in A and then watches her dad move it to B, after which they both leave.

As noted above, [protagonist] put it in A is ambiguous because it could refer either to the protagonist's false belief, or to the wrong location. Correspondingly, the justification [parent] put it in B is ambiguous because it could refer either to the protagonist's true belief or to the reality location (Fabricius et al., 2010).

PAR theory entails that many of the 5½-year-olds who pass the false-belief task and give the justification [protagonist] put it in A, will fail the true-belief task and give the same justification, in which case [protagonist] put it in A cannot be assumed to refer to the protagonist's false belief; instead, it would more likely refer to the fact that location A is now the empty, wrong location.

Children who use belief reasoning pass both tasks. They could use [protagonist] put it in A as shorthand for Maxi's false belief, and they could use [parent] put it in B as shorthand for Sarah's true belief. They could also use other references to mental state (e.g., In false-belief: "Maxi didn't see his mom move it; doesn't know she moved it; thinks it's still there." In true-belief: "Sarah watched dad put it there; knows it's there; remembers he put it there.").

Children who use PAR pass false-belief and fail true-belief. If they use [protagonist] put it in A to refer to the wrong location in one task, then they should do so in the other task as well.

Children who use reality reasoning fail false-belief and pass true-belief. If they use [parent] put it in B to refer to the reality location in one task, then they should do so in the other task as well.

This design also allowed us to evaluate Oktay-Gür and Rakoczy's (2017, p. 30) contention that true-belief tasks are too easy, and that older children may start to wonder about a potential hidden agenda behind the TB questions ("It is so obvious, why is she asking me this stupid question?"), reasoning that they must have missed or misunderstood something ("So the correct answer must be different from the obvious one—otherwise, why would she ask me, after all?").

The authors argue that the hypothetical reasoning suggested in the preceding quotation would explain why older children fail true-belief tasks. If this is the case, in the current study, we should be able to find evidence of suspicion of a hidden agenda in children's justifications of incorrect true-belief judgments. Oktay-Gür and Rakoczy did not ask for justifications. Evidence of children's "reasoning that they must have missed or misunderstood something" could be reflected in three types of justifications of incorrect true-belief judgments: (a) Mental state references, such as "Sarah forgot; didn't remember; didn't see; didn't pay attention to dad moving it" to B; (b) uncertainty references, such as "Maybe Sarah thinks" it's in A; or even (c) references to "I don't know why," or idiosyncratic reasons, such as "He likes yellow."

Method

Participants

Participants were forty-five 5½-year-olds (60–73 months; $M = 65$; 25 boys).

Procedures

The false-belief and true-belief location tasks are the same as those used in Study 2 of Fabricius et al. (2010). Children received the tasks in counterbalanced order. If they failed a control question, they were reminded of that part of the story and the question was repeated. The two tasks are presented verbatim below.

False-Belief Location Task

"This is Maxi's kitchen, and here's the green cupboard and here's the red cupboard. Maxi is eating some chocolate. He wants to save his chocolate for later, so he puts it away in the red cupboard. Then Maxi goes outside to play. [*Maxi leaves*] He's gone now so he doesn't see what happens next. Maxi's mom comes in and starts to clean the kitchen. She sees that the red cupboard is dirty so she takes the chocolate out and puts it into the green cupboard. Then Mom leaves the kitchen as well. Remember when Maxi was here, where did Maxi put the chocolate away? [CQ1] Then Maxi left, right? Where did Maxi's mom move the chocolate to? [CQ2] Did Maxi see her put it there?

[CQ3] Look, Maxi comes back to get his chocolate and stands right here. [*Maxi returns*, placed in his original position between the cupboards looking straight ahead] Where does Maxi think his chocolate is? [Belief Question] Why does he think his chocolate is there?" [Justification Question]

True-Belief Location Return Task

"This is Sarah's kitchen, and here's the purple cupboard and here's the yellow cupboard. Sarah is playing with her toy. She's finished playing, so he puts it away in the purple cupboard. After she puts her toy away, she comes back and sits down. She is sitting there so she watches what happens next. Sarah's dad comes in and starts to clean the kitchen. He sees that the purple cupboard is dirty so he takes the toy out and moves it into the yellow cupboard. He says, "Watch, Sarah. I'm moving your toy." She says "OK, I see. Thanks, Dad." Then Dad leaves the kitchen and Sarah goes outside to play. [*Sarah leaves*] Remember when Sarah was here, where did Sarah put the toy away? [CQ1] Then Sarah stayed there, right? And her Dad took the toy out, right? Did Sarah watch him move her toy? [CQ2] Where did Sarah's Dad move the toy to? [CQ3] Look, Sarah comes back to get her toy and stands right here. [*Sarah returns*, placed in her original position between the cupboards looking straight ahead] Where does Sarah think her toy is? [BQ] Why does she think her toy is there?" [JQ]

Coding

Justifications were coded into one of the following four categories: [Protagonist] put it in A, [parent] put it in B, mental state of the protagonist, and don't know or other. Mental state justifications included saying the protagonist did or did not see, watch, think, know, or forget; no child mentioned remember. Two independent coders coded all justifications into the above four categories, and agreed on .98 of the justifications; disagreements were resolved by discussion.

Results

In the false-belief location task, at least .98 of children answered each control question correctly the first time it was asked; in the true-belief location task, .89 of the sample did so. On the second presentation, all but one child passed the control questions who was nevertheless retained in the sample.

In the false-belief location task, .69 of children passed the belief question, and in the true-belief location task, .44 passed. Performance on the first task that children received was similar to the overall pattern: Among the 23 children who received the false-belief task first, .79 passed; among the 22 who received the true-belief task first, .53 passed. Task order did not significantly affect performance in either the false-belief task, Fisher's exact $p = .208$, two-tailed, or the true-belief task, $p = .236$, two-tailed. Thus, there appeared to be little carry-over from one task to the other, consistent with previous findings (Fabricius et al., 2010), but in contrast to Rakoczy and Oktay-Gür's (2020) finding that true-belief performance was worse when the false-belief task came first. Table 8 shows the number of children in each pattern of passing and failing the tasks, and the reasoning strategy associated with each pattern.

Table 9 shows the number of children with each pattern of justifications in each combination of passing and failing the tasks. Among the children who passed the false-belief task, .84 (26/31) gave [protagonist] put it in A justifications, which have been counted in the past as correct attributions of false belief. However, .62 (16/26) of those children who passed false-belief and gave [protagonist] put it in A justifications, failed the true-belief task and gave the same justification, which suggests that they did not use [protagonist] put it in A as a shorthand reference to Maxi's false belief, but rather as a PAR reference to the empty, wrong location.

The children who fit the pattern of PAR (i.e., those who passed false-belief but failed true-belief), provided little evidence in their justifications that they suspected that the true-belief task must have a hidden agenda and that the protagonist must in fact not have a true belief. Only .13 (3/23) gave mental state justifications when they failed the true-belief task indicating suspicion that Sarah would not have a true belief (i.e., "She didn't see dad move it to yellow." "She forgot it was in the purple." and "She thinks she put it there.").

Among the children who fit the pattern of belief reasoning (i.e., those who passed both tasks), .88 (7/8) gave belief reasoning justifications in both tasks, including [protagonist] put it in A for false-belief, and [parent] put it in B for true-belief. Among the reality-reasoners (i.e., those who failed

TABLE 8
NUMBER OF 5½-YEAR-OLDS IN EACH PATTERN OF PERFORMANCE AND REASONING STRATEGY IN LOCATION TASKS

	False-Belief	
True-Belief	Pass	Fail
Pass	8[a]	12[b]
Fail	23[c]	2[d]

Note. PAR = perceptual access reasoning.
[a]Belief reasoning.
[b]Reality reasoning.
[c]PAR.
[d]Switching = reality reasoning on false-belief task and PAR on true-belief task.

TABLE 9

Number of 5½-Year-Olds in Each Pattern of Justifications in Each Combination of Passing and Failing the False-Belief and True-Belief Tasks

		False-Belief: Pass ($N = 31$)			False-Belief: Fail ($N = 14$)			
True-Belief		Protagonist Put it in A	Mental State	Don't Know/ Irrelevant	Parent Put it in B	Mental State	Don't Know/ Irrelevant	Marginal Totals
Pass	Parent put it in B	4	0	1	7	0	0	12
	Mental state	2	1	0	0	1	2	6
	Don't know/ irrelevant	0	0	0	0	0	2	2
Pass totals		6	1	1	7	1	4	20
Fail	Protagonist put it in A	16	0	2	0	0	0	18
	Mental state	2	1	0	0	0	0	3
	Don't know/ irrelevant	2	0	0	1	0	1	4
Fail totals		20	1	2	1	0	1	25
All children		26	2	3	8	1	5	45

false- and passed true-belief), .58 (7/12) gave [parent] put it in B justifications in both tasks, referring to the object's current location.

Discussion

If children pass false-belief tasks, and also justify their answers by clearly indicating that the protagonist has a false belief, it would show that the children used belief reasoning, not PAR. We tested this possibility using 5½-year-olds, who should have the linguistic abilities to justify their correct judgments with unambiguous references to false belief.

The great majority of children who passed the false-belief location task did give the justifications that have been counted as correct in the previous studies. Almost all of those justifications were of the form, [protagonist] put it in A. However, this justification cannot be taken at face value as referring to the protagonist's false belief, because most (.62) of the children who passed false-belief and gave that justification simultaneously failed true-belief and gave the same justification, suggesting they were referring to location A now being empty.

Had 5½-year-olds justified their judgments by referring to protagonists' false beliefs, it would raise suspicion that the U-shaped developmental patterns in true-belief location (Chapter II) and in true-belief contents and identity tasks (Fabricius et al., 2010) could be false negatives due to children's pragmatic misconceptions of true-belief tasks as having hidden agendas (Oktay-Gür & Rakoczy, 2017). In the current study, few children gave justifications that suggested they were trying to rationalize why the obvious answer in the true-belief task was not the correct one. Such justifications could have included, "Sarah forgot; didn't remember; didn't see; didn't pay attention to her dad moving it" to B, or "Maybe Sarah thinks" it's in A.

Oktay-Gür and Rakoczy's (2017) argument that children have pragmatic misconceptions of true-belief tasks as having hidden agendas stemmed from their finding that children failed their true-belief location task, in which the protagonist stayed in the scene. However, the findings in Chapter II (see Figure 2a) showed that children seldom fail true-belief stay conditions, unless they are asked a look-first question. Oktay-Gür and Rakoczy asked a look-first question, which the findings in Chapter II show reduces true-belief performance even in stay conditions, in accord with PAR theory. Furthermore, highlight conditions would appear to make the true-belief task seem most obvious and arouse the most suspicions of a hidden agenda, because children are reminded before the test question that the protagonist saw the object in the true-belief location. Yet the findings in Chapter II showed that highlighting raises true-belief performance.

To return to the central issue, in the current study, only .18 (8/45) of 5½-year-olds passed both tasks. The tasks followed the standard procedure of asking children to predict where the protagonist will think the object is upon his return. Some researchers have argued that it is easier for children to attribute false beliefs when they are shown the protagonist returning and approaching the false-belief location, and are asked instead to explain why he is going to that one. If children use PAR, however, they should not be able to attribute false beliefs even when explaining the behavior of a protagonist who is acting on the basis of a false belief. We turn to this issue in Chapter VIII.

VIII. Explanation-Only False-Belief and No-Belief Tasks

Maxi comes into the kitchen and then leaves.
His mother comes in and puts his chocolate in cupboard A.
Then she moves it to cupboard B, and then she leaves.
Maxi returns to get some chocolate.
Maxi starts walking toward cupboard A.
Why is Maxi going there?

The above example is the no-belief task from Chapter V, but in this case when Maxi returns, he does not stand still while children are asked to predict where he will go. Instead, Maxi walks toward the empty cupboard, and children are asked to explain why he is going there. Why should we ask children to explain why Maxi is walking to the empty cupboard in no-belief tasks? The reason is that in false belief tasks, children often give what sound like perfectly good explanations that attribute a false belief to Maxi. PAR theory holds, however, that children do not attribute false beliefs to people. PAR theory leads us to suspect that children's explanations only sound like attributions of false belief, but are really references to Maxi getting it wrong and walking toward the empty cupboard. PAR predicts that children should give the same explanations for why Maxi is walking toward the empty cupboard in no-belief tasks. The same explanation could not refer to Maxi's false belief in one case, and his lack of belief in the other case. In the current chapter, we compare children's explanations in false-belief tasks to their explanations in no-belief tasks.

Introduction

It is usually easier to explain things after the fact than to predict them beforehand. The first suggestion that this might be the case with false beliefs came from analyses of spontaneous language production. Shatz et al. (1983), and later Bartsch and Wellman (1995), observed that children who were generally too young to pass false-belief tasks would at times apparently refer to their own past false beliefs and contrast them to the real state of affairs (e.g., "It's a bus—I thought it was a taxi."). When children made such statements, they were often explaining why they had been mistaken, and their statements resembled what adults might say when referring to their past false beliefs to explain why they had been mistaken. To study children's explanations more systematically, researchers have used what we will call explanation-only tasks. Like judgment-plus-justification tasks (Chapter VII), explanation-only tasks also have the potential to undermine PAR theory, if they reveal that at the ages when children begin to pass false-belief tasks, they reason about beliefs rather than about perceptual access.

In explanation-only tasks, the agent is shown acting on the basis of a false belief, thus removing the need for children to first predict what the agent will think or do. This seems to be more akin to how children use their ToM abilities in the real world (Bartsch & Wellman, 1989). In an explanation-only contents false-belief task, for example, the newcomer approaches a Smarties box that children know is empty, and they are simply asked, "Why is [newcomer] going to that box?" Bartsch, Campbell, et al. (2007) modified the explanation-only method in an attempt to resolve a long-standing controversy about whether explanation tasks elicited earlier false-belief attributions than prediction tasks (Bartsch, 1998; Bartsch & Wellman, 1989; Dunn et al., 1991; Moses & Flavell, 1990; Perner, 1989; Robinson & Mitchell, 1995; Wellman & Banerjee, 1991; Wellman & Bartsch, 1989; Wimmer & Mayringer, 1998a, 1998b). Bartsch, Campbell, et al. (2007) asked children three times, rather than only once, "Why (else) do you think [agent] is doing that?" Contrary to Perner and Horn's (2003) view that repeated questioning about a protagonist's mental states confuses children, Bartsch, Campbell, et al. (2007) reasoned that repeated questioning would give children, who first might refer to the agent's desires, a second and third opportunity to refer instead to the agent's false belief.

In all these versions, correct explanations include, "Because he thinks Smarties are in there," and "Because he thinks it's there." References to think, along with the other types of explanations that researchers have counted as correct, have face validity as attributions of false beliefs. In this chapter we test whether the explanations have construct validity as attributions of false beliefs. We used a comparison task, as we did in Chapter VII, to examine the pattern of responses. In this case we used no-belief comparison tasks, rather than true-belief comparison tasks. If explanations are valid attributions of false beliefs, then those explanations should be constrained to agents who actually have false beliefs, and should differ from explanations of the actions of agents with no beliefs.

Explanations that refer to what the agent thinks can be ambiguous between belief reasoning and PAR. Perner (1989, 1991) pointed out that references to thinking can refer either to falsely believing something, or to guessing. Wellman and Bartsch (1988) found that 4-year-olds interpreted a statement such as, "She thinks the book is in there" to also refer to wanting the book. PAR theory suggests that children use think to mean get it wrong, which Hogrefe et al. (1986; footnote 1) and Friedman and Petrashek (2009) also suggested. A clear example occurred when the first author's 3-year-old son laughed one morning while getting dressed and said, "I thought my bed was my pants!" He said this as his foot landed on the bed after missing the pant leg held out for him. This was not a reference to a false belief. It was his description of mistaken behavior, in which he had acted incorrectly toward his bed as if it were his pants. Even adults at times use mental verbs to refer to getting it wrong. For example, after having told someone in good faith that it is raining and then discovering that the drops on the window are from the sprinkler, adults sometimes say, "I lied that it was raining."

We tested 5- and 6½-year-olds to avoid a concern that the task demands of giving explanations might limit the ability of younger children "to construct a statement of their inference about the epistemic state of the character (e.g., "because she doesn't know where the book is now")" (Bartsch, 1998, p. 425). These two ages would also include some children who used belief reasoning. We employed both contents and locations tasks, and the Bartsch, Campbell, et al. (2007) repeated-question method. We followed the seminal Bartsch and Wellman (1989) procedures in two respects. First, we coded explanations into many categories to provide a sensitive test of whether explanations differed across tasks. Categories included expressions of uncertainty that should be used in the no-belief tasks, plus all the other categories that have been coded by previous researchers in false-belief location and contents tasks. Second, we included a group of adults for comparison. In our case, this was to determine whether there was a degree of ambiguity also present in their explanations, given the different meanings of the word think in adult usage.

Method

Participants

Participants were twenty-three 5-year-olds (53–66 months; $M = 60$; 15 boys), twenty-three 6½-year-olds (67–90 months; $M = 77$; 15 boys), and fourteen adults (20–26 years; $M = 22$; 3 boys).

Procedures

Each participant received four explanation-only tasks (false-belief and no-belief location, false-belief and no-belief contents) in one of five random orders, under the constraint that the first two tasks contained one false-belief and one no-belief task. If participants answered any control questions incorrectly, the story was re-told, and they were asked the controls again. If they were still wrong, they were corrected, and the experimenter continued with the story. Explanations were elicited using Bartsch, Campbell, et al.'s (2007) three explanation questions. For example, in the false-belief location task, (1) "Why is Jaime going to the blue cupboard? … That was a great answer." (2) "Why else is Jaime going to the blue cupboard? … What a great answer." (3) "Can you give me another one? Why else is Jaime going to the blue cupboard?" The first two explanations were labeled as "great answers" (for children only) to dissuade them from perceiving the repeated questions as criticism. Unlike Bartsch, Campbell, et al. (2007), however, we always asked all three explanation questions, and did not stop and move on to the next task when the participant gave what the experimenter considered to be a belief explanation. This ensured the same number of questions in each task, and postponed coding until later.

In the location tasks, the protagonist was described as wanting the chocolate or toy, in order to steer children away from desire explanations (e.g., "because she wants to get her chocolate"), and to encourage reference to beliefs, as did Wimmer and Mayringer (1998a). In the false-belief contents task we used an empty, familiar candy box, and in the no-belief contents task we used a plain white box that initially held a toy but was empty when the newcomer first appeared. Thus, in both contents tasks, the newcomer approached an empty box, and the experimenter had earlier mentioned one thing when showing the box and its contents to the participant (i.e., candy in false-belief, and toy in no-belief. Specific stories and questions used in each of the tasks follow.

False-Belief Location Task

Jaime and mom came back from the store with chocolate. Jaime asked mom to put the chocolate away, and watched her put it in location A. Jamie left, mom moved it to location B, and then mom left. "Where did mom put the chocolate first?" [CQ1] "Did Jaime see her put it in the [first] one?" [CQ2] "Where did mom put the chocolate next?" [CQ3] "Did Jaime see her put it in the [second] one?" [CQ4] Jaime returned, participants were told Jaime wanted the chocolate, and Jaime was shown walking toward location A, stopping within six inches of the box. Participants were asked the explanation questions.

No-Belief Location Task

Amy and her brother (or Andy and his sister) came back from the store with a new toy. Amy asked her brother to put the toy away, and left. He first put it in location A, then moved it to location B, and then he left. "Where did brother put the toy first?" [CQ1] "Did Amy see him put it in the [first] one?" [CQ2] "Where is the toy now?" [CQ3] "Did Amy see her brother put it in the [second] one?" [CQ4] Amy returned, and the task proceeded as in the false-belief version.

False-Belief Contents Task

Participants were shown a Dots candy box and were asked what they thought was inside. All but three children responded Dots or candy, and the others were prompted until they did so. The experimenter showed children that the box was empty, then closed the box. "What kind of box is this?" [CQ1] "What is in here now?" [CQ2] The newcomer (Dylan) was introduced, and participants were told that Dylan had been playing guitar outside and had not seen what was inside the box. Dylan was shown walking toward the box and stopping within six inches of it. Participants were asked the explanation questions.

No-Belief Contents Task

Participants were shown a plain, white box, and were asked if they knew what was inside. About half said they did not know, and the rest took a guess. The experimenter opened the box to reveal a small toy inside, then took the toy out, put it away, and closed the box. "What was in here first?" [CQ1] "What is in here now?" [CQ2] The newcomer (Bobby or Betty) was introduced, and the task proceeded as in the false-belief version.

Coding

Explanations were coded into three general categories: Explicit false-belief, implicit false-belief, and nonbelief. Implicit false-belief explanations were coded into three subcategories: Desire for specific content, ignorance of critical event, and critical evidence. Nonbelief explanations were coded into seven subcategories: General desire, desire to know, uncertainty, general ignorance, reality, knowledge of reality, and idiosyncratic. Table 10 shows the categories, subcategories, and examples in each set of tasks.

TABLE 10
EXAMPLES OF CURRENT EXPLANATION CODING CATEGORIES BY TASK TYPE AND NOTES SHOWING LINKS TO PRIOR RESEARCHERS' CODES

Category	Contents False-Belief and No-Belief	Location False-Belief and No-Belief
Explicit false-belief	Newcomer (N) thinks [that] [candy/toy] is in there[a]	Protagonist (P) thinks [that] it is in there[a,b]
Implicit false-belief		
Desire for specific content	[N wants to/To] [get/play with/ eat] [candy/toy][c]	
Ignorance of critical event	N not [see/know] [candy/toy] [is gone/was moved][d,e]	P not [see/know] [mom/sibling] [move/moved] it[d] [It was there but] P not know where it is now[f] P not [know/think] it's in the other one[a,g]
Critical evidence	It's a candy box	P saw [mom put] it there [first/ before][h] [(Mom/Sibling) put it/It was] there [first/before][i]

(*Continued*)

TABLE 10. (*Continued*)

Category	Contents False-Belief and No-Belief	Location False-Belief and No-Belief
Nonbelief		
General desire	[N wants to/To] get what's inside[j]	[P wants to/To] [get/play with/ eat] [it/toy/chocolate][j]
Desire to know	[N wants to/To] [see/know] [if anything/what] is inside	[P wants to/To] [see/know] [if it's there/where it is]
	N [wonders/is curious about] what's inside	[P (wants/is trying) to/To] find it
Uncertainty	[N (guesses/thinks maybe)/ Maybe]	[P (guesses/thinks maybe)/ Maybe] it's there
	[something/candy/toy] is in there	P randomly picks that one
	[N wants to/To] see if [candy/ toy] is in there	
General ignorance	N not [see/know] [(if) it's empty/what's inside]	P not [see/know] where [it is/ (mom/sibling) put it]
	N not know [where/if] [candy/ toy] [is/is inside]	
Reality	[There's nothing/(Candy/Toy) is not] in there[k]	[It's/(Mom/Sibling) put it] [not there/in the other one][k]
Knowledge of reality	N thinks [nothing is/(candy/ toy) is not] in there	P thinks it's [not there/in the other one]
Idiosyncratic	includes don't know and no response[l]	includes don't know and no response[l]

[a]Belief attributions ("false belief") in Moses and Flavell (1990) and Bartsch, Campbell, et al. (2007).
[b]Belief attributions ("epistemic reference") in Wimmer and Mayringer (1998a), and ("belief") in Clements and Perner (1994). Wimmer and Weichbold (1994) found no references to "thinking."
[c]Not a belief attribution in Moses and Flavell (1990), because they told children the protagonist wanted "Band-Aids."
[d]Belief attribution ("ignorance," and "lack of perceptual access to critical event") in Moses and Flavell (1990).
[e]Not a belief attribution ("ignorance") in Bartsch, Campbell, et al. (2007).
[f]Belief attribution ("epistemic reference") in Wimmer and Mayringer (1998a).
[g]Belief attribution ("belief") in Clements and Perner (1994).
[h]Belief attributions ("explicit reference to informational conditions") in Wimmer & Weichbold (1994), and ("epistemic reference") in Wimmer and Mayringer (1998a).
[i]Belief attributions ("implicit reference to informational conditions") in Wimmer & Weichbold (1994), and ("earlier location") in Wimmer and Mayringer (1998a). Implied belief attribution ("past deed") in Clements and Perner (1994).
[j]Not a belief attribution in Moses and Flavell (1990), Wimmer and Mayringer (1998a), Clements and Perner (1994), or Bartsch, Campbell, et al. (2007).
[k]Not a belief attribution ("reality") in Wimmer and Mayringer (1998a), who considered it absurd, or ("outcome related") in Moses and Flavell (1990).
[l]All previous researchers identified an Idiosyncratic category, and none counted those explanations as belief attributions.

Explicit False-Belief Explanations

This category is composed solely of the explanation, "[Agent] thinks (or, in one case each, knows or is confident)…" followed by a proposition that the object is in the empty container or location. If explicit false-belief explanations are given in no-belief tasks as well as in false-belief tasks, the explanations could reflect PAR use of the word, “think,” to mean get it wrong.

Implicit False-Belief Explanations

Desire for specific content. Explanations that are coded as desire for specific content are those in which the participant says that the agent wants the object that is in the box or at that location. In the false-belief contents task, the desire for specific content explanation, “[Newcomer] wants the candy,” can imply that the newcomer has a false belief that the Dots box contains candy. However, in the no-belief contents task, the newcomer cannot want the unknown toy that is inside the plain box. If desire for specific content explanations are given in no-belief contents tasks as well as in false-belief contents tasks, the explanations could reflect participants thinking about reality rather than belief.

In the false-belief location task, the desire for specific content explanation, “[Protagonist] wants the chocolate,” does not imply that the protagonist has a false belief that the chocolate is at that location, because participants were told that the protagonists want the objects. Thus, in the location tasks, desire for specific content explanations were coded into the general desire subcategory of nonbelief explanations (see below).

Ignorance of critical event. Explanations that are coded as ignorance of critical event are those in which the participant describes the agent as not seeing or not knowing the information that leads to the false belief. In the false-belief contents task, “[Newcomer] doesn’t know the candy is gone” can imply that the newcomer has a false belief that the Dots box contains candy. However, in the no-belief contents task, the newcomer not seeing or knowing that the toy was removed from the plain box would not lead to a false belief that it was still there, because the newcomer had not seen it there before. If ignorance of critical event explanations are given in both versions of the contents task, the explanations could reflect PAR rather than attribution of false belief.

Similarly, in the location tasks, the ignorance of critical event explanation, “[Protagonist] did not see mom move it,” can imply that the protagonist has a false belief that it is in the first location only if the protagonist had seen it there. If ignorance of critical event explanations are given in both versions of the location tasks, the explanations could reflect PAR rather than attribution of false belief.

Critical evidence. Explanations that are coded as critical evidence are those in which the participant describes the agent acquiring the information that leads to the false belief. In the false-belief contents task, the critical evidence is the familiar labeling on the box. There is no critical evidence in the no-belief contents task, because it is a plain box.

There are two types of critical evidence explanations in the false-belief location task. First, critical evidence explanations that cite the protagonist witnessing the initial placement of the chocolate (e.g., "[Protagonist] saw mom put it there.") imply that the protagonist has a false belief that it is still there. There is no critical evidence in the no-belief location task, because the protagonist does not witness the initial placement of the toy. Second, critical evidence explanations that cite only the initial placement, not the protagonist witnessing the placement (e.g., "[Mom] put it there first."), can be shorthand references to false belief (Chapter VII), but only if such explanations are not also given in the no-belief location task; otherwise, they could reflect PAR references to the location being empty and the protagonists getting it wrong.

NonBelief Explanations

General desire. General desire explanations in the contents tasks do not name the object, but refer to newcomers wanting whatever is in the box. As discussed above, desire for specific content explanations in the location tasks were also put in this subcategory.

Desire to know, uncertainty, and general ignorance. All of these explanations only refer to not seeing or to not knowing.

Reality and knowledge of reality. Reality explanations state that the agent is going to the box, or to that location because it is empty, and knowledge of reality explanations state that the agent is going there because he thinks it is empty. Wimmer and Mayringer (1998a) considered reality explanations absurd, but reality explanations could reflect PAR references to getting it wrong.

Idiosyncratic. Idiosyncratic explanations included story-telling about why the agent might go there; responses in which participants say they do not know; and cases in which participants provide no response at all.

Table 10 also shows how each category was coded in the false-belief contents and location tasks that have been used in the five previous studies (no previous study included no-belief tasks). All previous researchers coded explicit false-belief explanations as belief attributions, and almost all coded the three subcategories of implicit false belief as belief attributions. No previous researchers counted any of the nonbelief categories as belief attributions.

Participants occasionally gave a compound explanation that included either an explicit or implicit false belief explanation plus a nonbelief explanation. In such cases, we coded it as a belief explanation, as did Bartsch and Wellman (1989). If participants gave both an explicit and implicit false belief explanation, we coded it as an explicit belief explanation in order to obtain more data on participants' use of the word, "think," In all other compound explanations, we coded whichever response was given first. Two authors developed the coding system above, and then one of these two coded the explanations. A research assistant was trained on the coding system and coded a randomly selected 23% of the child explanations into the 11 categories. The coders agreed on 96% of their codes, and disagreements were resolved by discussion.

Results

In both versions of the contents tasks, the first time each control question was asked, at least .93 of children answered it correctly. By the second presentation, at least .98 of children answered all questions correctly. On both versions of the location tasks, the rates were .80 and .91 for the first and second presentation, respectively.

We counted both explicit and implicit false belief explanations as false-belief explanations. A majority of children (.70 of 5-year-olds and .78 of 6½-year-olds), and all adults gave a false-belief explanation in one or both false-belief tasks. This result replicated previous findings. Wimmer and Mayringer (1998a) also asked multiple explanation questions, and found that .70 of 4½- to 5½-year-olds, and .80 of 5½- to 6½-year-olds gave a false-belief explanation in one or both of two false-belief location tasks. Bartsch, Campbell, et al. (2007; Study 3, standard action explanation condition) found that .60 of 3½- to 7-year-olds gave a false-belief explanation in one or both of two false-belief identity tasks.

Despite the frequency of explanations that would appear to be references to false belief, most participants at all ages, .57, .74, and .64, respectively, also gave explicit and implicit false-belief explanations in one or both of the no-belief tasks. Table 11 shows the number of individuals who gave false-belief explanations in both, neither, or only one version of each task in response to the three explanation questions. The lower left cell in each task at each age shows how many constrained their false-belief explanations to the agents who actually had false beliefs. Averaged across ages and tasks, only .18 of children constrained their false-belief explanations to agents with false beliefs, and only .57 of adults did so.

The findings were unchanged when we examined responses to the first explanation question in each task. Table 12 shows the number of individuals who gave a false-belief explanation in both, neither, or only one version of each task in response to the first question. In this case, .15 of children and .64 of adults constrained their false-belief explanations to agents with false beliefs.

The failure to constrain false-belief explanations is also clear for the 11 categories of explanations. Table 13 shows the proportion of children in each task who gave at least one explanation in each category across the three questions. The similarity between the false- and no-belief versions of each task is evident in every category. The similar rates at which explicit false-belief explanations were applied to agents with false beliefs and agents who

TABLE 11
Number of Individuals Giving ≥1 Versus 0 False-Belief Explanations in Each Pair of Tasks in Response to the Three Explanation Questions

		False-Belief			
		Contents		Location	
		# False-Belief Explanations			
Age	No-Belief	≥1	0	≥1	0
5 years	≥1	7	2	8	1
	0	7	7	2	12
6½ years	≥1	9	4	8	3
	0	3	7	5	7
Adults	≥1	5	0	6	0
	0	8	1	8	9

were simply ignorant shows that "Because [agent] thinks…" cannot be taken uncritically as an attribution of false belief.

Table 14 shows the corresponding proportions for adults. Reflecting the ambiguity of the word think to refer to both believe and guess (Perner, 1989, 1991), .36 of adults gave explicit false-belief explanations in the no-belief location task, and .14 did so in the no-belief contents task. Adults most likely used think to mean guess in the no-belief tasks, which is consistent with the fact that similar percentages gave uncertainty explanations in those tasks.

TABLE 12
Number of Individuals Giving 1 Versus 0 False-Belief Explanations in Each Pair of Tasks in Response to the First Explanation Question

		False-Belief			
		Contents		Location	
		# False-Belief Explanations			
Age	No-Belief	1	0	1	0
5 years	1	4	3	4	1
	0	1	15	5	13
6½ years	1	6	4	5	2
	0	3	10	5	11
Adults	1	2	0	3	0
	0	7	5	11	0

TABLE 13
PROPORTION OF CHILDREN IN EACH TASK WHO GAVE AT LEAST ONE EXPLANATION IN EACH CATEGORY IN RESPONSE TO THE THREE EXPLANATION QUESTIONS

	Contents		Location	
Category	False-Belief	No-Belief	False-Belief	No-Belief
Explicit false-belief	.20	.15	.39	.33
Implicit false-belief				
Desire for specific content	.39	.33		
Ignorance of critical event	.00	.04	.07	.07
Critical evidence	.02		.15	.07
Nonbelief				
General desire	.09	.07	.54	.48
Desire to know	.41	.33	.13	.28
Uncertainty	.17	.20	.00	.04
General ignorance	.13	.15	.07	.13
Reality	.17	.24	.28	.26
Knowledge of reality	.02	.02	.07	.04
Idiosyncratic	.46	.39	.41	.37

Note. Empty cells indicate explanation categories not applicable in those tasks. Proportions would sum to 3 in each task if each participant gave a different category of explanation in response to each of the three explanation questions in that task, but some gave the same explanation category in response to more than one explanation question.

Discussion

If children at the ages when they begin to pass false-belief tasks were able to explain the actions of protagonists by referring to their false beliefs, it would show that the children used belief reasoning, not PAR. We conducted a stringent test of this potential outcome by testing children older (5- and 6½-year-olds) than typically tested on explanation-only tasks, to ensure that they would have the linguistic ability to answer open-ended questions about the epistemic states of agents.

The findings paralleled those of Chapter VII. Most children did in fact give an explicit or implicit false-belief explanation in at least one of the two false-belief tasks; however, most children also gave those same explanations in the no-belief tasks as well. Less than .20 of children constrained their false-belief explanations to agents who actually had false beliefs. Children's blanket use of explanations that sound like attributions of false beliefs shows that those explanations cannot be taken at face value as referring to agents' false beliefs.

Children could have demonstrated some awareness of protagonists' false beliefs by simply giving different explanations for the protagonists who had no beliefs. To cast a wide and fine net for any different explanations, we coded explanations into more categories than have been used in any previous study, included both location and contents tasks, and used the repeated-question method of eliciting explanations, which has been argued to be the most sensitive method (Bartsch, Campbell, et al., 2007). Nevertheless,

TABLE 14

PROPORTION OF ADULTS IN EACH TASK WHO GAVE AT LEAST ONE EXPLANATION IN EACH CATEGORY IN RESPONSE TO THE THREE EXPLANATION QUESTIONS

	Contents		Location	
Category	False-Belief	No-Belief	False-Belief	No-Belief
Explicit false-belief	.57	.14	.50	.36
Implicit false-belief				
Desire for specific content	.79	.29		
Ignorance of critical event	.00	.00	.36	.14
Critical evidence	.21		.93	.07
Nonbelief				
General desire	.07	.07	.50	.29
Desire to know	.50	.93	.00	.07
Uncertainty	.07	.14	.00	.29
General ignorance	.00	.14	.00	.07
Reality	.00	.00	.07	.00
Knowledge of reality	.00	.00	.00	.00
Idiosyncratic	.36	.57	.36	.71

Note. Empty cells indicate explanation categories not applicable in those tasks. Proportions would sum to 3 in each task if each participant gave a different category of explanation in response to the three explanation questions in that task, but some gave the same explanation category in response to more than one explanation question.

children did not give any substantially different explanations for protagonists with false versus no beliefs, but in both cases equally referred to want, not see, not know, or get it wrong, all consistent with PAR. Even the rates of idiosyncratic explanations did not differ between false-belief and no-belief tasks.

These findings are partly due to children's use of PAR, but also partly due to the inherent ambiguity of explanations. The ambiguity of the word "think" (Perner, 1989, 1991) is reflected in the fact that about half of adults gave explicit false-belief explanations in the false-belief tasks, but substantial minorities also did so in the no-belief tasks, indicating that they sometimes used think to mean guess. Some adults also gave explanations (i.e., desire for specific content, and ignorance of critical event explanations) that seemed to attribute unwarranted false beliefs. Relatedly, Wertz and German (2007) found that adults sometimes erred when asked to endorse or reject alternative explanations for the protagonist's action in a false-belief location task. These findings clearly underestimate the rate of belief reasoning in adults, and most likely also in 6½-year-olds, and give warning that the explanation-only methodology should not be used to identify individuals who use belief reasoning.

In Chapter IX, we took what we learned from children's true-belief and false-belief performance, and from children's justifications and explanations to construct the BUS, an individual difference measure to assess the development of representational ToM. Our findings suggested that the scale should use a true-belief contents task and a standard false-belief contents task, and that measurement should reflect both performance and justifications. In contents tasks, the justification question, “Why does she think that candy is in the box?” pulls for the informational condition that led to the belief. The correct, belief justification, “Because it’s a candy box,” is a linguistically simple statement, but it shows the child reasoning about appearances, and is an unambiguous reference to the newcomer’s mental representation of the unseen contents of the box.

IX. Validation of the Belief Understanding Scale (BUS)

Show the child that a Crayons box contains a toy car.
Put the car back in and close the box.
Ask the child, "If someone just looks at the box and doesn't open it,
What will they think is inside? Why will they think that?"

Show the child that an M&Ms box contains a key.
Remove the key, pour M&Ms into the box, and close the box.
Ask the child, "If someone just looks at the box and doesn't open it,
What will they think is inside? Why will they think that?"

The two tasks shown above make up the BUS that we developed from the findings in the previous chapters. The first task is a contents false-belief judgment-plus-justification task. The second is a contents true-belief judgment-plus-justification task. The BUS identifies four types of reasoning about beliefs: Reality reasoning, switching between reality reasoning and PAR, PAR, and belief reasoning. In the current chapter, we test the predictive validity of the BUS against the predictive validity of the false-belief task in accounting for aspects of children's social development, language development, and kindergarten adjustment.

Introduction

The work of the previous chapters would be of little importance if children's acquisition of PAR does not help them develop new skills that represent more than what they can do with reality reasoning, but less than what they will later be able to do with belief reasoning. If PAR does not make some difference, it might be nothing more than a waystation of temporary confusion, or misapplication of rules or word meanings. We would risk having spent a lot of time investigating a task, without learning any more about how children develop an understanding of mind, and about what that means for their development more broadly. In the present chapter, we tested whether a scale that identifies children who use reality reasoning, PAR, and belief reasoning, would account for more developmental variation in some of the social, regulatory, and academic skills that are associated with ToM than would the traditional workhorse, the false-belief task.

The BUS

The BUS uses judgments-plus-justifications in a true-belief contents task and a false-belief contents task. The BUS is summarized in Table 15. Judgment is the primary criterion. Children who—on the basis of the primary criterion of their judgments—pass the true-belief and fail the false-belief task are coded as using reality reasoning, and those who fail the true-belief and pass the false-belief task are coded as using PAR. Justification is the secondary criterion, and comes into play to identify belief reasoning. Children who provide correct judgments on both tasks and give belief justifications in both tasks are coded as using belief reasoning. Evidence is presented in the Results section to justify the criterion of two belief justifications for belief reasoning. We note here that the criterion seems reasonable, because in the study reported in the current chapter, few children who passed both tasks gave only one belief justification (i.e., only .14 (5/36) at age 4½, and .07 (6/89) at 6).

The BUS also identifies children in the transitional phase from reality reasoning to PAR. Children in transitional phases typically switch back-and-forth between the old strategy and the new strategy (Adolph et al., 2018; Siegler, 1996). Children in transition between reality reasoning and PAR will show up as using reality reasoning on the true-belief task and PAR on the false-belief task, or vice versa. Children who switch from PAR on true-belief to reality reasoning on false-belief are those who fail both tasks. Children who switch from reality reasoning on true-belief to PAR on false-belief are those who pass both tasks and do not give belief justifications in both, but in only one task or in neither task.

TABLE 15

Number of Children Tested at Age 4½, and Number Retested at Age 6, who Were Classified at Each Level of the Belief Understanding Scale, and the Criterion Pattern of Judgments-Plus-Justifications for Each Level

	Ages		Contents True-Belief		Contents False-Belief	
Belief Understanding Scale	4½	6	Judgment	Justification	Judgment	Justification
Reality reasoning	93	16	Pass		Fail	
Switching RR–PAR	21	7	Pass	[a]	Pass	[a]
Switching PAR–RR	13	12	Fail		Fail	
PAR	19	27	Fail		Pass	
Belief reasoning	15	82	Pass	Belief	Pass	Belief
Total at each age	161	144				

Note. Example of how to read the table: At age 4½, 93 out of 161 children were classified as using reality reasoning because they passed the true-belief task and failed the false-belief task; at age 6, 16 out of the 144 returning children were classified as using reality reasoning. Empty justification cells indicate that the pattern of judgments alone is sufficient to classify children at that level.
Switching RR–PAR = reality reasoning on the true-belief task and PAR on the false-belief task; Switching PAR–RR = PAR on the true-belief task and reality reasoning on the false-belief task.
[a]Belief justifications on neither or one, but not both tasks.

Research has shown positive relations between false-belief performance and social skills (e.g., Weimer & Guajardo, 2005), and between false-belief performance and inhibitory control skills (e.g., Carlson et al.,1998). Furthermore, theorists have discussed the transactional nature of these relations. Inhibitory control of prepotent, dominant responses can promote ToM, and ToM can enhance inhibitory control skills (Carlson & Moses, 2001; Perner & Lang, 1999; Russell, 1996). Similarly, ToM predicts later social skills (Devine et al., 2016; Eggum et al., 2011), and positive social interactions can enhance ToM (Carpendale & Lewis, 2006). Adjustment to kindergarten, an important milestone that predicts many later milestones (Romano et al., 2010), is at least partly due to better social skills (Denham & Brown, 2010) and inhibitory control (Allan et al., 2014), and so it too should be related to ToM.

PAR should be associated with distinct differences in social skills, inhibitory control, and kindergarten adjustment. PAR is an advance over reality reasoning because it allows children to determine whether someone knows or does not know, and to predict whether the person will act correctly or incorrectly. Reasoning about others' knowledge requires inhibiting reality-based judgments and predictions. The ways in which PAR would lead to advances in social skills include the following: Computing a listener's knowledge and ignorance enriches communication; reasoning that hiding something or concealing information will prevent someone else from knowing about it enables deception; and reasoning that a crucial piece of unknown information will allow someone to succeed enhances helping. Belief reasoning is an advance over PAR because it allows children to attribute mental representations. Belief reasoning requires greater inhibitory control to go beyond reasoning about what someone has perceptual access to in an immediate situation, to reasoning about what someone believes about the situation, in turn enabling sophisticated communication, deception, and helping.

In all tests, we controlled for expressive and receptive vocabulary development, and family SES. Early vocabulary predicts later social skills (Izard et al., 2001; Mostow et al., 2002) and school achievement (Ramsook et al. 2019), for children in both low- and middle-income families (Ackerman et al., 2003). Language skills also predict false-belief task performance, and moderate relations between false-belief performance and social competence (Astington, 2003; Astington & Jenkins, 1999; DeRosney & Hughes, 2006; de Villiers & de Villiers, 2000; de Villiers & Pyers, 2002; Hale & Tager-Flusberg, 2003; Jenkins & Astington, 1996; Watson et al., 2001). We were not able to control for communication skills (Ramsook et al., 2019) or syntactic development (de Villiers & de Villiers, 2000), although it will be important to do so in future studies for insight into the transition from PAR to belief reasoning.

Su et al. (2019) found modest positive relations between SES and social competence in kindergarten. Some researchers have found positive relations between SES and false-belief performance (Cole & Mitchell, 1998; Holmes et al., 1996; Holmes-Lonergan, 2003), even after controlling for language development (Cutting & Dunn, 1999; Weimer & Guajardo, 2005). Others have not found relations between SES and false-belief (Garner et al., 2005; Murray et al. 1999; Pears & Moses, 2003; Ruffman et al. 1999), and some have only found relations prior to controlling for language (Hughes et al., 1999).

Method

Participants

Children were tested within 2 months of turning 4½ (54 months), and again within two months of turning 6 (72 months). At age 4½, 168 children (79 girls), 189 mothers, and 145 nonparental caregivers participated. Mothers reported that .85 of children were non-Hispanic Caucasian, .13 were Hispanic, and .02 were African American, Native American, Asian, or Pacific Islander. Mothers reported average annual family income was $60,000–$75,000. Nearly all mothers (.89) were employed at least 10 hr per week (.66 full-time), as were .99 of fathers (.93 full-time). Most (.77) were married. At age 6, when most children were in the second semester of their kindergarten year, 150 children (69 girls), 160 mothers, and 140 teachers participated.

Recruitment

Participants were part of a longitudinal study of emotional development from birth to 7 years under the direction of Eisenberg and Spinrad. Mothers were recruited from hospital maternity wards in the Phoenix, Arizona metropolitan area. Eligibility criteria included full-term birth, no serious medical conditions, parents over the age of 18 expecting to stay in the area at least two years, and ability to read questionnaires proficiently in English. Procedures were approved by the university IRB for data collection between 2003 and 2005.

Recruitment initially yielded 352 mothers, although not all could be contacted later, and some could not commit to the longitudinal study when contacted. Researchers collected questionnaire data from mothers and caregivers or teachers every six months thereafter, and data from children when they were 18 months old ($N = 247$), 30 months ($N = 216$), 42 months ($N = 192$), 54 months ($N = 168$), and 72 months ($N = 150$).

Attrition

Families who participated at both ages 4½ and 6 ($N = 171$) were compared to those who attritted ($N = 27$). A multivariate analysis of variance on mothers' and fathers' highest level of education, family income, and all study variables at age 4½, and a χ^2 test on family ethnicity found no significant differences. Compared with families who participated at 18 months and 4½ years ($N = 191$), mothers who attritted ($N = 71$) had less education than those who remained in the study, $F(1, 238) = 4.69$, $p < .05$. Those who attritted between 18 months and 6 years ($N = 90$) had lower father and mother education, Fs$(1, 239;\ 1, 238) = 4.85$ and 8.89, ps $< .05$ and $.01$ for fathers' and mothers' education respectively, and lower income than those who remained in the study, $F(1, 241) = 7.76$, $p < .01$.

Procedures

At age 4½, data were collected in the laboratory, and at age 6 in the families' homes. All procedures were videotaped. A graduate student and a postdoctoral fellow supervised trained research assistants. Sessions lasted about 1.5 hr, and included approximately 20 tasks. Mothers completed questionnaires during the session and provided consent to contact nonparental caregivers or teachers; nonparental caregivers and teachers completed questionnaires via mail.

Belief Tasks

The belief tasks were those used in Study 3 of Fabricius et al. (2010), except that in the true-belief task here, the object that was initially in the box was removed from view after, rather than before, the two control questions. At age 4½, one task immediately followed the other in counterbalanced order. At age 6, four orders counterbalanced which task came first, and whether the second followed either immediately or after two other tasks. If children failed to say they thought that the typical contents were in the box, they were prompted until they did so. If they failed a control question, they were corrected and asked again. If they said, "I don't know" or "both" in response to the belief question, they were asked to make a choice; if they did not, it was scored as incorrect. The same materials were used at both ages for false-belief, but new materials (a Band-Aids box, a Band-Aid, and a pencil) were used for true-belief at age 6.

False-belief contents task. Children were shown a "Crayons" box with a toy car inside, and were asked what they thought was inside. After responding "crayons" (or "colors" or "markers"), they were shown that the box contained the car, which was removed, given to them to handle for a moment, and then replaced into the box. "What kind of box is this?" [CQ1] "What is inside the box now?" [CQ2] "Let's pretend I have a friend named Suzie waiting right outside the door. She's never seen inside this box. When she first looks at the box, before she opens it, will she think there [are crayons; is a car, counterbalanced] inside?" [Belief Question] "Why will she think there [are crayons; is a car] inside?" [Justification Question].

True-belief contents task. Children were shown a familiar candy box with a key inside, and were asked what they thought was inside. After responding "candy," they were shown that the box contained a key, which was removed, given to them for a moment, and then placed next to the box. The experimenter brought out a clear plastic cup of the candy and said, while pouring it into the box, "Here, let's put some candy inside." "What did we put inside the box?" [CQ1] "What was inside the box first?" [CQ2] Then the experimenter removed the key from view. "Let's pretend I have a friend named Sam waiting right outside the door. He's never seen inside this box. When he first looks at the box, before he opens it, will he think there is [candy; a key, counterbalanced] inside?" [BQ] "Why will he think there is [candy; a key] inside?") [JQ].

Justification Coding

Two authors coded all justifications for belief justifications. Belief justifications required passing the belief question and referring to the appearance of the container (e.g., "It's a Band-Aids box.") to justify why the protagonist would think the typical contents were inside. All disagreements were resolved by discussion. A research assistant was trained on the coding criteria, and interrater reliability was calculated on a sample of 50 children at each age. Belief justification coding was reliable on both tasks at both ages (κs $\geq$ .90). For the justification code instructions, see the Supporting Information, *Chapter IX: Justification Codes for Contents Tasks.*

Social Skills

At age 4½, social skills were assessed with the compliance subscale (8 items, e.g., "Follows rules") of the Infant-Toddler Social and Emotional Assessment (ITSEA; Carter et al., 1999; Carter et al., 2003). Mothers ($\alpha = .75$) and nonparental caregivers ($\alpha = .78$) rated children on a 3-point scale from "not true" to "somewhat true or sometimes true" to "very true or often true." At age 6, social skills were assessed with the social competence subscale (10-items, e.g., "Shares toys with others") of the Social Competence and Behavior Evaluation, Short Form (SCBE, LaFreniere & Dumas, 1996). Mothers ($\alpha = .83$) and teachers ($\alpha = .93$) rated children on a 6-point scale, ranging from "never" to "always." At each age, if both observers provided data, we calculated the mean of the standardized ratings ($r = .31$, $N = 124$ and $r = .27$, $N = 117$, respectively, ps $< .01$).

Inhibitory Control

At both ages, children were assessed on the inhibitory control subscale (13 items; e.g., "This child can wait before entering into new activities if she or he is asked to.") of the Child Behavior Questionnaire (CBQ; Rothbart et al.,2001). Mothers (age 4½, $\alpha = .80$; age 6, $\alpha = .89$) and nonparental caregivers or teachers (αs = .84 and .87) rated children on a 7-point scale from "extremely untrue" to "extremely true." If both observers provided data, we calculated the mean of the standardized ratings ($r = .38$, $N = 127$ and $r = .40$, $N = 119$, respectively, ps < .01).

Kindergarten Adjustment

At age 6, kindergarten teachers completed the Teacher Rating Scale of School Adjustment (TRSSA; Ladd et al., 1999; Ladd & Burgess, 2001), including Cooperative Participation (e.g., "Listens carefully to teacher's instructions and directions") and Independent Participation (e.g., "Works independently"; Buhs & Ladd, 2001) on a 3-point scale from "does not apply" to "certainly applies." The mean was 1.51, and $\alpha = .95$.

Language Development

Children were administered the Wechsler Preschool and Primary Scale of Intelligence (WPPSI; Sattler & Dumont, 2004). At age 4½, expressive and receptive vocabulary scaled scores were computed from the raw scores according to the WPPSI manual, and were averaged to create a single vocabulary score. The expressive vocabulary scale includes 25 possible items (5 picture and 20 verbal). Testing began at the verbal items. If children failed the first or second verbal item, the picture items were used. The receptive vocabulary scale includes 38 possible items, and testing began at item 6. At age 6, receptive vocabulary scaled scores were computed from the raw scores. Testing began at item 6 for children who had not had their 6^{th} birthday, and at item 16 for children who were 6 years or older.

SES

At age 4½, mothers reported the family income on a 7-point scale ranging from less than $15,000 to over $100,000, and each parent's highest level of education on 7-point scales ranging from grade school to Ph.D., J.D., or M.D. The mean of these three variables served as the measure of the family SES ($M = 4.42$).

Results

The belief tasks were not administered to one child at age 4½, due to experimenter error. Among the remaining 167 children, 22 in true-belief and 26 in false-belief initially failed a control question. After being corrected and re-asked the question, 2 failed again, 1 was inadvertently not corrected, and 3 others' justifications could not be coded due to speech difficulties. Data from these six children were excluded from analyses. At age 6, out of the 150 children, 7 in true-belief and 11 in false-belief initially failed a control question. After being corrected and re-asked the question, 2 failed again, 3 were inadvertently not corrected, and 1 was inadvertently not asked a justification question. The data from these six children were dropped. No children failed the control questions at both ages.

Preliminary Analyses

We tested for order effects at age 4½ with a 2 (task order) × 2 (task) repeated measures ANOVA on correct responses to the belief question. There was only a significant effect of task, $F(1, 159) = 70.85$, $p < .001$, and no significant interaction, $F(1, 159) = 1.940$, $p = .166$. Children did better on true belief ($M = 0.80$) than false belief ($M = 0.34$). False-belief performance was lower than the typical mean of .67 at that age, and more typical of 3-year-olds (Wellman et al., 2001). Performance on the first task that children received was similar to the overall pattern, .72 and .32, respectively.

At age 6, a 4 (task order) × 2 (task) repeated measures ANOVA showed a marginally significant effect of task, $F(1, 140) = 3.359$, $p = .069$, and no significant interaction, $F(3, 140) = 2.074$, $p = .106$. The means for true belief and false belief were .73 and .80, respectively. False-belief performance was typical for that age. Performance on the first task that children received was similar to the overall pattern, .65 and .82, respectively. At both ages, true-belief performance was slightly worse when it came first than second (at 4½: .72 versus .88; at 6: .65 vs. .80), opposite to Rakoczy and Oktay-Gür's (2020) finding. Thus, there appeared to be little carry-over from one task to the other (see also Chapter VII).

Table 15 shows the number of children at each level of belief understanding: Reality reasoning, the two patterns of strategy switching between reality reasoning and PAR, PAR, and belief reasoning. Three types of evidence indicated that the dual-justification criterion for belief reasoning is developmentally appropriate. The first evidence comes from the frequency of switching between reality reasoning and PAR. Switching from reality reasoning on true belief to PAR on false belief (i.e., RR–PAR) results in passing both tasks. Switching from PAR on true belief to reality reasoning on false belief (i.e., PAR–RR) results in failing both tasks. If RR–PAR also includes some belief reasoners who give only one belief justification, then the RR–PAR group should be larger than the PAR–RR group. However, the groups do not significantly differ at either age (21 vs. 13; binomial $p = .23$ and 7 vs. 12; $p = .36$).

The second evidence comes from the proportion of children at age 4½ who progressed to belief reasoning at age 6. If RR–PAR includes some belief reasoners, then the proportion who progressed to belief reasoning should be higher in that group than in PAR–RR, but it was actually lower: RR–PAR = .56, PAR–RR = .67.

The third evidence comes from performance on the dependent variables. If RR–PAR includes some belief reasoners, then that group should outperform the PAR–RR group, but the means were in the opposite direction in all cases except for inhibitory control at age 6, where the means were similar.

Combining the two switching patterns gives a four-level scale: Reality reasoning, transitioning between reality reasoning and PAR, PAR, and belief reasoning. At age 4½, the proportions of children at each level were .58, .21, .12, and .09, respectively, with over half using reality reasoning. The pattern at age 6 (.11, .13, .19, and .57) was the mirror image, with over half using belief reasoning. Both patterns are consistent with the strategies developing in this sequence. Small differences at one age that might cast doubt on the reliability of the observed sequence—such as the difference between PAR (.12) and belief reasoning (.09) at 4½—are larger, and in the opposite direction, at the other age (i.e., .19 and .57, respectively, at age 6). In addition, only .06 (8/131) reverted from a higher level at age 4½ to a lower level at age 6; .17 stayed the same; and .77 progressed to a higher level. Averaging the proportions across ages reveals a flat distribution of one-third at reality reasoning, one-third either in transition to or at PAR, and one-third at belief reasoning.

The BUS is scored as an ordinal scale: Reality reasoning (1), switching (2), PAR (3), and belief reasoning (4). Figure 3 shows the standardized means for social compliance and inhibitory control at age 4½ at each level of belief understanding at 4½. Figure 4 shows the means for social competence, inhibitory control, and kindergarten adjustment at age 6 at each level of belief understanding at 6. As expected, at each age, PAR and belief reasoning appear to be associated with distinct, postreality reasoning patterns of broader development.

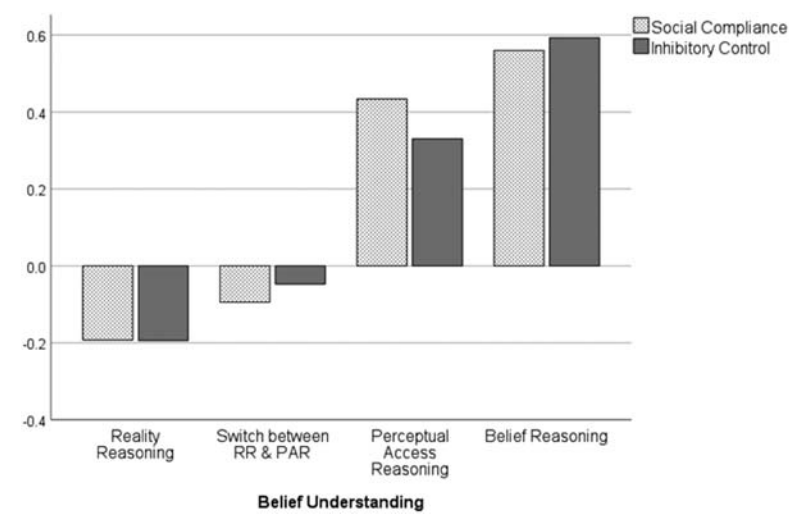

FIGURE 3.—Standardized means of social compliance and inhibitory control at age 4½ in relation to concurrent levels of belief understanding.

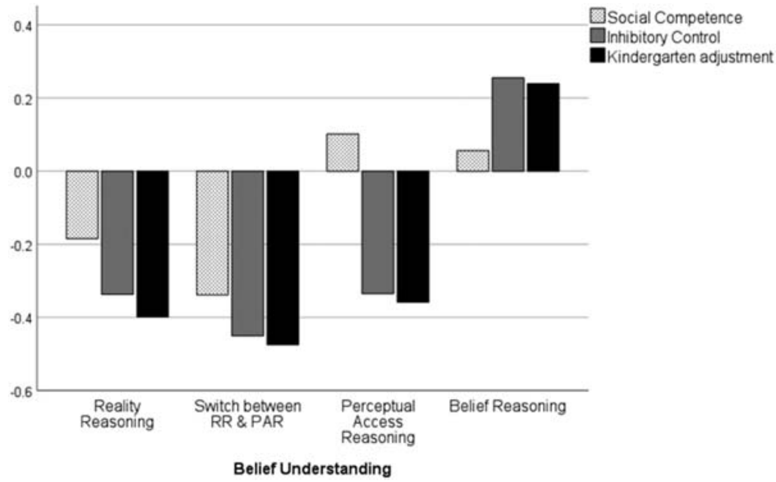

FIGURE 4.—Standardized means of social competence, inhibitory control, and kindergarten adjustment at age 6 in relation to concurrent levels of belief understanding.

Main Analyses

Table 16 shows the correlations among the measures. BUS at age 4½ was significantly positively related to social skills and inhibitory control at both ages, and to kindergarten adjustment. BUS at age 6 was also significantly related to those measures, except for social competence at 6 and inhibitory control at 4½. BUS at age 4½ was significantly positively related to language

TABLE 16
CORRELATIONS BETWEEN MEASURES

	4½ Years			6 Years			
	BUS	SC	IC	BUS	SC	IC	KA
4½ years							
SC	.28**						
IC	.29**	.69**					
6 years							
BUS	.17	.20*	.15				
SC	.24**	.40**	.48**	.12			
IC	.26**	.54**	.70**	.29**	.59**		
KA	.22*	.33**	.41**	.28***	.62**	.73**	
Language	.25**	.23**	.38**	.36**	.25**	.36**	.34**
SES	.22**	.19**	.23**	.15	.18*	.31**	.23**
Gender	.10	.07	.15*	.07	.21**	.24**	.30**

Note. Gender: 1 = *boy*, 2 = *girl*.
BUS = belief understanding scale; IC = inhibitory control; KA = kindergarten adjustment; Language = Wechsler Preschool and Primary Scales of Intelligence mean expressive and receptive vocabulary (4½ years) and receptive vocabulary; SC = social compliance (4½ years) and social competence (6 years); SES = socioeconomic status.
*$p < .05$
**$p < .001$.

at 4½ and SES, although only language was significantly independently related to BUS (standardized β coefficient [β] = .208, p = .015; β for SES = .132, p = .122). BUS at age 6 was significantly positively related to language at 6, but was not significantly related to SES.

The main analyses pitted the false-belief task against the BUS in hierarchical regressions examining the concurrent relations between belief understanding and each dependent variable at each age. The analyses controlled for children's vocabulary development at each age, children's gender, and family SES. An important issue to resolve in these analyses was the multicollinearity that would result from both the false-belief task and the BUS in the same model.

We avoided multicollinearity in two ways. First, we dummy coded BUS into three variables to capture the four levels, and ran hierarchical regression analyses using three steps. The first step of the model included only the control variables. The second step allowed us to examine a reasonable stand-in for the false-belief task; namely, the first BUS dummy variable (coded as 1 = *reality reasoning*, and 0 = *all other levels*), which pitted reality reasoning against the other three levels. This is reasonable because children scored as reality reasoning failed the false-belief task and passed the true-belief task, which amounts to using true-belief success as a control for basic task understanding and thus genuine false-belief failure (e.g., Clements & Perner, 1994; Ruffman et al., 2001; see also Mitchell et al., 1999, Studies 2 and 3). All other children passed false-belief, except the .08 at each age classified as PAR–RR (failing both); they were necessarily misclassified with those who passed false-belief. (We subsequently checked whether PAR–RR watered down the effect of the false-belief stand-in, reality reasoning, by rerunning the main analyses excluding PAR–RR. It did not. The amount of variance accounted for by reality reasoning at step two actually decreased slightly.)

The third step added the remaining BUS dummy variables with belief reasoning as the reference group, thus testing whether the remaining variables accounted for additional variance over and above that accounted for by reality reasoning alone. Multicollinearity was not an issue in any analysis; for all predictors, variance inflation factor <3.26. Table 17 shows the R^2 change tests from the separate hierarchical regressions of BUS at age 4½ onto social compliance and inhibitory control at age 4½, and BUS at age 6 onto social competence, inhibitory control, and kindergarten adjustment at age 6.

The only case in which reality reasoning significantly outperformed the BUS was for age 4½ inhibitory control. Reality reasoning captured only 2% more variance (p = .070) than control variables, but the BUS did not capture significant additional variance. In the final model, only reality reasoning significantly differed from belief reasoning (β = −.279; p = .034).

In all other analyses, the BUS outperformed reality reasoning. For age 4½ social compliance, reality reasoning captured significantly more variance (2.6%; p = .035) than the control variables, but the BUS captured significant additional variance (3.5%; p = .054). Both reality reasoning (β = −.340,

TABLE 17

R^2 Change Tests From Hierarchical Regressions of Concurrent Belief Understanding Scale Onto Social Compliance and Inhibitory Control at Age 4½, and Social Competence, Inhibitory Control, and Kindergarten Adjustment at Age 6

	Social Compliance			Inhibitory Control		
Model: 4½ years	R^2	F for ΔR^2	p Value	R^2	F for ΔR^2	p Value
Step 1						
Language, SES, gender	.064	3.51	.017	.169	10.48	<.001
Step 2						
+Reality reasoning	.090	4.50	.035	.186	3.22	.070
Step 3						
+Switching, PAR, belief reasoning	.125	2.98	.054	.202	1.48	.232

	Social Competence			Inhibitory Control			Kindergarten Adjustment		
Model: 6 years	R^2	F for ΔR^2	p Value	R^2	F for ΔR^2	p Value	R^2	F for ΔR^2	p Value
Step 1									
Language, SES, gender	.095	4.81	.003	.211	12.27	<.001	.186	8.70	<.001
Step 2									
+Reality reasoning	.096	0.08	.785	.211	0.15	.701	.186	0.00	.952
Step 3									
+Switching, PAR, belief reasoning	.129	2.54	.082	.254	3.83	.024	.232	3.32	.040

Note. Gender: 1 = *boy*, 2 = *girl*.
Language = Wechsler Preschool and Primary Scales of Intelligence mean expressive and receptive vocabulary (4½ years) and receptive vocabulary (6 years); SES = socioeconomic status.

$p = .014$) and switching between reality reasoning and PAR ($\beta = -.253$, $p = .040$) differed significantly from belief reasoning. At age 6, reality reasoning did not capture significantly more variance than control variables in any analysis (ps > .700). In contrast, the BUS captured significantly more variance than controls in both inhibitory control and in kindergarten adjustment (about 4.5%; ps = .024 and .040, respectively). Both switching (βs = −.171, $p = .034$, and −.177, $p = .051$) and PAR (βs = −.173, $p = .029$, and −.183, $p = .039$) significantly differed from belief reasoning. The BUS captured significantly more variance (3.4%; $p = .082$) than the controls in social competence, but no βs were significant.

The second way that we avoided multicollinearity was to rerun the analyses using only the false-belief task (coded as 0 = *fail*, 1 = *pass*) and the control variables. This reunites the misclassified PAR—reality reasoning children with those who fail the false-belief task, and pits them against all those who pass. The false-belief task fared as badly as, or worse than, reality reasoning fared in the second step in the models above. For age 4½ inhibitory control, the false-belief task likewise captured only 1.6% more variance ($p = .080$) than the controls. For age 4½ social compliance, in the one case where reality reasoning had captured significantly more variance than the controls, the false-belief task failed to do so ($p = .108$). At age 6, the false-belief task likewise did not capture significantly more variance than the controls in any analysis (ps > .455).

In conclusion, we checked one more thing. The BUS reflects performance on two tasks, true-belief and false-belief. We checked whether simply counting the number of tasks that children passed, rather than assigning children to the four theoretically derived patterns of judgments and justifications, would yield similar, or even meaningful results. At neither age did the number of tasks passed have any significant partial correlations (controlling for gender, language, and SES) with any of the dependent measures. Thus, it is the pattern of children's responses that is important.

Discussion

The BUS integrates the linear increase with age in the proportion of children who pass false-belief tasks (Wellman et al., 2001), and the U-shaped pattern on true-belief tasks (Chapter II; Fabricius et al., 2010). The findings in the current chapter showed that the coding of the scale was reliable; that the scale exhibited consistent developmental sequences at ages 4½ and 6 in terms of the proportion of children in each category; and that only .06 of children reverted to a less advanced category, while .77 progressed to a more advanced category.

The four BUS categories of reasoning strategies about the mind form a more extended developmental progression than the below-chance to above-chance progression on false-belief tasks alone. The false-belief task can obscure aspects and features of ToM development by not distinguishing among children who might pass the task in very different ways—belief-reasoners, PAR-users, and half of those in transition between reality reasoning and PAR. For researchers interested in basic questions of development, a more extended developmental progression provides more clues about how development proceeds. The first application of the BUS in this chapter revealed that only about half of 6-year-olds had reached belief reasoning, and the rest were still making their way into, and through, PAR.

For researchers interested in individual differences and developmental relations between ToM and cognitive, social, and educational functioning, the BUS revealed more links than the false-belief contents task, which is used as the indicator of understanding false beliefs in the Wellman and Liu (2004) ToM scale. In the five tests reported here at the two ages, after controlling for vocabulary development, SES, and gender, the false-belief contents task and the BUS accounted for similar amounts of variance only in age 4½ inhibitory control; in all other cases, BUS accounted for more variance than the false-belief contents task.

Our goal for this chapter was to illustrate the kinds of new information and insights that the BUS can provide, not to test theories about social development and school readiness, or about reciprocal relations between belief understanding and inhibitory control and social development. Nevertheless, the findings provide some new insights. At both ages, children who used belief reasoning outperformed other children in all these areas. At age 4½, belief reasoners outperformed reality reasoners but not PAR-users. This finding suggests that being on track for social skills and inhibitory control at 4½ is tied to moving out of reality reasoning, but not necessarily to early progression from PAR to belief reasoning. At age 6, however, children who had progressed to belief reasoning showed better inhibitory control and kindergarten adjustment than children still using PAR. This suggests that belief reasoning, but not PAR, confers different advantages for school adjustment than the advantages conferred by having a larger vocabulary, being from a higher SES family, and being female.

The current data came from children at two ages, the first at age 4½ when most children used reality reasoning, and the second at age 6 when most used belief reasoning. Thus, the sample did not allow us to test the most important developmental prediction of PAR theory: All children should pass through the developmental sequence of reality reasoning, PAR, and belief reasoning.

The BUS can also be used with true- and false-belief identity tasks. Using both identity and contents tasks can provide more opportunities to observe strategy-switching, as well as evidence of generalized understanding of belief. Coding the justifications given by PAR-users and reality-reasoners can provide insights into the role of strategy awareness in strategy transition. Fabricius et al. (2010) provided codes for all their true-belief tasks, and reported that most children coded as reality-reasoners and PAR-users gave corresponding reality and PAR justifications.

From PAR to Belief Reasoning: Preliminary Suggestions

Experience can teach children that their reality reasoning and PAR judgments are sometimes wrong, but feedback alone cannot lead them to belief reasoning. To get some preliminary insight into how children make the transition from PAR to belief reasoning, about which nothing is currently known, we looked at those children who gave a single belief justification. Children who were either in transition to PAR, or were at PAR, were more likely than those using reality reasoning to give a belief justification in the one task, or in only one of the two tasks, that they passed. However, those who gave a single belief justification did not differ from those who did not on any of the five dependent variables. This result suggests that the children who gave a single belief justification were not in transition to belief reasoning, and that their single belief justifications do not indicate that they switched to belief reasoning on that task.

It is possible that single belief justifications could have been semantically primed by a correct judgment followed by the justification question. When children judge correctly, they identify the typical contents (e.g., "He'll think crayons are inside.") and the justification question repeats the name (e.g., "Why will he think there are crayons inside?"). Restating the object name could prime some children to give a belief justification ("Because it's a crayons box."). At age 4½, but not at age 6, those who gave single belief justifications had significantly higher vocabulary scores than those who did not. It is reasonable that children would need a certain threshold of vocabulary skill to be susceptible to semantic priming, and that in the present case, only some 4½-year-olds, but most 6-year-olds were at that level.

Although single belief justifications did not indicate transitional use of belief reasoning, they did predict later transition to belief reasoning: .94 of 4½-year-olds who gave a single belief justification progressed to belief reasoning by age 6, versus only .51 of those who did not. A primed belief justification would give children the opportunity to notice that their judgment about what the newcomer will think is consistent with the appearance of the container. This explanation suggests a possible developmental mechanism for the transition from PAR to belief reasoning. In daily life, we do not ask children to explain why people think things are in boxes, but they can observe that judgments about the contents of containers covary with the appearance of the containers. People make correct judgments about what is inside labeled containers, and make incorrect judgments about what is inside unlabeled containers (e.g., birthday presents). Likewise, children can observe that people's judgments about an object's location overlap with statements about where they put it in the past (e.g., "I know where it is. I put it in the drawer!" and "Where is it? Where did I put it?").

However, noticing these consistencies cannot tell the child how someone can see through, as it were, the outside of a familiar container, or into a closed drawer, to know what is inside. It must seem like nonsense to a child who has finally arrived at PAR that people can get it right when they do not have perceptual access in the current situation. For that to make sense, it would require a concept of mental representation. This is as far as the data allow us to see into the transition from PAR to belief reasoning. We can imagine, however, an analogy to the model proposed by Gonzales et al. (2018) in which parents' use of see and know covary with the child being in those mental states, focusing the child on those distinct phenomenological experiences. In the construction of the concept of mental representation, an analogous role could be played by the covariation between agents' judgments about the contents of containers and the appearances of the containers, and between agents' judgments about where things are and their statements about where they put them in the past. These covariations would seem nonsensical to the PAR child, but thinking about them could allow children to gain introspective awareness of their own cognitive cueing processes (Gordon & Flavell, 1977), in which a visual experience leads to a belief about an unseen reality, and a memory of a past event leads to knowledge about the present situation. Evidence of introspective awareness of cognitive cueing could be obtained by asking a justification question ("Why do you think that?") after children answer the initial question in contents tasks ("What do you think is in this box?") and identity tasks ("What do you think this is?"). If children were able to give belief justifications for their own judgments before they were able to give belief justifications for their correct predictions of what a newcomer will think, that would be evidence of introspective awareness of cognitive cueing, and would suggest that it could play a role in constructing the concept of mental representation.

X. How Do Children Construct the Concept of Mind?

In this chapter, we first systematically compile the findings from studies that employed methods that could distinguish children who used PAR from those who used belief reasoning. We then discuss the theoretical implications of the evidence of PAR for understanding ToM development, and offer suggestions for future research.

Compiled Findings

The top part of Table 18 ("Proportion of Children Classified as Using Belief Reasoning") includes two groups of studies in which children who used PAR were distinguished from those who used belief reasoning. These include studies that are reported for the first time in this monograph as well as studies that had been previously published. For each study, Table 18 shows the proportion of children, by age, classified as using belief reasoning. In the text below we provide additional information about each study listed in the top part of Table 18. Specifically, we identify the method that was employed to classify children as using belief reasoning, we specify the figure or table from the study that was the source of the data in Table 18, and we explain any calculations we performed on the data from each study before we entered the information into Table 18.

Fabricius et al. (2010), Study 1

The data come from Figure 2, and the method was judgments-plus-justifications in the true-belief contents task.

Fabricius et al. (2010), Studies 2 and 3

The data come from Tables 1 and 2, and the method was judgments-only on a pair (or two pairs, in which case we used the mean) of 2-option false-belief and true-belief tasks. The few children who failed both tasks likely switched between reality reasoning on false-belief and PAR on true-belief, and a similar number likely switched in the opposite direction and thereby passed both tasks. Thus, the proportion of belief reasoning in Table 18 is the number of children who passed both tasks minus the number who failed both, divided by the total number.

TABLE 18

Proportion of Children Classified as Using Belief Reasoning (Study N), and Proportion of Children Passing 2-Option False-Belief Tasks

	Mean Age (Years)					
Data Source	3½	4½	5	5½	6	6½
Proportion of children classified as using belief reasoning						
Fabricius et al. (2010) Study 1	.10 (29)	.25 (28)		.35 (29)		.64 (22)
Fabricius et al. (2010) Study 2				.42 (52)		
Fabricius et al. (2010) Study 3		.22 (16)		.54 (12)		
Fabricius and Khalil (2003) Studies 1–3			.30 (132)		.64 (32)	
Monograph Chapter III		.10 (202)				
Monograph Chapter IV			.30 (40)			
Monograph Chapter 5		.12 (25)		.23 (22)		.59 (29)
Monograph Chapter VI		.16 (19)		.35 (17)		.60 (10)
Monograph Chapter VII				.13 (45)		
Monograph Chapter IX		.09 (161)			.57 (144)	
Weighted mean proportion	.10	.11	.30	.31	.58	.61
Proportion of children passing 2-option false-belief tasks						
Meta-analysis (Wellman et al., 2001)	.47	.67	.74	.80	.86	.90

Note. Empty cells indicate no data are available. Fabricius et al. (2010, Studies 1 and 2) are discussed in Chapter II; Study 3 did not include a location true-belief task. Fabricius & Khalil (2003, Studies 1–3) are discussed in Chapter III.

Fabricius and Khalil (2003), Studies 1–3

The data come from Table 6, and the method was a yes-or-no question about each option in 3-option false-belief tasks. The findings replicated across three multicondition studies, and Table 6 shows the mean proportion of children in each response pattern. The correct pattern (i.e., yes to the false-belief option and no to the irrelevant option) likely includes some PAR-users, because children who use PAR could be in any one of the four patterns of answers to the false-belief and irrelevant options. Thus, the proportion of belief reasoning in Table 18 is the mean proportion of children in the correct pattern minus the proportion in the reverse pattern (i.e., no to the false-belief option and yes to the irrelevant option).

Monograph Chapter III

The data come from the nine studies reported in Table 4, and the method was one open-ended question in 3-option false-belief tasks. Children who use PAR are as likely to choose the false-belief as the irrelevant option. Thus, the proportion of belief reasoning in Table 18 is the mean proportion of children across the studies who chose the belief option, minus the proportion who chose the irrelevant option, calculated for location and contents tasks separately, and then averaged.

Monograph Chapter IV

The data come from the second column of Table 6, and the method was a yes-or-no question about each option in 3-option false-belief tasks, plus a 2-option false-belief task.

Monograph Chapter V

The method was certainty judgments in false-belief tasks and no-belief tasks. The proportion of belief reasoning in Table 18 is the number of children who bet more tokens that the protagonist would get it wrong in false-belief than in no-belief tasks, minus the number who bet in the incorrect direction, divided by the total number.

Monograph Chapters VI and VII

The data come from Table 7 (stratified by age) and Table 8, respectively, and the method and calculations were the same as in Fabricius et al. (2010), Studies 2 and 3.

Monograph Chapter IX

The data come from the belief reasoning pattern in Table 15, and the method was judgments-plus-justifications in the true-belief and false-belief contents tasks.

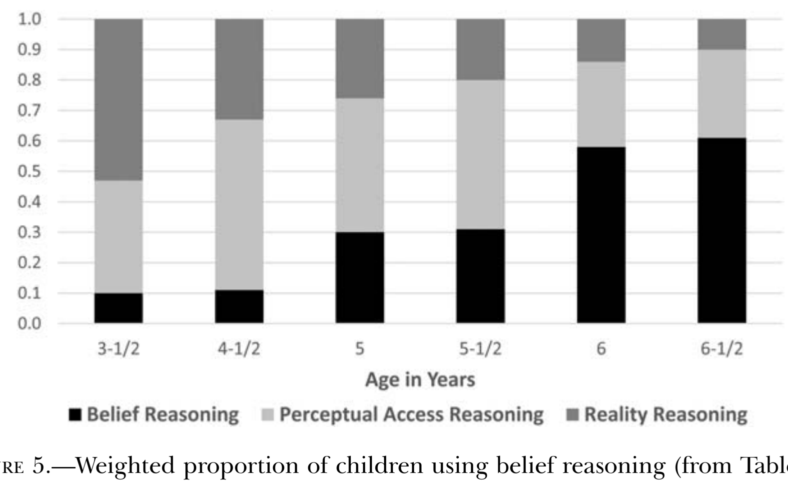

FIGURE 5.—Weighted proportion of children using belief reasoning (from Table 18), and the proportion of children using realty reasoning (from Wellman et al., 2001). The remaining proportion is the estimate of the rate of passing false-belief tasks by using PAR. PAR = perceptual access reasoning.

The top part of Table 18 (Proportion of children classified as using belief reasoning) represents 20 individual studies, including 57 age or experimental group X diagnostic condition means and 922 children. (Including Chapters II and VIII, the Monograph as a whole represents 40 studies, 139 age or experimental group × diagnostic condition means, and 2,304 children.) The proportions of children using belief reasoning within each study in Table 18 show that the different methods revealed similar rates of developmental change. Table 18 also includes the weighted mean proportion of belief reasoning across studies at each age. This provides replicated, multimethod evidence that shows that at ages 3½ and 4½, very few children used belief reasoning. At 5 and 5½ years, only about .30 of children used belief reasoning, and at 6 and 6½ years, a bare majority, about .60, used belief reasoning. The developmental shift to a bare majority of children using belief reasoning occurred between ages 5 and 6, not 3 and 4 as the standard 2-option false-belief tasks have led us to believe; thus, we cannot expect a substantial majority to understand belief until age 7.

The bottom part of Table 18 (Proportion of children passing 2-option false belief tasks) includes the rate at each age of passing the standard false-belief tasks obtained in the meta-analysis by Wellman et al. (2001, fig. 2A). Those rates are higher, as expected if many children passed false-belief tasks by using PAR. While the rate of passing false-belief tasks is ambiguous regarding how it reflects belief reasoning versus PAR, the rate of failing provides an unambiguous estimate of the rate of using reality reasoning. Figure 5 shows the proportion of children at each age who used reality reasoning in false-belief tasks, obtained from the 2001 meta-analysis, and the weighted proportion at each age who used belief reasoning, obtained from Table 18. The remaining proportion at each age provides an estimate of the rate of passing false-belief tasks by using PAR; it averages .50 at ages 4½, 5, and 5½ years. Figure 5 suggests that from ages 3½ to 4½, the primary development is from reality reasoning to PAR; from ages 4½ to 5½, as more children move into PAR, others move out of PAR into belief reasoning, thus maintaining the .50 rate across these ages; and from ages 5½ to 6½, the primary development is from PAR to belief reasoning.

Implications for a Theory of ToM Development

Innateness

On the face of it, the evidence that barely half of 6-year-olds use belief reasoning makes it unlikely that the concept of belief is already implicit in infants' and toddlers' behaviors. The evidence raises specific problems for the classic account of ToM innateness (Leslie, 1987, 1994, 2000a). In that account, an innate neurocognitive mechanism generates candidate true-belief and false-belief representations. A learning mechanism initially attributes true beliefs by default, and subsequently learns the rules for tracking protagonists' information acquisition to determine when to inhibit the true-belief default and attribute false beliefs instead. The classic account, if true, would provide an elegant account of how children learn the rules. The rules, such as, "represent the belief to be the location where the person last saw it," and "represent the belief to be the contents pictured on the container," agree with the true-belief default in true-belief tasks, but conflict with it in false-belief tasks. Learning that the default is incorrect in false-belief situations would lead to inhibiting the default in those situations, but would not reveal the correct rule. The agreement between the default and the rule in true-belief situations would reveal that the rule is correct. Thus, a true-belief default would guide hypothesis testing toward the correct rule for attributing beliefs within a dual-mechanism (conflict and agreement) model of cognitive change (Bryant, 1986). During learning, there would be no incentive for inhibiting the true-belief default in true-belief situations (e.g., Leslie & Polizzi, 1998).

However, the fact that children fail true-belief tasks would have to mean, from the point of view of the classic account, that children do inhibit true beliefs in true-belief situations, and attribute false beliefs in their place. It would make little developmental sense that children would switch from over-attributing true beliefs before they turn four years of age, to over-attributing false beliefs thereafter, until they eventually learn the rules for when to attribute one versus the other.

We did not find evidence of a true-belief default. In Chapter VI, among belief-reasoners, there was no indication of the signature of a true-belief default in the effects of added processing demands. In Chapter IX, among transitional children in the switching patterns shown in Table 18, there was little indication of the effects of a true-belief default on performance in the true-belief task. A true-belief default should lead more transitional children to pass the true-belief task than to fail it. However, Table 18 shows that at age 4½, 21 passed the true-belief and 13 failed, and at age 6, 7 passed and 12 failed, and, as reported, the groups do not significantly differ at either age.

There is also little evidence of a true-belief default in implicit location tasks with younger children. Instead, most young 3-year-olds correctly look to the false-belief location in anticipation of the protagonist's return (Clements & Perner, 1994; Garnham & Perner, 2001; Garnham & Ruffman, 2001; Ruffman et al., 2001). Similarly, infants and toddlers also respond correctly in implicit false-belief tasks (Baillargeon et al., 2010).

An alternate account of innate ability to track information acquisition and implicitly represent both false and true beliefs would avoid the problems associated with a true-belief default. Failing explicit false-belief tasks could then be explained in terms of children simply failing to inhibit the verbal reality response, rather than failing to inhibit a true-belief default. Passing the tasks could be explained as children becoming aware of their implicit representation of false beliefs. However, the problem would then be to explain why children fail true-belief tasks at the same time that they are presumably gaining awareness of false beliefs. It would not make sense for children to become aware of false beliefs before they become aware of true beliefs, leaving it a mystery why implicit representation of true and false beliefs should not eventually make its way into the awareness of typically developing children until middle childhood.

Empirical Evidence From Implicit Tasks With Chimpanzees, Monkeys, Infants, and Toddlers

The method of using both a true-belief and a false-belief task to distinguish PAR from belief reasoning can easily be adapted to implicit tasks to test for implicit belief representation. Below we briefly review selected studies that used implicit tasks in order to illustrate how the method can be adapted, and we evaluate the evidence produced for implicit belief representation.

In the Discussion of Chapter IX, we noted that the true- and false-belief tasks in the original four implicit belief studies with young 3-year-olds are not comparable in terms of cues for situation change. The tasks are not comparable because the true-belief leave conditions did not include an inconsequential object movement during the protagonist's absence. Return conditions could also have made the tasks comparable, but return conditions are as hard to find in infant and toddler studies as in studies of older children (see Table 2). In their review of the first implicit infant and toddler studies, Baillargeon et al. (2010) did not describe the true-belief tasks. Hedger and Fabricius (2011) reviewed the studies, and found that the true-belief tasks contained much weaker cues for situation change than the false-belief tasks.

Hedger and Fabricius (2011) reviewed two chimpanzee studies that included methods that made it possible to distinguish implicit belief reasoning from an alternate mechanism that links behavioral indications of perceptual contact in a situation to anticipated behaviors in that situation. Kaminski et al. (2008) included a true-belief object-movement condition, and Hare et al. (2001) included a no-belief task. Both studies also included a false-belief task. Chimps expected their chimp competitors to get it wrong in both the true- or no-belief task and the false-belief task. The control conditions in both studies were true-belief stay conditions, and in those conditions, chimps expected competitors to get it right. Hedger and Fabricius proposed that chimpanzees used what they called Rule A: Perceptual contact → get it right; no perceptual contact → get it wrong. Rule A predicts behavior without attributing mental states. The input to Rule A is a representation of a conspecific with a goal (generated by a different mechanism). The hypothesized Rule A mechanism recognizes behavioral cues of the other's perceptual contact with the goal. The output is a prediction that the other will succeed or fail to obtain the goal, but only in the present situation. The output is not a representation of the other's mental state, and so when the situation changes, Rule A cannot predict current behavior based on the agent's past perceptual contact; Rule A must be reapplied to the new situation. Horschler et al.

(2019) recently found a response pattern in rhesus macaques consistent with using only the first half of Rule A (perceptual contact → get it right). Monkeys expected a human agent to get it right in a true-belief stay (box movement) condition, but had no expectation in a true-belief object-movement (fruit movement) condition.

Hedger and Fabricius (2011) also reviewed Southgate et al. (2007), who used a novel false-belief object-not-present condition with 2-year-olds, in which the object ultimately leaves the scene before the agent returns. Krupenye et al. (2016) used the same procedure with great apes. Both children's and apes' anticipatory looks were correctly directed at the last place the agent had perceptual contact with the object. The object-not-present condition originated in the explicit false-belief literature, where researchers assumed that the absence of a competing reality response would make the agent's mental state more salient, thus facilitating implicit belief attribution (Wellman et al., 2001). Hedger and Fabricius (2011) pointed out that there is also no competing reality response in standard true-belief tasks. Thus, false-belief object-not-present conditions should be paired with standard implicit true-belief return and object-movement conditions to test whether absence of a competing reality response in all three conditions produces the response pattern of implicit belief attribution. If it does not, researchers would have to consider that in the implicit object-not-present condition, infants face a random choice between the two locations when the agent returns, because Rule A cannot point to a unique wrong location. In that case, the correct location could simply be the more salient of the two because infants had begun to use Rule A at that location during the hiding portion.

Sodian and Thoermer (2008) discussed several infant looking-time studies that used implicit true-belief stay conditions and no-belief tasks, and in one case also an implicit true-belief return condition. In the first study, Rule A was used at 24 and 18 months, but not at 14 months. Infants expected that an agent would get it right in the stay (visual access) condition, and get it wrong in the no-belief (no visual access) task. In the second set of studies, 16-month-olds used only the first half of Rule A. Infants expected the agent to get it right in the two stay conditions, but they had no expectations in the no-belief task or in the true-belief return condition. In a third study, 14-month-olds used the first half of Rule A. Infants looked longer if the agent got it wrong in the stay (irrational) condition compared to the no-belief (rational) task, but the researchers did not assess whether infants also used the second half of Rule A; that is, whether they had any specific expectations in the no-belief task.

Powell et al. (2018) replicated Knudsen and Liszkowski's (2012) findings with 18- and 24-month-olds by testing 25-month-olds and 3½-year-olds on the original researchers' novel implicit true-belief double-return condition and false-belief task. In the former, the agent places a toy in box 1 and leaves. The experimenter begins to transfer the toy to box 2. The agent returns by opening the door to watch the completed transfer to box 2, remarks, "Oh, I see," then leaves a second time (closing the door), and finally returns again to get the toy. Response patterns at both ages matched implicit attribution of beliefs. Children expected the agent to get it wrong in the false-belief task and get it right in the true-belief task. However, the potential proactive effect of the agent's first return and statement, "Oh, I see," is unknown. Perhaps the first return and statement triggered Rule A, or PAR in some 3½-year-olds, and an expectation that he will get it right, and perhaps the second return through the same door did not signal a new situation. In that case, children could have defaulted to their initial expectation rather than engage in a new round of Rule A or PAR.

Surian and Geraci (2012) embraced the goal "to maximize the similarity of the TB and the false belief trials" (p. 34) in their true-belief return condition and false-belief task. The response pattern of 17-month-olds matched implicit attribution of beliefs. Infants responded to animated geometric shapes, which would implicate an abstract mechanism in which the input is not confined to a conspecific with a goal. Heyes (2014) raised a concern about possible differential effects of the true- and false-belief trials on infants' memory of the object's last hiding place, which might account for the observed response pattern. Surian and Franchin (2020) addressed this concern, but they also admitted that more research is needed to fully address it. That would include stay, leave, object-movement, and no-belief conditions.

The implicit ToM literature is in flux, with little consensus about which procedures produce robust findings, and what those findings mean. The above review is not meant to be comprehensive, but rather to highlight some findings relevant to whether a Rule A-type mechanism or implicit belief representation might be part of core cognition (Carey, 2009). A comprehensive battery of implicit tasks would include a standard implicit false-belief location task, true-belief stay, leave, return and object-movement conditions, as well as a no-belief task. A comprehensive battery would provide a baseline for systematic developmental and cross-species comparisons, and for understanding effects of novel task variations such as object-not-present and double return. If evidence accumulates that infants solve implicit belief tasks without attributing beliefs, then it would be unlikely that they would switch in one kind of condition to a different mechanism that does attribute beliefs. In that case, researchers would be more justified searching for an explanation of the anomalous finding than announcing an innate ability to represent false beliefs.

From Rule A to PAR

Infants are likely doing something more interesting than responding to local stimulus factors and contingencies that are sometimes congruent with belief attribution. It is not likely to be implicit belief attribution, however, because the data do not unambiguously support it, and as noted, it is difficult to see why explicit belief reasoning should then make such a late developmental appearance. We think that it is more likely that a Rule A-type mechanism is a part of core cognition. It would support simple cooperation and competition (Hedger & Fabricius, 2011), and it would not need to become explicit. PAR does not logically build upon Rule A, and thus we would not necessarily expect longitudinal associations between implicit and explicit tasks.

Teufel et al. (2013)'s findings suggest that Rule A and early PAR coexist in 2½-year-olds, and are likely dissociated. Toddlers watched as a toy was hidden, and their parents, sitting next to them, covered their own eyes so they could not see where the toy was hidden. In this case, toddlers helped their parents afterwards by pointing to the correct location of the toy, suggesting that Rule A led toddlers to expect that their parents would otherwise get it wrong. In a separate procedure, the toddlers wore a pair of opaque glasses, and thus they experienced their own loss of sight while wearing the glasses. Afterwards, they explicitly understood, when questioned, that their parents also could not see with the glasses, suggesting that the toddlers were using rule 1 of PAR. In the third procedure, toddlers again watched as a toy was hidden, but this time their parents, sitting next to them, wore the opaque glasses. In this case, toddlers did not help their parents afterwards by pointing to the correct location of the toy. Thus, toddlers appeared to use Rule 1 of PAR to understand that their parents could not see with the glasses, but PAR Rule 1 apparently did not activate Rule A.

Teufel et al. (2013)'s findings suggest that Rule A and PAR are dissociated. Rule A is behavioral, and takes as input behavioral cues that the other is in or out of perceptual contact. Covered eyes are such a cue for 2½-year-olds, but opaque glasses are normally not. PAR is mentalistic. Toddlers used their own experience with the opaque glasses to understand that their parents could not see with the glasses, but that awareness did not trigger Rule A.

In a final procedure, Teufel et al. gave toddlers behavioral training that showed their parents getting it wrong while wearing the glasses. After the training, the glasses became a behavioral cue that their parents were out of perceptual contact, and triggered Rule A helping.

The researchers did not test whether the behavioral training showing their parents getting it wrong while wearing the glasses with the opaque glasses also triggered PAR Rule 1 awareness that their parents could not see with the glasses, which it should not if Rule A and PAR are dissociated.

Thoermer et al.'s (2012) findings also suggest that Rule A and PAR are dissociated. Their Rule A task (implicit Level 1) at 15 months was not related to their PAR tasks at 30 months (two explicit Level 1 tasks), or at 48 months (explicit location and contents false-belief, which are most often passed by using PAR at this age; see Figure 5). It was related to another Rule A task (implicit [object-not-present] location false-belief) at 18 months, which in turn was not related to three of the four PAR tasks; the sole exception was explicit location false-belief, suggesting that this one relation reflected something specific to those location tasks.

In contrast, Low (2010) reported relations between Rule A and several PAR tasks. However, the relations were not assessed longitudinally, but rather concurrently among children aged 3–5 years. At those ages, it is not possible to know whether looking longer at the false-belief than the reality location during the 4-s delay between the implicit prompt and the explicit test question reflects Rule A or PAR. Low (2010) used Ruffman et al.'s (2001) implicit procedures, but ignored their warning that amount of looking time "*cannot* always be taken as evidence of implicit understanding. Correct eye gaze in this situation may sometimes be accompanied by explicit insight" (p. 218, emphasis in original), or as we would say, Rule A can be accompanied by PAR. Ruffman et al.'s (2001) warning stemmed from their finding that among the children who passed the eye gaze measure, the older reality-reasoners, as well as those who passed the false-belief task, were less certain than the younger reality-reasoners of their explicit answers in the false and true-belief tasks (as assessed by the betting question). Ruffman et al. noted that uncertainty often marks knowledge transitions (Siegler, 1996), which in this case would be the transition from reality reasoning to PAR. Relations between implicit and explicit tasks must be tested longitudinally, and the implicit tasks administered before children are in transition from reality reasoning to PAR, to ensure that the presumed implicit measures are not contaminated by PAR.

From PAR to Belief Reasoning

At the end of Chapter IX, we discussed what we could glean from the 2-wave longitudinal data about how children navigate the path from PAR to belief reasoning. To understand this last transition, we will need to understand how it fits into the longer developmental trajectory, for which we will need data from the implicit and explicit tasks administered within a finer-grained longitudinal design. That strategy will allow us to provide new answers to fundamental questions about the innate basis of ToM, and about the developmental mechanisms by which children acquire an explicit representational ToM. The comprehensive battery of implicit tasks discussed above in the section entitled "Empirical Evidence From Implicit Tasks With Chimpanzees, Monkeys, Infants, and Toddlers" is useful in exploring whether infant and toddler capability is best understood as a Rule A-type mechanism, or as an belief representation. Modeling longitudinal continuity versus discontinuity, and testing it with cross-sectional experimental evidence (e.g., Teufel et al., 2013), can reveal whether the later developments of either, or both, PAR and belief reasoning are based on, or dissociated from, the infant capability.

If the evidence were to favor longitudinal continuity with infant capability, it would imply that the developmental mechanism includes what has been termed *explicitation* (Karmiloff-Smith, 1992), that is, becoming aware of implicit skills. In that case, moderators of any paths of longitudinal continuity could provide insight into processes by which implicit cognition could become explicit. If the infant capability is Rule A, then, on the explicitation model, PAR is explicit awareness of Rule A. However, we have seen that PAR is more than that because it trades on mental state concepts; furthermore, the sequence of understanding seeing in self before other, followed by understanding knowing in self before other (Gonzales et al., 2018), is probably better understood as a sequence of conceptual developments than as progressive awareness of Rule A. Likewise, for the many reasons discussed above, it is unlikely that belief reasoning is explicit awareness of implicit representation of beliefs.

We suspect that the evidence will favor longitudinal discontinuity between infant capability and both PAR and belief reasoning. As we have discussed, PAR likely begins with introspection that is likely adult-guided, which means that children could discover the phenomenological sensorium of mental states of seeing and knowing, rather than having to somehow theorize mental states into being (Fodor, 1980; Leslie, 2000b). To understand how PAR is constructed between the ages of 2 and 6, we will need to determine the relative contributions of parent mental state talk, introspection, theorizing, language development, executive function, strategy awareness, social experiences, and culture. PAR provides children the major insight that one mental state causes another, but these are still tied to the worldly state; belief reasoning requires a new concept of mental representation. To understand how belief reasoning is constructed between ages 5 and 7, we will need to determine what pattern of the above factors accounts for the development of belief reasoning, and how that pattern compares to the pattern for the development of PAR. Belief reasoning could begin with dawning awareness that knowing persists across worldly changes, as well as with awareness of forgetting, followed perhaps by awareness of cognitive cueing, and the on-going flow of thoughts (Flavell & Flavell, 2004). These introspections are likely to be more personally guided than adult-guided, and their coalescence into a new concept of mental representation would also be, simultaneously, their coalescence into a new concept of the autobiographical self that is thinking these thoughts.

Beyond Belief Reasoning

Important progress has been made toward achieving an integrated view of correlates and antecedents of ToM development in later childhood and adolescence (Weimer et al., 2021). However, it is not known whether ToM in adolescence requires the development of new concepts. One approach (Miller, 2009) is to view adolescent ToM as second-order reasoning about concepts that have already been acquired, including beliefs ("A thinks that B thinks…") and desires and intentions (e.g., "A hopes that B likes…").

A different approach is to view adolescent ToM as a new, *constructivist ToM* that involves understanding the interpretive processes embedded in knowledge construction (Chandler, 1988; Carpendale & Chandler, 1996; Fabricius & Schwanenflugel, 1994). Developing a constructivist ToM would make it possible for adolescents to begin to understand that beliefs are not either true or false, but that beliefs can also be interpretations, such that two people can have different, but equally good interpretations of the same thing. If representational ToM is not acquired until middle childhood, it would be reasonable to expect that a second sustained period of development is necessary to acquire a constructivist ToM. Representational ToM employs a concept of a mind that forms and manipulates representations. It is possible that constructivist ToM requires a new, or at least highly modified concept of a mind that acts as a selective and interpretative filter between experience and representation, and that can give different meanings to the same situation.

Two examples of social skills that would require constructivist ToM to be maximally effective are the ability to maintain a credible lie, and the ability to persuade someone to change their mind, because both involve tailoring messages to listeners' perspectives and interpretations. Maintaining a lie under cross-examination requires more than just monitoring what one says so as to not tell the truth. Ensuring that the lie is convincing requires keeping in mind how the listener will interpret the fabricated situation, whether it will make sense to the listener, what questions the listener will ask, etc. Persuading others to change their minds requires understanding not only what they believe, but also why they believe it and what it means to them, so as to be able to convince them of the benefits of believing something else.

Better theoretical specification of constructivist ToM, along with theoretically informed measurement tools, will help to identify what develops in adolescent ToM, hypothesize and test mechanisms for how it develops, and understand how individual differences arise. Weimer et al. (2017) developed the Constructivist ToM Interview to directly test for an understanding that the mind actively interprets and selects information. Children are asked to explain examples of constructive aspects of attention, memory, comprehension, comparing, planning, and inference. Many 12- to 13-year-olds readily gave mentalistic explanations in most of these contexts, and high school students with a broader appreciation of the constructivist nature of mental states were more likely to understand better ways to handle interpersonal conflict, and in turn were more likely to avoid referrals for serious behavior problems in school (Weimer et al., 2017).

Other tests of advanced ToM were not designed to test for constructivist ToM, but the different tests which have been developed may tap the construct in different ways. In Bosacki and Astington's (1999) Social Understanding vignettes, which they used to assess ToM, children are asked to image alternative perspectives and emotions for characters in a scene that could be interpreted in different ways (e.g., two schoolmates approaching a new student on the playground). These investigators reported that among

12-year-olds, those with high scores on this test received higher peer ratings of their social interaction skills.

In the Faux Pas task (Baron-Cohen et al., 1999), children are told stories in which a speaker mistakenly assumes that the listener shares the same perspective on the situation, makes a comment about the situation that turns out to be an unintentional insult to the listener, and then misinterprets the listener's reaction. Investigators have found that children develop an understanding of faux pas between 6 and 11 years of age, and that students who evidenced difficulty in understanding faux pas stories were more likely to experience peer rejection (Banerjee et al., 2011).

Happé's (1994) Strange Stories task measures children's ability to explain the mental states of speakers who make different types of statements. White et al. (2009) found that 4 of the 12 stories are more difficult to explain than the others for 8- to 13-year-olds. The four difficult stories require understanding that the speakers are either succeeding or failing to tailor their messages to the listeners' perspectives: Double-bluff (the speaker tells the truth because he anticipates that the listener will expect him to lie); Misunderstanding (the speaker mistakenly assumes that the listener's perspective is congruent with hers), and White Lie and Persuasion (in both stories, the speakers anticipate the listeners' emotional reactions, but for different purposes). The other eight stories can likely be more simply explained by referring to the speakers' intentions (lying, forgetting, using metaphor, being sarcastic, tricking, pretending, joking, hiding true emotions). Among 8- to 13-year-olds, high scores on the four difficult stories were related to less peer rejection (for boys) and less loneliness (for girls; Devine & Hughes, 2013).

People differ in their ability or willingness to understand that one interpretation is not necessarily better or more correct, and that two different interpretations can both be right. Individual differences in understanding representational diversity have lifelong implications, not just in personal social interactions and relationships, but also in broader cultural and political contexts where it is important to understand how people's background experience in terms of gender, race, or ethnicity affects how they will see the world.

Individual differences in understanding representational diversity are likely rooted, at least in part, in differences in how the development of representational ToM in middle childhood is facilitated or challenged. It is especially important to understand the beginning, when the conceptual root of understanding mental representations takes hold. If we overlook or misconstrue the beginning, then we will not be able to understand how the initial understanding that mental states represent the worldly state grows into a constructivist understanding of representational diversity, nor why it takes different directions in different individuals.

A Final Thought

> *It is, as the hundreds of studies make clear, a preschool achievement; 3-year-olds typically fail the false-belief task, 4-year-olds show some success, and 5-year-olds typically succeed* (Miller, 2009, p. 749).

Our hope is that the case that we have made that representational ToM is not acquired until middle childhood will challenge others to reexamine our field's seeming consensus that younger children have already passed the landmark understanding that beliefs are mental representations. Miller (2009) accurately describes the larger view of children's abilities that stems from preschoolers' success on false-belief tasks when he states that we now know that young children are "capable of new forms of social interaction and social understanding; …[they] can manipulate others' mental states in order to tease or deceive; more positively, they can console, cooperate, and in general coordinate their actions with the beliefs and desires of their interaction partners" (p. 749). One worry is that if we, as developmental scientists, overestimate preschoolers' understanding of the mind, we may contribute to overestimations of young children's social abilities in the general public. Parents who harbor developmentally unrealistic expectations of children are at greater risk of raising children with developmental disabilities (Sameroff et al., 1993); parents who assume that infants can read others' minds and can act with negative intent toward others are at greater risk of engaging in abusive behaviors with their children (Bugental et al. 2002).

We appreciate that doing new research on old facts is not generally appealing. But it is important that our characterizations of children's understanding of the mind are as accurate as we can make them, not only for our scholarship in developmental science, but also because research news travels quickly to the public sphere and carries connotations about what is realistic to expect of young children.

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Author Contributions

William V. Fabricius authored or coauthored each chapter. Christopher R. Gonzales coauthored Chapters II, III, and IV. Annelise Pesch coauthored Chapters II and III. Amy A. Weimer coauthored Chapter IX and contributed to all phases of initial draft of Chapter IV. John Pugliese contributed to all phases of the initial draft of Chapter VI. Kathleen Carroll contributed to data collection, coding, analysis, and writing of initial drafts of Chapters V, VII, and VIII. Rebecca R. Bolnick contributed to all phases of initial drafts of Chapters VIII and IX. Anne S. Kupfer contributed to development of the research design and data collection in Chapter III. Nancy Eisenberg and Tracy L. Spinrad contributed to development of the conceptual model, research design, and data collection in Chapter IX.

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Supporting Information

Additional supporting information may be found online (https://doi.org/10.1111/mono.12432) in the Supporting Information section at the end of the article.