THE ROLE OF PERSONAL INVOLVEMENT IN ACCESSING FALSE-BELIEF UNDERSTANDING

by

SUZANNE MARIE PAULINE HALA

B.A., University of Victoria, 1978
M.A., University of British Columbia, 1989

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Department of Psychology

The University of British Columbia
Vancouver, Canada

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ABSTRACT

This dissertation provides the conducting of one of the first systematic explorations of the effects of actively involving young three-year-olds in otherwise standard assessment measures of false-belief understanding. Specifically, the study sequence reported here was designed with the aim of determining whether having young subjects actively participate in the *strategic planning* of a deception would facilitate their performance on questions concerning another's beliefs.

Converging results obtained using an earlier generation of "standard" assessment strategies have been taken by many investigators concerned with developing "theories of mind" to suggest that children under the age of 4 lacked any understanding of false-belief. In contrast to results obtained using these earlier measures, several more recent research efforts have demonstrated surprising competencies among even young 3-year-olds. With these newer results has come a counterpart shift in research interest, away from the threshold question of *whether* 3 year-olds have any false-belief understanding at all, and toward potentially more promising theoretical and procedural questions concerned with how whatever abilities such children already do possess might best be accessed. The series of studies reported here was designed with the purpose of addressing this more current issue through an examination of the effects of modifying standard assessment procedures in such a way as to place subjects themselves in the central role of bringing about another person's false beliefs through their own deceptive efforts.

In the first and most popular of the standard measures, the *unexpected transfer* task, subjects are asked to predict where a story character will search for an object that has been relocated or "unexpectedly transferred" in his or her absence. Study 1 introduced an *active* version of this task, in which subjects themselves were assigned the job of hiding an object from a protagonist. Study 2 is based on the alternative assessment procedure, the *unexpected contents* task. In the standard version of this measure, subjects are shown a stereotypical box the usual contents of which have been removed and replaced with some surprising or "unexpected" contents. Subjects are then required to comment upon what another person viewing the closed box would believe its contents to be. Study 2 , like Study 1, once again afforded subjects with the opportunity to create a false belief in another -- in this case by first hiding the stereotypical contents of a box, and then replacing these original contents with some unexpected object .

The results from these first two studies showed that when subjects were actively involved in planning and carrying out a deception, even the youngest three-year-olds went on to correctly predict another's consequent false beliefs. Of the 40 subjects who completed the procedures in Study 1, 87.5% correctly predicted where the protagonist would mistakenly look for the now relocated object and 70% also correctly responded that a protagonist would think the object was still in its original location. Comparable results were obtained using the active unexpected contents procedure in Study 2. Of the subjects who had the opportunity to themselves substitute some surprise item for the box's original contents, 81% correctly predicted that the protagonist would mistakenly believe the box to still hold its more usual contents.

Study 3 was introduced in order to rule out potential competing explanations for this pattern of results. Because, in contrast to most standard unexpected change tasks, the active tasks reported here employed *real people* in an explicitly *deceptive context,* it was possible that these factors, rather than subjects' active involvement, were responsible for their unusually good performance. Study 3 was designed to control for such alternate explanations by having subjects passively *observe* while an experimenter both planned and carried out a *deception* on another *real person.* In contrast to the good performance obtained on the more active procedures in Studies 1 and 2, of the twenty 3-year-olds who participated in this *observer* condition, only 40% went on to correctly answer questions about the protagonist's resulting false belief.

The results from these first three studies offer strong support for the claim that having young 3-year-old subjects actively involved in the strategic act of carrying out a deception worked to facilitate their performance on otherwise standard false-belief test questions. What was proposed in this thesis was that the good results obtained on the *active* conditions were primarily due to having provided subjects with the opportunity to generate a *strategic plan* aimed at manipulating another's beliefs. Study 4 was designed to further test this hypothesis by introducing a new *planning only* condition in which subjects were required to generate a strategic plan to deceive another but were themselves prevented from physically carrying out this plan. In this fourth, and final study, 60 subjects were randomly assigned to one of three conditions, an *active* condition; and *observer* condition; and the novel, *planning only* condition. As predicted, performance on both the *active* and *planning only* conditions was superior to that of subjects in the *observer* condition. Also in line with the hypothesis put forward in this thesis, subjects in the *planning only* condition were as accurate in their predictions about other's resulting false beliefs as were those in the *active* condition. For the *planning only* condition, 75% of subjects were correct for the *look* question and 70% correct for the *think* question as compared to the *active* condition in which a comparable 80% were correct in their responses to the *look* question and 70% correct for the *think* question.

Taken altogether, the evidence from this series of studies supports the claim that, when allowed to go about the ordinary business of planning their interactions with reference to the mental lives of other persons, even young 3-year-olds clearly demonstrate an understanding that people can come to hold and to act upon false beliefs. The research reported thus not only corroborates evidence from other sources that suggests that 3-year-olds already do have a considerable grasp of the representational nature of the mind but also yields new insights into the importance of providing subjects with a central and active role in assessing the relevance of another's representational state.

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INTRODUCTION

Statement of the Problem

Our everyday maneuvering through a complex social world depends on our ability to predict and explain the actions of others. As adults we obviously rely heavily on our understanding of mental life to guide our social interactions. We predict and explain action on the basis of presumptions about internal states such as beliefs, desires and intentions, and do so in an organized way that suggests we operate in terms of some naive or folk theory of mind (D'Andrade, 1987; Wellman, 1990). In recent years there has emerged a debate among developmental psychologists as to when and how children first come to be appropriately characterized as using such models of intentional action that warrant the title "theory-of-mind".

Much of the debate has centered on determining what it is that young children understand about the nature of *beliefs*, and more specifically, when they first come to the understanding that beliefs can be wrong. The use of false-belief understanding as a crucial marker of whether young children do or do not as yet possess some theory-like understanding of mental lives has arisen because, as Perner (1991) has noted, the whole point of the notion of beliefs is that such constructions may, in fact, be wrong. That is, only when it can be demonstrated that children possess some understanding of the possibility of *misrepresentation* can they be said to truly understand the representational nature of beliefs.

The first wave of research examining young children's earliest insights into the possibility of false-belief understanding initially provided what seemed to be a very robust finding. Based on an assessment procedure introduced by Wimmer and Perner (1983), and subsequently used by numerous others, early results indicated that when required to predict the likely beliefs of an inadequately informed story character, whose original belief had been rendered false through some unexpected turn of events, children younger than 4 or 5 typically responded incorrectly by ascribing to the protagonist knowledge of the real state of affairs, rather than successfully ascribing

a false belief. The poor performance that 3-year-olds displayed in response to false-belief test questions based on such "unexpected transfer" tasks was also apparent when subjects were assessed using closely related procedures such as Hogrefe, Wimmer and Perner's (1986) "unexpected contents" (or deceptive box) task (see also: Perner, Leekam & Wimmer, 1987), in which children younger than four were shown to routinely attribute to others privileged knowledge they themselves had acquired about the atypical contents of some standard container.

Based on the use of these two procedures, which have come to be viewed as "standard" measures of such false-belief understanding, results from this early wave of research led some investigators to the perhaps premature conclusion that children under 4 years of age have absolutely no appreciation of the fact that people can be mistaken in their beliefs. On the strength of such evidence children younger than four have been said to fall short of holding to anything that could qualify as a representational conception of mind (e.g., Gopnik, 1993; Gopnik & Astington, 1988; Hogrefe, Wimmer & Perner, 1986; Perner, 1991; Perner, Leekam & Wimmer, 1987; Wimmer & Perner, 1983).

The typically poor performance of 3-year-olds on such "first generation" false-belief measures stands in puzzling contrast to reports of more naturalistic observations of young children's apparent grasp of certain of the hallmarks of mental life. Well before even 2 years of age, for example, children clearly seem to know when and how to draw another's currently averted attention to an object of interest through the use of referential pointing (Bates, Benigni, Bretherton, Camaioni & Volterra, 1979), and further understand that when an object is occluded from view another person will lack perceptual knowledge of it (e.g. Lempers, Flavell & Flavell, 1977) which suggests that well before 3-years of age children already have in their grasp some understanding of what it means to be ignorant of an event. Similarly, the spontaneous use of teasing and deception (e.g., Dunn & Munn, 1985; Reddy, 1990) evident in young children well prior to their third birthday, has been taken by some to indicate a practical understanding of what it means to lead others into false-beliefs. In addition to these more practical indicators, by three years of age children also make clear reference to beliefs in their everyday speech (e.g., Bartsch, 1990; Bartsch

& Wellman, in press; Bretherton & Beeghly, 1982; Dunn, 1988; 1991; in press; Moore & Furrow, 1991; Moore, Pure & Furrow, 1990; Shatz, Wellman & Silber, 1983; Wellman & Banerjee, 1991; Wellman & Bartsch, in press). On a slightly different note, the early emergence of pretend play has been argued to require the ability to metarepresent, an ability that is also considered to be one of the defining features of false-belief understanding (e.g., Leslie, 1988).

More recently, a number of investigators have sought to resolve the apparent contradiction presented by the fact that young preschool children can be observed talking about and acting upon ignorance and false-beliefs in their everyday interactions on the one hand, but perform poorly on experimental measures of false-belief understanding on the other (e.g., Chandler, Fritz & Hala, 1989; Freeman & Lacohee, 1993; Freeman, Lewis & Doherty, 1990; Hala, 1991; Hala, Chandler, & Fritz, 1991; Fritz, 1992; Lewis & Osborne, 1990; Mitchell & Lacohee, 1991; Moses, 1993; Siegal & Beattie, 1990; Sullivan & Winner, 1993; Zaitchik, 1991). In contrast to the earlier experimental results, these newer research findings provide evidence to suggest that, when tested using measurement strategies which provide perhaps more realistic testing conditions than those that dominated the early literature in this field, false-belief understanding may be shown to be already in place much earlier than previously assumed.

With these newer results the central question of interest has shifted away from that of determining some absolute threshold of false-belief understanding to one of making sense of why such discrepancies emerged between results based on what might loosely be termed "standard" measures of false-belief understanding and newer, potentially more sensitive, versions of the same assessment procedures. The sequence of studies reported in this dissertation was designed to extend this newer wave of research by addressing this more contemporary issue through an exploration of the as yet unexplained consequences of involving subjects in false-belief testing procedures designed to better engage their own self interests and planning efforts.

Before turning to a more detailed examination of the question of how and why the use of assessment procedures better calculated to ensure young children's active involvement in the measurement task could help to uncover young children's previously overlooked competence,

however, certain background considerations that have shaped this thesis need to be made explicit. To this end, attention is first turned to an account of the key role that false-belief understanding has played in the debate over when in the course of their cognitive development children should be credited with some theory-like understanding of mind. An attempt is then made to critically evaluate already available efforts to assess false-belief understanding in young children. Finally, a case will be made for the appropriateness of attempting to devise more interactive and personally relevant measures of false-belief understanding, as is done in this thesis.

REVIEW OF THE RELEVANT LITERATURE

Early theories of mind - the case of false-belief understanding

Throughout the last decade a great deal of interest developed in answering the question of precisely when young children begin to behave in ways that can be said to confidently demonstrate the presence of some theory-like understanding of their own and others' minds. (For recent reviews of this literature see: Astington, 1993; Gopnik, 1993; Frye & Moore, 1991; Lewis & Mitchell, in press; Moses & Chandler, 1992; Perner 1991; Russell, 1993; Wellman, 1990; Whiten & Perner, 1991). Although there is some controversy over whether knowledge of the sort summarized in these publications is truly theory-like (e.g., Harris, 1991; Johnson, 1988), there is, nonetheless, general agreement among most contributors to this literature that young children do in fact acquire some real, if fledgling, knowledge of mental events, including beliefs, before the end of their pre-school years and that this knowledge is sufficiently well organized to make talk of it being at least tacitly theory-like seem coherent (see Olson, Astington & Harris, 1988, also Wellman, 1990). Part of what continues to remain unsettled in this literature, however, is just when in children's early cognitive development such abilities are first in place, and whether such understanding can best be thought of as arriving all of a piece at some particular point in the course of preschool children's development or, conversely whether this so called theory-of-mind is better characterized as developing more gradually. A brief account of each of these matters of debate follows.

Any review of this available literature is made simpler because previous research aimed at examining young children's early representational understanding of the mind tended to fall into one or the other of two mutually exclusive categories. On the one hand, there were those who have claimed that such an understanding of mind is a product of those same developing cognitive structures and functions that support the emergence of language or pretense or other early symbolic and referential abilities, and so is already well in place by the third birthday (e.g., Bretherton &

Beeghly, 1982; Leslie, 1988; Raver & Leadbeater, 1993; Reddy, 1991). On the other hand, there are those who have argued that, whereas 3-year-olds might indeed be able to do all that is claimed for them in these more naturalistic settings, these accomplishments fall short of what is required to constitute a truly representational theory of mind (e.g., Perner, 1991).

The major point of disagreement between these early and delayed onset groups has turned upon what is assumed to count as evidence for any so-called "theory-of-mind". Early on in this controversy, debate largely centered upon the privileged place which some contributors to this literature have assigned to the specific matter of false-belief understanding. During the first round of this ongoing debate advocates of what I and my research colleagues (Chandler, Fritz & Hala, 1989) termed an "early onset view" were generally happy to concede that any mature grasp of the concept of belief would necessarily require some real appreciation of the possibility that beliefs could be false. At the same time, however, advocates of the early onset view were equally quick to insist that other lines of evidence such as the spontaneous use of mental state terms, for example, should also be allowed to stand as evidence on an almost equal footing. This more liberal or inclusive approach to the necessity of accepting procedurally problematic lines of natural evidence grew, in some measure, from what have been seen as the seemingly insurmountable procedural difficulties in more objectively testing for false-belief understanding in 2- and 3-year-olds. It has been argued, for example, that placing young children in unfamiliar and abstract testing situations with unfamiliar experimenters prevents them from demonstrating all that they know about mental life (see Raver & Leadbeater, 1993 for a review). More recently the grounds of this debate have shifted, with advocates of the early onset position working to construct improved or simplified measures of false-belief understanding in attempts to more readily access the competence that very young children might already have (e.g., Chandler et al., 1989; Freeman & Lacohee, 1993; Freeman, Lewis & Doherty, 1990; Fritz, 1992; Hala, 1991; Hala, Chandler & Fritz, 1991; Lewis, in press; Lewis & Osborne, 1990; Mitchell & Lacohee, 1991; Moses, 1993; Siegal & Beattie, 1990; Sullivan & Winner, 1993; Wellman & Bartsch, 1988; in press; Winner & Sullivan, 1993).

The broad consensus about the defining role of false-belief understanding outlined above has arisen in some part because it is generally agreed that only when it can be demonstrated that children possess some such understanding of the possibility of *misrepresentation* can they be said to truly understand the representational nature of beliefs (Perner, 1991). By these lights, if procedures meant to assess children's understanding of beliefs were to instead concern themselves only with representations that actually corresponded with reality, (i.e., were to avoid a focus on matters that are manifestly false), then it would be impossible to distinguish between those children who really understand the nature of beliefs and those who simply match their responses to what they take objective reality to be. Acquiring some better grasp of this debate requires beginning by exploring in some greater depth the evidence arising from observations of children in more natural settings.

Naturalistic evidence of false-belief understanding

Before turning attention to laboratory based investigations of children's false-belief understanding, then, some mention first needs to be made of those lines of more naturalistic evidence which come from children's use of mental state terms in their everyday talk and actions and which suggest that children younger than four do already possess some working theory of mind. Evidence that children operate in their social world with reference to their own and other's mental lives comes from many sources (see Wellman, 1990 for a review). Although briefly referred to earlier in the Introduction, two of these sources have had a direct bearing on the question of what young children understand about false beliefs and as such warrant further elaboration here.

Talking about false beliefs.

One such line of evidence comes from the literature concerning young children's spontaneous use of mental state terms in their everyday speech. For some time research evidence based on parental reports has been available that suggests that by their third birthday children

already employ various mental state terms in their spontaneous speech (e.g., Bretherton & Beeghly, 1982). Still, questions have remained as to whether any and all uses of these terms reflect a legitimate understanding of their meaning. In response to such questions, a number of researchers -- those earlier labeled as advocates of an "early onset" view -- have argued that the ways in which mental state terms are used by these young children are in fact illustrative of a real underlying grasp of mental state concepts (e.g., Bartsch, 1990; Bartsch & Wellman, in press; Dunn & Brown, 1991; Shatz, Wellman and Silber, 1983; Wellman, 1985). More specifically, and with reference to the key understanding of the concept of belief, these investigators have found that by 3 years of age most children are able to talk about beliefs in contrastive ways, suggesting that they not only recognize that beliefs are *about* the world, but that these beliefs can be *different* from reality as well. Wellman and Bartsch (in press), for example, have found that by 3 years of age, children's talk includes explicit reference to false-beliefs and often involves their directly contrasting these beliefs with reality. At three a child might say, for example, "I thought it was busted. But it really works," suggesting that they are cognizant of the fact that their own prior belief about an object was false.

As compelling as this natural language evidence is in demonstrating that very young children at least are talking about false beliefs, there still exists a potential reductionist reading of these natural language data. According to this line of reasoning, it is argued that such preschool children are simply displaying the understanding of an implicit social script in routine situations, and, as such, their words are held to fall short of providing clincher evidence for false-belief understanding. Embedded in this line of argument is the claim that it is not until false-belief understanding can be demonstrated in a novel situation, without the usual supporting cues afforded in many dialogues, can it be concluded that children truly have grasped the concept. In addition, it has been argued that the evidence from natural language studies, even if accepted as reflecting some genuine understanding of false beliefs, remains suspect as a more general indicator of false-belief understanding in 3-year-olds because it is possible that the samples studied might simply be a group of especially precocious children (Perner, 1991).

Spontaneous teasing and deception

A second line of evidence that suggests that children under the age of four have some grasp of what it means to hold a false belief comes from studies of young children's spontaneous use of deception in familiar settings (e.g., Dunn, in press, 1991; Dunn & Munn, 1985; Reddy, 1990). The evidence from this quarter suggests that, in their home environment, when interacting with parents and siblings, children as young as two years of age are already engaging in simple acts of teasing and deception. Similarly, observations carried out by Reddy (1990) suggest that as early as even the first year of life, infants engage in some basic, though limited, forms of teasing and tricking behavior. For example, an infant might offer an object to a parent, while maintaining a serious expression. Then, when the parent reaches for the object, the infant smiles and quickly withdraws it. Reddy suggests that in this sort of teasing the infant demonstrates an understanding of the other's intentions and expectations and the consequences of not fulfilling them. Obviously this sort of teasing is open to similar reductionistic readings as the natural language data (e.g., an ambition to simply thwart another's desire). Equally obviously, the susceptibility of a phenomena to such reductionistic readings is not the same thing as providing more positive evidence to indicate that rich interpretation is actually mistaken.

Less problematic examples of deception are provided by the work of Dunn and her colleagues (e.g., Dunn, in press, 1991; Dunn & Munn, 1985;). Most recently, for example, Dunn found that in situations where the child's own self interests were directly threatened, or to evade punishment, or to obtain some desired but forbidden object, children as young as 2 years of age frequently engaged in deceptive talk or action. For example, a 21 month old girl who badly wanted to play with a bar of soap but was prohibited from doing so claimed that she was "dirty" and so needed the soap. When the initial attempt at deception didn't bring about the desired result, this toddler continued to elaborate the lie by claiming that, not only was she dirty, but that she in fact also had a dirty diaper.

As is the case with examples from natural language studies, however, more reductive readings of this sort of evidence still are possible. Once again it could be argued that even these

more elaborate instances of apparent youthful deceit are no more than social scripts or ritualized game-like patterns of interaction. Following this line of reasoning what would be required in order that such apparently deceptive acts be allowed to stand as serious evidence of true false-belief understanding would be some demonstration that these young children could take steps to deceive another in a novel situation. As was the case with the natural language studies, so long as studies of apparent spontaneous deception cannot be said to offer iron-clad guarantees that the context of the observation is truly novel, then such claims remain open to the challenge that they fall short of providing unequivocal evidence of false-belief understanding. Alternatively, and more in line with the sentiments that drove this thesis, it could prove to be the case that Dunn and Reddy's critics, in their search for a sufficiently "novel" testing situation, ended up going too far in the other direction by creating measurement contexts so foreign and stripped of the usual marks of authenticity found in naturalistic settings that young children failed out of some difficulty due to the novelty of the situation rather than any deep seated lack of false-belief understanding.

Early false-belief measures

As a foil against granting any overgenerous account of what young children know about mental states, and as a concession to their more reductionistic critics, other researchers have held out for more stringent criteria for evidence of false-belief understanding. Employing procedures based loosely on what Dennett (1978) characterized as a "minimally complex" measurement paradigm, Wimmer and Perner (1983) initially presented evidence which they maintained established that children younger than approximately 6 years of age lacked any real understanding of the possibility of false belief and, consequently, any operative theory of mind. The original "unexpected transfer" procedure promoted by these authors has since become a widely accepted litmus test (Wellman, 1988) of young children's early understanding of false belief.

In this widely adopted procedure subjects are told a story that is also acted out with two doll figures. In the most often repeated version the story is about a boy, named Maxi, and his

mother. The essential details of the story include the facts that Maxi has some chocolate which he places in a particular cupboard to come back and enjoy it later. He is then made to leave the room and in his absence his mother moves the chocolate to a second location -- thus the chocolate is *unexpectedly transferred* to a new location. Maxi is then shown as about to return to the kitchen to retrieve his chocolate. At this point subjects are typically asked to predict either where Maxi will *look* for his chocolate or where he *thinks* his chocolate is. Although a subsequent series of minor modifications and simplifications in this procedure (e.g., Hogrefe, Wimmer & Perner, 1986; Perner, Leekam & Wimmer 1987; Wimmer, Gruber & Perner, 1984) have resulted in a lowering of the original threshold of successful performance from 6 years of age, children younger than 4 or 5 years of age continue to regularly respond incorrectly by reporting that Maxi will wrongly *search* or mistakenly *think* that his chocolate is in its actual location, rather than correctly ascribing to him a false belief. More recently, the poor performance levels that young preschool children displayed on these "look" and "think" test questions, after witnessing this original "unexpected transfer" task also have been apparent when subjects are assessed using a counterpart "unexpected contents" (or deceptive box) task (Gopnik & Astington, 1988; Hogrefe, Wimmer & Perner, 1986; Perner, Leekam & Wimmer 1987). In this task subjects are shown a familiar, well marked box which ordinarily contains the depicted item, such as "Smarties" (small brightly colored candies). Upon opening the box, however, it is revealed that instead of containing the expected smarties the box actually contains some other unexpected contents such as pencils. Subjects are then asked either to report on their own prior false belief, or to predict what another person seeing the box for the first time would think was in it. As with the unexpected transfer task, 3-year-olds faced with the unexpected contents task typically respond by reporting what really is the case (e.g., that someone looking at the Smarties box would think that it actually contained pencils) and fail to ascribe a false-belief . Four year old children, in contrast, typically succeed on this measure of false-belief understanding by correctly responding that the other person would think that there were in fact Smarties in the box. Based on the use of these two procedures early research led a majority of investigators to the conclusion that children 3 years of age and under have absolutely no

appreciation of the fact that people can be mistaken in their beliefs (e.g., Gopnik, 1993; Gopnik & Astington, 1988; Hogrefe, Wimmer & Perner, 1986; Perner, 1991; Perner, Leekam & Wimmer 1987;)

In contrast to the regular findings of Wimmer and Perner and their colleagues, other more recent research efforts, using either modified versions of the unexpected transfer task or other more disparate measurement strategies, have demonstrated that 3-year-olds already have some grasp of what it means to hold a false belief (e.g., Chandler, Fritz & Hala, 1989; Freeman & Lacohee, 1993; Freeman, Lewis & Doherty, 1990; Fritz, 1992; Hala, 1991; Hala, Chandler & Fritz, 1991; Lewis, in press; Lewis & Osborne, 1990; Moses, 1991; Siegal & Beattie, 1990; Sullivan & Winner, 1993; Winner & Sullivan, 1992). What is obviously required, if these competing findings and contrasting claims are to be arbitrated is some new way of re-reading the evidence in hand.

Alternate readings of the "standard" false-belief tasks

In spite of a potential for more reductionist interpretations of the natural language and deception research carried out with children as young as one or two, it remained a puzzle as to why 3-year-old children are able to talk so freely about beliefs and to manage their everyday social lives with seeming reference to beliefs and other mental states, and yet consistently perform poorly on "standard" tests of false-belief understanding. In an attempt to address this dilemma various investigators recently have introduced new measurement strategies designed to better draw out what it is very young children do understand about false beliefs. Whereas a few of these attempts have introduced novel assessment paradigms which provide dramatically different routes to accessing false-belief understanding than do the typical standard unexpected transfer and unexpected contents tasks, others have employed only slightly modified versions of the standard tasks. Whatever the route taken, however, all of these researchers have shared as their goal the development of some procedural means of more readily accessing young children's competencies

that previously may have been underestimated by other better known and so more "standard" measures.

An early clue that incidental kinds of task complexity can interfere with the performance of younger subjects on standard measures of false-belief understanding comes from work carried out by the original developers of these standard procedures. For example, results reported in the original Wimmer and Perner (1983) study indicate that most 4- and even many 5-year-olds performed poorly on this measure, with only 6- and 7-year olds demonstrating any kind of consistent competence. Subsequent modifications of these procedures which included memory probes to ensure that subjects were following the story appear to have operated to simplify this task and to demonstrate that false-belief understanding is firmly in place at 4 years of age (Perner, Leekam and Wimmer, 1987).

The more recent efforts of other investigators have often proceeded on the prospect that, in spite of the addition of devices such as memory probes designed to ensure that subjects have understood and remembered the procedure, problems involving incidental task complexity might continue to operate in these standard false-belief measures obscuring a more accurate portrayal of what it is that children younger than four actually do understand about false belief. Such attempts at redesigning standard false-belief measures encompass a broad variety of approaches, ranging from simply changing the wording of the test questions to more radically restructuring the assessment task to make the whole procedure more relevant for the subjects. In spite of this variety, however, these different approaches can be usefully rough-sorted into three broad categories to be described in the sections that follow.

Answering the right question

The first of these general classes of procedural alternatives that are meant to take up the issue of whether spurious task demands are obscuring the otherwise good performance of young children on the more standard tasks is based on the proposal that such standard tasks pose particular *linguistic* difficulties that impede very young children's understanding of the force

behind the questions being asked. For example, Siegal and Beattie (1990) have proposed that young subjects who fail the standard Wimmer and Perner unexpected transfer measure may be interpreting the key test question in ways other than that originally intended. In particular, these investigators suggest that children may fail to understand that the actual goal of the task is to report on where a poorly informed story character would *initially look* for an unexpectedly relocated object, rather than where he or she would *eventually find* it. If subjects are confused in this way about what is being asked, then it would seem reasonable that they would respond in ways more consistent with their own understanding of the *goal* of the story. Were they to read the goal of the inquiry as getting at "Where would Maxi have to look in order to get his chocolate" then the appropriate answer would be "in the cupboard where the chocolate really is". In their efforts to make the meaning of the question more explicit, Siegal and Beattie explicitly asked their subjects about where the protagonist would "look first". With this small modification Siegal and Beattie found that approximately 70% of their 3-year-olds correctly answer the false-belief test questions.

Although these results indicate that with more explicit adherence to standard conversational rules otherwise standard false-belief tasks can be made easier for young subjects, the claims made regarding the abilities of the 3-year-olds who participated in the Siegal and Beattie studies still may not convince those investigators adhering to a cognitive deficit view. The age of Siegal and Beattie's subjects is only one of the reasons for their continued skepticism. The youngest age group in the Siegal and Beattie study consisted of children aged 3 to 4 years, including both "young" and "old" 3-year-olds. This becomes relevant for the reason that there is a growing body of recent research to suggest that by 3 1/2 years a substantial number of children also pass the more standard versions of the false-belief task if adequate probes are included to ensure that subjects are following and remembering the relevant details of the story (e.g., Hala, Chandler & Fritz, 1991; Moses & Flavell, 1990; Perner, Leekam & Wimmer, 1987). In spite of this improved performance for older 3-year-olds, however, these studies also found that most children less than 3 1/2 continue to have difficulty with the standard false-belief tasks. Since the Siegal and Beattie studies do not divide age groups along these lines it is difficult to know how their youngest subjects fared with

their linguistically modified version of the critical false-belief test question, or whether the linguistic manipulation was effective primarily for only the older 3-year-olds. It should also be mentioned that the good results Siegal and Beattie obtained when using a temporally well marked test question have not been replicated by some other researchers who have introduced similar linguistic modifications (e.g. Moses, 1993; Moses & Flavell, 1990).

A similar linguistically based explanation for 3-year-olds' difficulties with standard false-belief tasks has been put forward independently by Lewis and Osborne (1990). Like Siegal and Beattie, these authors maintain that the test questions in traditional false-belief tasks are not adequately temporally marked. As reported above, Siegal and Beattie's efforts concentrated attention upon standard unexpected transfer procedures and turned upon their efforts to provide more definite temporal markings for the key false-belief test question. In contrast, Lewis and Osborne aimed their attempt at providing clearer temporal markings at the newer but equally popular "unexpected contents" (or deceptive box) task developed by Hogrefe, Wimmer and Perner (1986). As described earlier in the introduction, this task involves showing subjects a familiar box, such as a Smarties box, which actually contains some unexpected contents, such as pencils. In the standard versions of this task, subjects are typically asked to comment on their own prior belief as well as to predict what another person looking at the box for the first time would think it contained. In contrast to the usual test questions, Lewis and Osborne introduced clear temporal markings for both versions of the critical false-belief questions. Without such clear marking, these authors argued, children might interpret the goal of the task as being that of reporting on what they currently *know* to be true rather than what they themselves previously *mistook* to be true. Consequently, Lewis and Osborne changed the form of the standard question regarding the subjects' own prior false-beliefs from "what did you think was in the box" to an explicitly temporally marked form such as "what did you think was in the box *before I took the top off*?" In addition, subjects were asked to predict what another person, currently looking at the closed box for the first time would think was in the box *before the box was opened.* In response to these more

specific questions even young 3-year-olds performed better than on the more standard test questions.

The results of Lewis and Osborne's study suggest that at least some of the difficulty that 3-year-olds have had with standard versions of the unexpected contents task are traceable to the failure of these procedures to clearly temporally mark the questions being asked. In addition, because this study provided separate analyses for *young* 3 year-olds as compared to *older* 3-year-olds it provides a more differentiated picture of very young preschool children's abilities that was missing in Siegal and Beattie's study. Although the frequency of correct responses was not as high among the youngest of Lewis and Osborne's subjects when compared to the success rate enjoyed by the 3 1/2 and 4-year-olds, the performance of even the youngest subjects tested was clearly superior to performance on more standard questions.

In spite of these encouraging findings, however, an issue that remains unresolved in the Lewis and Osborne results is the fact that in an earlier study Gopnik and Astington (1988) had employed a similar attempt to carefully provide temporal markings but failed to find any comparable facilitation effect (see also Moses, 1991; Moses & Flavell, 1990). In light of these discrepant findings, it remains possible that some as yet unidentified second feature of the Lewis and Osborne testing procedures was actually responsible for the good performance of their young subjects. Indeed, upon closer examination it appears that although the linguistic manipulations were equivalent across the two studies, the Lewis and Osborne study further differed from the Gopnik and Astington study in at least one additional way. Specifically, when Lewis and Osborne's subjects were asked to comment upon another person's belief they were required to do so in the presence of a *real* rather than hypothetical person, who was currently engaged in the act of looking at the Smarties box while the experimenter had the subject whisper the answer. Thus in addition to providing clearer temporal marking, it is possible that these investigators also succeeded in making their assessment task more "realistic" or contextually better elaborated, and that this factor played an additional facilitating role in subjects' correct responses.

Matters of Salience

The second category of studies includes the work of those researchers who maintain that the difficulties young children evidence in standard false-belief tasks may be a coincidental by-product of the fact that "reality" is afforded more salience than the various mental representations chosen as the focus of such assessment procedures (e.g., Fritz, 1992; Zaitchik, 1991). These researchers suggest that the so called "reality" error commonly seen in the responses of three-year-old subjects to standard false-belief measures is less an expression of any absolute failure on their part to understand false beliefs than it is some special vulnerability to general saliency effects. Similar to arguments made by those who maintain that the youngest children commonly tested using the more usual false-belief procedures may be easily confused or led into answering the wrong question, these researchers maintain that such saliency effects may operate as a smokescreen that obscures from subjects the more relevant details pertaining to another's beliefs and instead misleads them into focusing, mistakenly, on reality.

In order to determine whether such saliency effects might be acting to detract from young children's performance on the more standard tasks, advocates of this view have introduced various attempts to either reduce the saliency of objective reality (e.g., the real location of Maxi's chocolate), or to somehow highlight the importance of the mental representations involved (e.g., Maxi's lack of access to critical information necessary for him to form a correct belief about the current location of the chocolate).

One of the first of the studies aimed at manipulating the saliency of reality within the context of a standard false-belief task was a procedural modification introduced by Zaitchik (1991). In her own modified version of Wimmer and Perner's (1983) unexpected transfer task subjects either directly *witnessed* the transfer of the chocolate from one location to another, as is the case in the standard versions, or, alternatively, simply *heard* rumors about the fact that the chocolate had been moved. Zaitchik found that, in contrast to those 3-year-olds who actually saw the displacement of the chocolate, the majority (72%) of those subjects who only heard rumors about

its new whereabouts correctly answered test questions concerning the ill-informed protagonist's false belief.

In a related program of study, Fritz (1992) also attempted to reduce some of the weight she maintained was usually attached to the concrete realities present in standard unexpected transfer and unexpected contents tasks. In the case of the unexpected transfer task, "pretend" chocolate, which subjects were simply asked to imagine, was substituted for the usual attention-grabbing candy ordinarily used in Wimmer and Perner's (1983) classic "Maxi" task. Fritz proposed that making use of a "pretend" chocolate would not only reduce the mind-grabbing attentional impact of real candy but, in addition, that talking about the target object in such a hypothetical or imaginary way, would underscore for subjects the fact that mental states were of special importance in this task. In a parallel study, Fritz modified an otherwise standard unexpected contents task so that the unexpected feature revolved around the fact that the stimulus box was unexpectedly *empty* rather than filled with some surprising or anomalous and therefore highly salient contents that disproportionately captured subjects' attention. Like Zaitchik, Fritz found that both of these attempts at manipulating the salience of reality resulted in improved performance on otherwise standard assessment procedures, with the majority of her 3-year-old subjects correctly responding to questions about their own and another's false beliefs.

Other studies which might appropriately be grouped together with those by Zaitchik (1991) and Fritz (1992) are those research efforts in which stimulus saliency was manipulated by limiting the subjects' direct *experience* with some aspect of the objective reality. Wellman and Bartsch (1988) for example, designed tasks in which the actual location of the target item was unknown to the child, or in which the desired item(s) were to be found in more than one location. Under these modified conditions, 3-year-olds once again were successful in answering belief attribution questions. In a related vein, Flavell, Flavell, Green and Moses (1990) found that their young subjects performed significantly better when the beliefs about which they inquired concerned matters of "value" rather than matters of "fact" which the child took to be objectively true. According to these researchers, young 3-year-old subjects may have experienced the "fact" beliefs

as so salient as to override any fledgling ability they might have had to understand beliefs contrary to their own.

All of the studies just summarized share the goal of attempting somehow to *reduce* the salience of objective reality in order to better access what it is young children understand about beliefs. The flip-side of this research strategy is taken up by those investigators who, rather than attempting to reduce the saliency of the objective stimulus situation, chose instead to attempt to *increase* the salience of the mental representations (e.g., Fritz, 1992 [as mentioned above]; Mitchell & Lacohee, 1991; Stevenson & Mitchell, 1992; Freeman & Lacohee, 1993). In a modification of an otherwise standard unexpected contents task, Mitchell and Lacohee (1991), for example, found that the effect of highlighting subjects' original beliefs by having them "post" (i.e., put in the "mail") a picture postcard that corresponded to those beliefs, was sufficient to allow the majority of their young 3-year-old subjects to correctly remember their own, prior false belief about the contents of the box. For example, subjects were first shown a box that would have ordinarily contained smarties, and, as in the more standard versions of this task, were asked what they thought was in the box. Where this procedure departed from the standard, however, was that, after subjects responded that they thought the box held smarties, they were then asked to choose a picture of smarties from an array of several picture postcards and asked to "post" this reminder in a "post box" provided for that purpose. Again, as with standard tasks, subjects were then shown that, instead of containing the expected smarties, the box actually contained crayons. After replacing the lid the experimenter asked the subjects, "When you posted your picture in the postbox what did you think was in here?" Most of the young subjects in this *posting* condition, in contrast to a standard version, correctly reported that they had originally believed that the box contained the expected smarties.

In a series of studies designed to extend these results, Freeman and Lacohee (1993) found that not just any cue will do to facilitate subject's recall of their prior false belief. As a result, these authors have gone on to assert that, in order to serve as helpful cue, not only did the picture posted need to be *relevant* to the subjects' belief, but also that the closer the posted material was to being a

representation itself, the more likely it was to facilitate performance on the critical belief question. For example, when faced with a closed egg box, after subjects had stated that they believed the box contained eggs, they were required to post either a *picture* of an egg in the picture condition, or a real *sample* of an egg. Performance in the condition employing selection of an appropriate picture was superior to that in which a real egg had been used as a potential reminder. These authors maintain that because a picture is a representation in and of itself, posting such a picture likely provides a more direct cue to subjects that what is of importance in this task is their representations rather than the real contents of the box.

This last series of studies carried out by Mitchell and Lacohee (1991) and Freeman and Lacohee (1993) go beyond the earlier studies which were aimed at directly reducing the salience of *reality* by explicitly undertaking to manipulate the salience of subjects' *mental representations* through the use of relevant cueing. There is, however, an additional element to the Mitchell and Lacohee (1991) and the Freeman and Lacohee (1993) studies that further distinguishes them from those more straightforward salience manipulations which focus more simply on altering the conceptual weight assigned to various component parts of the testing procedure. In these studies, as will be argued in the following section, not only were subjects' beliefs highlighted by some external cue, such as a picture of what the contents of the box were assumed to be, but, just as importantly for the purposes of this thesis, the highest levels of performance were obtained when subjects themselves were provided with an active role in the process of deciding what this appropriate corresponding cue should be. This additional procedural manipulation of engaging subjects in the task of selecting the relevant picture moves this series of "postbox" studies towards the third and final category of those few remaining research efforts already carried out with a view towards accessing any existing belief understanding already in place in very young children.

Matters of Relevance

This third group of studies directed at attempting to sort out whether 3-year-olds' usual failures on standard false-belief tasks actually marks the presence of some cognitive deficit in belief

understanding or, more simply, reflects some confusion with other aspects of the traditional testing procedures, consists of those research efforts that are directed, in some part, at providing testing contexts which are on the whole more relevant to subjects. Rather than focusing on altering the specific stimulus materials (e.g., substituting imaginary for real chocolate) or altering the particular wording of the test questions, research efforts belonging to this third category have sought to increase the personal relevance of the task by various means such as providing measurement contexts that are more meaningful than standard tasks, for example, or by embedding the testing situation in a narrative in which the subject is required to play some active role.

The "postbox" studies, (Freeman & Lacohee, 1993; Mitchell & Lacohee, 1991) introduced in the previous section, could be interpreted as providing such a relevant context for the subjects by requiring subjects to take an active role in the selection of the appropriate picture to post. In contrast to the good performance obtained when subjects themselves actively decided which picture to post, no strong facilitating effect was found for a comparable posting condition in which subjects were simply handed the appropriate pictorial representation of their beliefs (Freeman & Lacohee, 1993). This finding, although not central to the purposes guiding these studies, might well be taken to suggest that the improved performance reported is not simply a result of boosting the salience of mental representations through some externally imposed perceptual cueing or memory trigger. Rather, the good abilities demonstrated by the 3-year-olds of these studies also may have to do with the subjects' own role in actively choosing the relevant pictorial cue. Providing subjects with this more active role in the selection of the picture may assist them in grasping what is relevant in this task, and do so in a way that goes beyond simply providing such external markers which help to underscore the importance of beliefs rather than the real state of affairs to which they refer.

Some further strength to the adequacy of the hypothesis just introduced is to be found in a distinct but related study by Freeman, Lewis, and Doherty (1990) in which these investigators attempted to correct for what they saw as a failure of standard false-belief tasks to establish any essential "need to know" in the story characters that are portrayed. That is, Freeman and his

colleagues argued that the narrative sequences common to most standard false-belief tasks unfolded in such a way as to rob their story characters of any special motivation (or need to know) for processing and remembering the information that key test questions are intended to retrieve. More specifically, without such a "need to know" subjects are unlikely, these investigators proposed, to take cognizance of the relevant mental representations of the protagonists. By their account, a person with a "need to know" is characterized as having an epistemic desire to sort out some particular but uncertain state of the world. In the traditional "unexpected transfer" task which they argue lacks this "need to know" component, Maxi presumably "sees" the chocolate go into the first cabinet, but has no special "need to know" this fact. Consequently, how Maxi might *represent* this situation is not a matter of particular interest to the typical young subject.

Why should a target character's having some "need to know" facilitate the ability of young children to report on such representations? Freeman et al. argue that by creating a natural belief-desire link such epistemic motives enable preschoolers to make use of a simple causal inference. Freeman et al. operationalize the "need to know" criteria in their adaptation of the standard unexpected transfer task by arranging events such that the target character in their story problems is faced with an acknowledged uncertainty, the reduction of which would lead to his or her clearly forming some relevant belief. In their 1990 study, this assessment procedure took the form of a story about a game of hide-and-seek in which one of the characters, badly "needed to know" the whereabouts of the other, and so cheats by peeking, only to have this ill-gotten information later rendered false by an unexpected turn of events.

Thus, in contrast to Wimmer and Perner's (1983) stories in which the central character's belief about the initial location of the chocolate is formed without any special uncertainty or "need to know", the process of belief formation among the protagonists in the Freeman et al. (1990) stories plays a key role in the plot. In short, this early emphasis on belief formation is hypothesized to alert the child subjects to the story character's mental life, thus presumably making it easier for the child to keep track of the mental state of that target person *throughout* the telling of the narrative, rather than forcing them to unexpectedly reconstruct these seemingly peripheral matters

after the fact. In this respect Freeman et al.'s approach is akin to the postbox studies (Freeman & Lacohee, 1993; Mitchell & Lacohee, 1991) in which posting the picture of the subject's current belief was meant to somehow highlight for the subject the importance of the representational elements of the assessment task.

As was the case with the "postbox" studies (Freeman & Lacohee, 1993; Mitchell & Lacohee, 1991), however, the Freeman et al. studies also included a procedural manipulation that, while perhaps tied to the methodology adopted, went beyond simply highlighting the importance of the central character's mental state. That is, the especially effective manipulation included in the Freeman et al. study was also one that allowed subjects to partially step outside the generally passive role of mere "onlooker" which they are usually assigned in other tests of false-belief understanding. More specifically, in an "acting out" condition, Freeman et al. proceeded, not by asking subjects to merely verbally respond to the test question regarding where the protagonist would search in the hide-and-seek game, but to use the puppet from the story narrative to actively demonstrate where he would search. When 3-year-old subjects were assigned this more active role in working out what the protagonist would think and do, 80% went on to demonstrate an understanding of the story character's false-belief. Again it should be noted that the subjects who participated in this procedure were mostly "older" 3-year-olds, and while it is clear that the experimental manipulation of having subjects themselves actively portray the story character's misguided action, it is less clear whether comparable facilitation would have occurred given a younger sample.

In sum, in both the Freeman et al. (1990) studies and the postbox studies (Freeman & Lacohee, 1993; Mitchell & Lacohee, 1991), although the process of emphasizing beliefs early on in the procedures appears to have played some clear role in facilitating performance on false-belief assessment tasks, in all of these studies performance was highest when subjects themselves were provided with some more active role in relation to their own or another's mental states.

In contrast to those studies where the central aim was to highlight the beliefs held by either various story characters (Freeman et al., 1990) or by the subjects themselves, other investigators,

myself included, (e.g., Avis & Harris, 1991; Chandler, Fritz & Hala, 1989; Chandler & Hala, 1991; Hala 1991; Hala, Chandler & Fritz, 1991; Hala & Chandler, 1993a; 1993b; Sullivan & Winner, 1991; 1993; Winner & Sullivan, 1993) have sought, in one way or another, to directly engage the interests of their child subjects in other's beliefs. For the most part, those who have pursued these research strategies have argued that the ability of young preschoolers to understand the possibility of false belief in others can be assessed in a more representative fashion by adopting procedures aimed at actively involving subjects in tasks that turn upon their own efforts to somehow manipulate the beliefs of others.

Deception as an alternate marker of false-belief understanding

The idea of employing assessment strategies that involve subjects in acts of deception as a way of attempting to index the presence of some theory of mind was originally introduced by Premack and Woodruff (1978) in their early efforts to determine whether chimpanzees impute mental states to themselves and others. Based on this work and the responses it generated (e.g., Dennett, 1978), Wimmer and Perner (1983) have also maintained that deceptive action is a potentially good, although perhaps not "minimal" index of the possession of a theory of mind. This follows for the reason that, inherent in any plan to perpetrate a deceit is an appreciation of, and an attempt to harness for one's own ends, the fact that another may be misled into taking as true something that one personally believes to be false. On these grounds, Wimmer and Perner, as well as many others (e.g., Chevalier-Skolnikoff, 1986; Vasek, 1986), suggest that deception requires the presence of some "meta" or second-order beliefs and, by implication, the presence of some theory of mind. Despite having acknowledged its conceptual relevance, Wimmer and Perner (1983), nevertheless, finally recommended against the use of deception as a measure of false-belief understanding, primarily because they assumed that the additional planning steps which deception requires would lead to a systematic underestimation of children's true capacity for false-belief understanding.

Although deception may indeed be a relatively complex undertaking, children's earliest insights into the possibility of false belief may very well arise in the context of their efforts to coordinate their own and others' conflicting ambitions (e.g., Dunn, 1991; in press). It will be argued here that it may be the very act of working through the strategic planning steps necessary to mislead another that helps the child to keep track of another's changing belief state. Studies of young children's spontaneous use of deception in naturalistic settings are in extremely short supply (e.g., Dunn, 1991; Dunn & Munn, 1985; Reddy, 1990). What little evidence is available does suggest, however, that in their usual environment, when interacting with parents and siblings, children as young as two years of age are already engaging in simple acts of teasing and deception. As was discussed earlier, however, examples of deception in these naturalistic settings are easily subject to more reductive readings and, as such, are often discounted as clincher demonstrations of false-belief understanding.

Whereas research attempts to study children's use and understanding of deception in a laboratory setting are in less short supply, those so far available have tended to produce apparently conflicting results. Among the earliest of these research reports are those studies of deception that made use of competitive game procedures (e.g., Selman, 1980; Shultz & Cloghesy, 1981). In most of these early studies child subjects needed to anticipate their opponent's beliefs, not only about some particular piece of concrete information at hand, but their opponent's beliefs about their own beliefs as well. Research involving such organized, rule-bound games of deceit has tended to demonstrate that subjects do not successfully use deception in such competitive ways until they are older than 5 or 6. Because research of this sort is now seen to conflate an appreciation of the very possibility of deceit with its strategic use within certain elaborate game settings such early findings are not seen to be particularly relevant in the present context.

Subsequent to this work carried out in the seventies and early eighties, there have been several more recent studies that directly sought to examine young children's ability to deceive in ways that do not require moves and counter moves in recursive, rule-bound games. In these more recent studies children are typically given some opportunity to misinform an opponent either

verbally or by deceptive pointing. In one such study carried out by LaFreniere (1988) preschool subjects were instructed to "try to fool" an experimenter about the location of an object. Rather than being free to employ deceptive strategies of their own choosing, however, children in this study were given only the opportunity to lie in response to direct interrogation by an adult experimenter. Under these potentially intimidating circumstances 3-year-olds were not proficient at concealing the true location of hidden objects. LaFreniere further suggested that part of the difficulties demonstrated by his young subjects may lie in motivational factors, in that his 3-year-old subjects were thought to be confused about what the goal of the game was. That is, LaFreniere reported that his young subjects often seemed pleased to demonstrate what a good job they had done in the hiding phase of the procedure by blurting out the hiding location they had chosen. Although somewhat older children seemed to better understand the competitive nature of the game, even they were reported to have relied most heavily on the strategy of simply not answering or "feigning ignorance" when asked about the hiding location they had chosen. As well, even when these older children did attempt to actively mislead the searcher, LaFreniere reports they did not prove to be expert liars. That is, most were said to be unable to effectively control their affective expressions and the resulting nonverbal "leakage" that often resulted which provided "give away" clues to the location of the object. While all of this goes some distance to demonstrate the point that preschoolers rarely make very competent liars, studies of this sort, with their multiple displays of children's immature standing as liars may not focus enough attention on what is of most relevance here, which is whether children of a specific age do or do not yet have any grasp of the very possibility of a lie.

A somewhat higher level of deception was reported by Lewis, Stranger and Sullivan (1989) who found that when 3-year-olds violated a prohibition, 38% spontaneously lied about their actions and a further 24% refused to answer the experimenter when questioned about their minor misdeed. In contrast to LaFreniere's findings, the majority of the 3-year-olds in the Lewis et al. study clearly made some attempts to avoid giving themselves away. Although it is impressive that 62% of these young subjects acted deceptively or secretively without any explicit instructions

to do so, it is important to point out that the behavior of those subjects who behaved secretively by simply withholding information about their rule violation cannot be unequivocally interpreted as evidence of acting to intentionally bring about false beliefs (rather than ignorance) in the experimenter (see Chandler et al., 1989 for a discussion of the distinction between secret keeping and explicit attempt at disinformation).

It was just such problems of interpretation of apparent acts of deception that earlier prompted Dennett (1978) to propose that only procedures that required subjects to go beyond simple "secret keeping" by taking more *active* steps to disinform be held out as satisfying the minimal criterion for attributing deceptive intent. Note, however, that Dennett's criteria for deception (unlike that seemingly adopted by LaFreniere) does not require that the would be deceiver be able to execute the "perfect lie", a lie which defies detection. What is clearly of interest in any attempt to demonstrate children's earliest grasp of the possibility of false belief is when such young subjects first begin taking action meant to lead others astray rather than any measure of how long it takes such young children to become expert deceivers. It was with these considerations in mind that Chandler, Fritz, and I first undertook to construct an experimental procedure which facilitated children's efforts to come up with their own best deceptive solutions to a hiding task (Chandler, Fritz & Hala, 1989; Hala, Chandler & Fritz, 1991).

Our solution to the challenge of accessing children's best deceptive performance was a novel variation upon the familiar theme of "hide-and-seek". At a minimum, what we saw as necessary was some experimental situation in which verbal instructions were few and easily understood by subjects. We reasoned that a game type task could be made to fulfill this requirement if it could be lent an air of familiarity and credibility while, at the same time, keeping task complexity to a minimum. We also undertook to develop methods meant to ensure that subjects were not restricted to a single modality for deception, such as lying, but had sufficient opportunity to invent their own strategies with which to actively manipulate the information available to an opponent.

In this first study (Chandler et al., 1989) young preschool children took part in a hide-and-seek game in which they were given the task of hiding a "treasure" from an experimenter who momentarily had stepped out of the room. The particular challenge facing subjects in this hiding task was that they were required to hide a treasure using a puppet whose comings and goings were given away by a clear trail of tell-tale footprints on a white surface. Subjects were thus faced with the problem of constructing a way to deceive a protagonist as to the whereabouts of the treasure in the face of evidence that otherwise gave the location away.

Through the use of this hide-and-seek procedure Chandler et al. (1989) demonstrated that on or before their third birthday most children could engage successfully in acts of purposeful deceit even in novel situations. A series of subsequent experiments (Hala, Chandler & Fritz, 1991) replicated these findings, and went on further to guard against various reductive readings of these results by demonstrating that such 3-year-old subjects did not simply behave randomly, but could act either deceptively or cooperatively in accordance with instructions, and were capable of correctly answering questions as to the likely consequences of their deceptive efforts on their opponent's actions and their beliefs.

Consistent with the evidence just cited, a number of other studies have also demonstrated that when the interests of the child are actively engaged in bringing about a deception, children as young as 3 years of age already seem to have some grip on what it means to hold a false belief and what steps must be taken to create such mistaken notions in others. For example, in their study of Baka children of the Cameroons, Avis and Harris (1991) found that when subjects themselves hid some food from a target person, most 3-year-olds went on to correctly answer questions about where the target person would mistakenly believe the food to be. In a similar vein, Sullivan and Winner (1993; Winner & Sullivan, 1993) found that when subjects were instrumental in replacing the usual contents of a box with something unexpected, the majority of the 3-year olds went on to correctly predict what another person would think was in the box.

In contrast to the good results obtained in the studies just summarized, however, other researchers have failed to find comparable good performance in their three-year old subjects (e.g.,

Peskin, 1992; Russell, Mauthner, Sharpe & Tidswell, 1991; Ruffman, Olson, Ash & Keenan, 1993; Sodian, 1991; Sodian, Taylor, Harris & Perner, 1991). Although this poor performance has been interpreted by some as demonstrating a lack of *any* understanding of false-belief on the part of the 3-year-olds, alternate explanations can be put forward which locate the cause for these subjects' failures on the level of expertise required in order to succeed in these procedures.

In some of the studies (e.g., Peskin, 1992; Sodian, 1991) subjects were engaged in games in which success turned upon their revealing their true intentions or desires to help others while hiding these same sentiments from those who didn't have their interests at heart. In these procedures subjects first had to state their preference for some object (e.g., a desired sticker) in the presence of an experimenter. After committing themselves in this way, subjects were then confronted, in turn, with two puppet protagonists, one who was portrayed as friendly and would give the subject the treat in question, or a different and supposedly greedy puppet who would keep the treat for himself. The winning strategy in these games is obviously to tell the truth to the friendly puppet and to lie to the unfriendly one, either about the location of the object (Sodian, 1991) or about which object the subject desired (Peskin, 1992). Under these conditions, most three-year olds, and, in fact, many four-year-olds were unable to successfully hide their own desires from the unfriendly puppet.

In a related procedure, Russell et al. (1991) had subjects point to one of two boxes, only one of which held a treat. In the testing phase of this procedure the actual location of the treat was revealed to the subject through a transparent "window", but was obscured from the opponent's view. Like the Peskin and Sodian studies, the particular difficulty facing these young subjects was again the fact that they were pitted against someone who would keep the object for themselves if they gave away its whereabouts. Here the obvious solution, at least from an adult perspective, is to point to the incorrect location, thereby thwarting the greedy opponent. These researchers found, however, that in contrast to this presumably reasonable solution, three-year-olds continued to point to the actual location of the object across as many as 20 trials.

There are several reasons why we should be cautious in accepting the poor performance of three-year-olds on these "greedy opponent" types of procedures as evidence for a lack of appreciation of deception. The first of these is that across these studies, in order to be successful at deception subjects were required, not only to keep in mind the *beliefs* of the protagonist, but also to simultaneously keep track of the protagonist's beneficent or malicious *intentions*. This double loaded obligation constitutes a clear step towards greater complexity than the more straightforward hiding procedure used in the studies reviewed earlier. In the Chandler et al. (1989) hide-and-seek procedure, for example, subjects simply were required to manipulate another's beliefs without having to remember whether their intentions were honorable or not. The only intention subjects were required to keep track of was the opponent's simple intention of finding the object. In contrast, the subjects in the "greedy opponent" procedures were required to keep track of both their opponent's intention to find the object as well as that person's intentions to either keep or give the desired object to the subject.

In addition to having to keep in mind another's complex intentions, successful deception in both the Peskin and Sodian studies, as well as the Russell et al. (1991) study, required that subjects *lie* (either verbally or by pointing) about the location of the desired object while directly confronting their opponent. There are at least two potential difficulties with having lying as the only strategy available to these young subjects. The first is that there is research to suggest that young children find direct lying a difficult enterprise even though they may be capable of other forms of deception (e.g., LaFreniere, 1988; Lewis, Stranger & Sullivan, 1989). Similarly, Chandler et al.(1989) found that very few 3-year-olds chose lying as their preferred means of deception. A second reason to be cautious about accepting failure to lie as evidence that young children are incapable of deception is that in these studies, lying or misleading pointing was the only strategy available to subjects. Working out when you are likely to be caught out in a lie is, as most adults know, not always as straightforward as it would seem. It may be that in their own spontaneous acts of deception children also have some appreciation of this fact and so first come to

use other sorts of deceptive strategies that, while arguably more foolproof, just as surely rely on understanding another's beliefs but require less in the way of expertise.

Finally, in addition to having to keep in mind another's complex intentions, as well as being limited solely to the strategy of lying, successful deception in the Peskin study required that subjects effectively mask their own desires and intentions. Thus the object that subjects were attempting to mislead their opponent about was their own mental state. It may be that the ability to deceive about one's own wishes and desires develops well after already coming to an understanding that one can deceive others about more concrete actions and objects.

Ruffman et al. (1993) recently completed a series of studies that also failed to find evidence for an understanding of deception in 3-year-olds but which, like these "greedy opponent' studies also runs the risk of overloading subjects with incidental task demands. In the "active" condition in these studies, subjects were required to assist one doll figure ("green boy") in stealing a treat. To complicate things green boy was said to always carry a green crayon, but in this case he wanted to make the target person believe that it was "red boy" and not himself who had stolen the treat. In order to be assessed as capable of deception subjects in this experiment had to substitute a red crayon for the usual green one that green boy was rumored to habitually carry. Like the Peskin (1992) Sodian (1991) and Russell et al. (1991) studies, this experiment once again placed very high demands on subjects' abilities to draw multiple inferences and to keep these in mind throughout the procedure. In the case of the Ruffman et al. (1993) studies, subjects had to keep in mind that a red crayon would stand as a clue for the red boy while at the same time keeping in mind the deceptive intentions of the green boy as well as the impact all of this would have on a third, hypothetical target figure.

Finally, this list of studies which have reported failure to demonstrate an understanding of deception in 3-year-olds also needs to include a further study, carried out by Sodian, Taylor, Harris, and Perner (1991), that purports to provide a "near replication" of the hide-and-seek procedure (Chandler, et al., 1989; Hala et al., 1991). Based on results from what is actually a heavily modified version of this original task these authors make the claim that 3-year-olds entirely

lack the ability to attempt deception as well as any understanding of the consequences of deception. There are, however, several reasons to be cautious about this interpretation of their data. First, the task that they used does not as closely match the original Chandler et al. procedure as is claimed or would be desirable. For example, subjects were required to help "drive" a toy truck across a sandbox and then hide the *driver*. After hiding the driver the only way to lay false tracks was to move the truck *without* a driver. Given that the truck is initially hypothetically controlled by the driver it may have been difficult for subjects to make the transition to having a driverless truck become animated. There is, for instance, evidence from the literature on pretend play to support the notion that young preschool children take very great efforts to stay within the loosely defined boundaries of the pretense and object if an experimenter attempts to introduce action anomalous to that pretense (Giffin, 1984).

In addition to the potentially confusing nature of the task, a second area of confusion arises from the manner in which these authors report and interpret the data for both their 3- and 4-year olds. Although these authors make strong claims that 3-year-olds lack an understanding of deception whereas 4-year-olds have a strong grasp of its implications, their data do not clearly support this claim. In fact there was no significant difference found between 3 and 4-year olds for laying false tracks. Rather, where age differences were found they appear to better reflect differences in *expertise* than in absolute ability. For example, 4-year olds required less prompting and were more consistently deceptive across trials. However, if we look more closely at whether 3-year-olds were taking deceptive action at all, 71% produced false trails on at least one trial suggesting that, these subjects may in fact have had some understanding of the fact that misreading information may promote false beliefs.

Finally, Sodian et al. make the point that even in those cases where 3-year-olds were apparently taking steps to deceive their opponent, the majority clearly failed to understand the potential consequences of their action on their opponent's beliefs, as evidenced by their poor performance on the false-belief test questions. What is curious about this interpretation is that Sodian et al. do not apply the same criteria for their 4-year-old subjects. Only 55% of the 4-year-

old subjects answered the test questions correctly, a level of performance which falls short of the level more usually reported for standard unexpected transfer measures of false-belief. Rather than take this suggestive finding as a clue that something might be amiss with their procedure, however, these authors simply maintain instead that the poor performance of the 3-year-olds reflects a failure to grasp the possibility of false belief. This conclusion, which is consistent with other published claims by these authors would be more persuasive if it were applied more even handedly to both their 3- and 4- year old subjects.

In light of these cautions and the potential limitations to the studies that failed to find deception in young preschool children, and given that several studies clearly demonstrated such deceptive competence, the next important step seemed to be to attempt a more systematic examination of the factors which might be providing a facilitating effect on young children's ability to demonstrate false-belief understanding in certain of these more active tasks. A first step towards this goal is to bring together from what has already been suggested a list of potential explanations for the divergence of findings obtained when using active deception assessment procedures like Chandler et al.'s (1989) hide-and-seek task, as compared to more standard sorts of measures of false-belief understanding.

Why do the findings from Hide-and-seek and Unexpected Transfer tasks not agree?

As already suggested in the foregoing review of the literature, there are several potential reasons as to why 3-year-olds might have performed so well on the Chandler et al. (1989; Hala et al. 1991) hide-and-seek measure while continuing to perform poorly on more standard unexpected transfer measures of false-belief understanding. One of these is that Wimmer and Perner's (1983) original unexpected transfer task has been made unnecessarily difficult because it involves subjects as third-party bystanders who simply passively observe the unfolding of a rather drawn out hypothetical narrative about a sequence of events in which they have no personal stake. By contrast, in Chandler et al.'s (1989) hide-and-seek task, successful subjects are personally involved in bringing about a false belief in another person through the use of deceptive maneuvers

of their own invention. The act of purposefully creating a false belief in others requires that one take into account the content of the minds of those one wishes to deceive. In this sense subjects intent upon deception are faced with an interpersonal problem which can only be solved by constructing and executing a strategic plan of action that takes account of another's mental state. It is my contention that providing subjects with an opportunity to engage in such *strategic actions* aimed at finding a solution to a personally relevant problem concerning mental states (in this case how to deceive another person) correctly orients them, and, by not otherwise overburdening them with incidental task demands, draws out their best abilities and enables such young preschool children to demonstrate an already present, if fledgling, understanding of the possibility of false beliefs. Before proceeding to a discussion of why assessment tasks requiring strategic action of this sort might provide a more appropriate procedural means of assessing early theory of mind competence, it might prove useful to follow a more detailed account of just what is meant here by "action". Appendix A is meant to supply this supporting information.

Action as appropriate procedural medium

> ""In the beginning was the Act" as Goethe said, and the operation followed!" (Piaget, 1973, p. 63).

Although there are many alternative avenues along which one might proceed in considering the potential role of action in accessing young children's understanding, the central place given to "praxis" or action by Piaget makes his theory an important place to search for such new insights. For Piaget the term "praxis" referred to action which goes beyond mere reflexive movement to include reference to the actual coordination of such movements in ways that serve to bring about a certain result. In this respect, Piaget, like Searle (1983) and others (e.g., Dretske, 1988), sets this particular form of purposeful or intentional acting apart from simple bodily motion. According to

Piaget, it is through such "praxis" that children act upon and learn about their world. For Piaget, action ontologically preceded thinking.

Even though action is in this broader sense marginally implicated in the behavior in which even the youngest of infants engage, behaviors that clearly qualify as intentional action in the full sense are not achieved, according to Piaget, until the sixth sensorimotor stage, that he claimed is marked by the emergence of what he called "internalized action". With the new understanding of means-ends causality that Piaget maintained occurred at this level, children now became capable of arriving at solutions to self-acknowledged problems through planning. Rephrased in the perhaps more manageable language of contemporary Action theory, (see for example, Frese & Sabini, 1985; Wolf, 1982) young children at this developmental level can now form a *plan of action.*

In the long interim since Piaget originally proposed that action played such a central role in the process of knowledge acquisition, many researchers, while accepting the importance Piaget attached to action, have at the same time, claimed that classical Piagetian assessment methods created difficulties for young children that had little to do with their actual competence in the domain being examined. Threshold levels of understanding in very young children have been claimed by many researchers to be scaffolded by and first surface within the domain of socially-scripted goal-directed interaction (e.g., Bruner, 1990; Donaldson, 1978; Dunn, 1988; 1991; Wolf, 1982).

In her rich account of sibling interaction, for example, Dunn (1988) asserts that if we want to have a clear picture of early competencies we must observe them within their appropriate context. In the case of social understanding this context takes the form of studying the child as *actor* or protagonist, acting on his or her own behalf in a social interaction. Similarly, Bruner (1990) maintains that this "*enactive knowledge*" of folk psychology arises first through participation in family interactions and is most evident in young children's efforts to regulate these interactions. This enactive knowledge, Bruner argues, later provides a basis for children to construct narratives about their actions and the consequences of their actions.

From the work of these and other researchers (e.g., Reddy, 1991) it is clear that, at least in familiar social interactions, when children are observed in strategic goal-directed actions, with the purpose of manipulating some aspect of social interaction, children behave in ways that suggest a working competence beyond that which might be displayed in standard individualized testing situations. The earlier work on deception carried out by my colleagues and myself (Chandler et al., 1989; Hala et al., 1991) was, in part, an attempt to bring just such an opportunity for children to engage in goal-directed or strategic action into the less familiar realm of the testing room.

A similar attempt to make the testing situation more engaging for children was apparent in a recent cross-cultural study carried out by Avis and Harris (1991). These researchers sought to determine whether young Baka children of S. E. Cameroon shared a similar understanding of false-belief as their Western counterparts. Avis and Harris describe in great detail the difficulties they had in setting up an appropriate testing situation for the Baka tribe (including using native testers familiar to the children and placing the testing situation in a context that made real sense to them). Like my colleagues and myself, these authors also ended up relying on a procedure that involved subjects in deceptive hiding tasks embedded in a familiar social context. Such contextual validity was deemed by Avis and Harris to be crucial to ensure that they obtained the best performance possible in a testing situation that might otherwise be strange to the Baka children. It is unfortunate that similar efforts towards contextual validity are rarely seen as so crucial when subjects are obtained much closer to home.

Although it is difficult to directly compare the results of the Avis and Harris (1991) study with results from Western populations because we do not know how these non-Western children would have performed on more standard versions of false-belief tasks, some interesting parallels emerge. Based on their more interactive procedure, Avis and Harris report that 65% of their 3-year-old Baka subjects correctly answered the false-belief test question. While this performance falls somewhat short of the 4-year-olds in their sample, it, nevertheless, exceeds results obtained from most studies using standard false-belief tasks with Western children.

Alternatives to the "Strategic Action" explanation

What is being proposed in this thesis as the likely key difference separating the results from the hide-and-seek task utilized in my own previous research from the typical poor performance of young preschool children on more standard sorts of false-belief tasks is that hide-and-seek tasks of this sort engage subjects in *strategic action* specifically aimed at manipulating the beliefs and behavior of another person. Although it seemed reasonable that this sort of active involvement might be a crucial factor operating to facilitate performance on false-belief test questions, there existed a number of other differences between the hide-and-seek task and more standard sorts of measurement strategies which might also account for any improvement. A list of some of these likely differences follows.

The most obvious of these differences involves the presence of the tracks left by the puppet in the hide-and-seek task employed by the Chandler et al.(1989) and the Hala et al. (1991) studies. It could, and has been argued (e.g., Sodian et al., 1991) that the presence of such tracks artificially inflated the proportion of correct responses by providing some sort of external marker as to the correct response. In this case all children would have had to do was read off the response in a mechanical fashion without any real understanding of false-beliefs. Given this possibility, what seemed necessary as a next step in the process of isolating the effects of false-belief tasks involving deceptive interaction was to design a task that included the active deception component of the Chandler et al. hide-and-seek task but that was at the same time stripped of any incidental attributes that might give subjects tested on this measure some inappropriate procedural leg-up.

A second factor potentially contributing to the better performance of subjects on certain of the hide-and-seek tasks previously reported has to do with the way that these procedures served to form the false-belief of the protagonist. In standard unexpected transfer measures of false-belief understanding the protagonist *starts* with a true belief about the location of the object and this belief only becomes false after some artfully arranged unexpected turn of events that is known to the subject, but not to the target character. In contrast, in the usual hide-and-seek task, the protagonist does not *start* with an already formed belief but instead appreciates that the very nature of the game

being played naturally creates a condition of uncertainty and the need to look for clues as to the hidden object's location. Thus the protagonist is faced with *uncertainty* from the start -- perhaps something akin to what Freeman et al. (1990) describe as a "need to know". It is consequently possible that what is accounting for the good performance of young subjects in the Chandler et al. hide-and-seek task is this "need to know" rather than the fact that subjects themselves play an active and strategic role in bringing about another's false belief.

As a first step towards determining whether active involvement in the strategic planning and executing a deception does in fact contribute to better performance on false-belief test questions what was needed was some new procedure that provided subjects with an opportunity to be actively involved in intentionally bringing about another's false belief, and which, at the same time, eliminated, or held to a minimum, any other of the potential contributing factors just outlined. The first two studies reported in this thesis, consequently, were designed, in part, to determine whether the same good performance obtained on the Chandler et al. hide-and-seek task would also be found on tasks that otherwise closely matched the standard unexpected transfer and unexpected contents measures except that subjects were provided with the opportunity to engage in strategic actions aimed at bringing about false beliefs in another person. Specifically, in the first study to be reported below these efforts were directed towards determining whether modifying an otherwise standard unexpected transfer task so that young children were provided with an opportunity to actively bring about another's false belief would facilitate their performance on false-belief questions. The second study goes on to extend the findings of Study 1 to the most common alternate false-belief task, the unexpected contents task.

Although such efforts to guard against potential reductive readings of the findings from the earlier deception studies and to provide corroborative evidence for young children's early understanding of the fact that such deception leads others to hold false-beliefs (Chandler et al., 1989; Hala et al., 1991) forms an important part of the purposes of this thesis, the more central goal was to provide a systematic examination of the effects of involving subjects in various aspects of this deception. What was proposed was that the most critical component contributing to

subjects' good performance on false-belief test questions based on these more deceptive measures, was that the subjects were themselves required to construct a strategic plan of deception. Study 3, thus was included primarily to rule out the possibility that the good results obtained when subjects participated in the "active" procedures in Studies 1 and 2 were merely a result of embedding the assessment task in a deceptive context and not a result of their active involvement as was claimed. Finally, Study 4 was designed to more directly test the principal hypothesis put forward in this thesis by examining the role that being involved in the *planning only,* as opposed to the *planning and execution,* of the deception has on subjects' performance.

STUDY 1

The new procedures introduced in Study 1 are modeled after perhaps the best known version of the now "standard" unexpected transfer paradigm originally developed by Wimmer & Perner (1983) -- a false-belief task involving a story about a boy named Maxi and his mother. The essential ingredients of the story are that Maxi has some chocolate which he places in one of two available cupboards. He then leaves the room and in his absence his mother *transfers* the chocolate to the second, and for Maxi, *unexpected*, location. Maxi is then shown enroute to retrieving his chocolate. At this point subjects are typically asked to predict either, where Maxi will *look* for his chocolate on his return, or where he *thinks* his chocolate is. Thus in this standard assessment procedure subjects simply *observe* a scene in which a protagonist's originally true belief is rendered false by some unexpected turn of events, in this case the fact that the chocolate is moved from one location to another without Maxi's knowledge.

The central purpose of Study 1 (see also Hala, 1991) was to determine whether providing subjects with a pivotal role in planning and actively bringing about another person's false belief would facilitate 3-year-olds' performance on questions about that other's actions and beliefs. This first study was designed to accomplish this by embedding an otherwise standard unexpected transfer task in a deceptive hiding context in which subjects themselves were instrumental in perpetrating the deception.

As in the usual "Maxi" or unexpected transfer task, subjects in the *active* unexpected transfer task in Study 1 were still required to comment on the likely beliefs of a protagonist, who had not been witness to the unexpected transfer of an object from one location to another. In contrast to the more standard versions in which subjects simply watched this transfer take place, in the *active* version introduced in Study 1, however, subjects were assigned the task of hiding an object from the protagonist and thus were personally instrumental in bringing about the protagonist's false belief. It was hypothesized that casting these subjects in the role of framing and

carrying out a plan to actively deceive another would work to alert them to the beliefs of the target for their deception in ways not required by standard false-belief tests.

Method

Subjects

Altogether 43 three-year-olds were recruited from local urban daycare centers and preschools. Of this total, 3 were later excluded because they were unable to correctly answer control questions meant to determine whether they understood the task. The remaining 40 subjects ranged in age from 2-11 to 3-11 (M = 3-6, 19 males, 21 females).

Procedure

Subjects were seen individually by two experimenters who earlier had visited their classroom on a regular basis. One experimenter (E1) remained with the child throughout the procedure to structure the assessment task and to ask the necessary control and test questions. A second experimenter (E2) took on the role of protagonist, acting as the target for the subject's deceptive efforts.

The procedures followed in this study closely correspond to those of standard unexpected transfer measures with the important exception that here the subject and experimenters played the roles typically assigned to the usual doll figures (for a complete script of this *active* version of the unexpected transfer task see Appendix B). The experimental setting was consequently "life sized" and, in part, constructed by the participants. After introducing the procedure as a "kitchen game" the experimenters engaged the subject in "setting the stage" for the play. The experimenters invited the child's help in laying out a blanket to form the boundaries of an imagined kitchen into which all three participants subsequently entered and sat down. Then E1 remarked that they needed some things for their kitchen and brought out two opaque containers to be used for kitchen cupboards, handing these one at a time to the subject to situate in the pretend kitchen. The color of the container which subjects were handed first was counterbalanced across subjects. E1 then brought out a clear container of "fruitbears", (a "parent-friendly" fruit juice based snack food, less likely to

solicit parental opposition than would chocolate treats) offering one to the subject, and explaining that the treats would serve as "lunch" in their game. Before they could have lunch, however, E1 announced that the toy plates to be used needed washing and E2, the "protagonist" agreed to leave to wash them. Prior to this departure, E2 pointedly placed the fruitbears in one of the opaque containers, explicitly remarking upon the location while doing so. The location chosen was counterbalanced across subjects, alternating between the container closest the subject and the container furthest away from where the subject was sitting.

After noticeably making sure that E2 was not within earshot, E1 announced, in a conspiratorial tone, that she wanted the subject to help her "do something". At this point E1 took the fruitbears out of the original container, placed them on the blanket and then actively encouraged the subject to hide the fruitbears so that E2 could not find them (for exact wording see Appendix B). The arrangement of the testing area was intended to make the second of the two containers the most easily available hiding location and almost all subjects spontaneously chose this hiding place. Those few subjects ($n = 4$) who suggested hiding the fruitbears somewhere else in the room were told that they were required to stay within the boundaries of the "kitchen". No subjects attempted to return the fruitbears to their original location. After the subject had hidden the fruitbears in the second container a control question was asked regarding his or her understanding of exactly what E2 knew and did not know about their hiding efforts. "So, now you've put the fruitbears in the (red or blue) cupboard over here. Did 'Anna' (E2) *see* you move them?" An additional control question was asked to ensure that subjects had correctly remembered where E2 had first put the fruitbears.

Subsequent to posing these *control* questions, but prior to E2's return, subjects were asked two critical *test* questions regarding their understanding of the consequences of their own deceptive actions. These questions separately inquired about where E2 would "look" for the fruitbears and where she would "think" they were located. Asking subjects to predict where a protagonist will *look* is generally considered to be the most acceptable way to assess whether young children understand the possibility of false-beliefs (Wimmer & Perner, 1983; Hogrefe, Wimmer & Perner,

1986; Perner, Leekam & Wimmer, 1987) and studies using the more standard measures have most frequently used such a *look* version of the test question. The alternate "think" question is less frequently used because, it is argued to be potentially more ambiguous in its referent (Wimmer & Perner, 1983). Subjects may, for example, interpret questions about where the protagonist will think the treat is as really asking where he or she will *eventually* come to think it is located. Because, however, some few studies have now included such a think version of the test question, and to provide the widest possible range of comparisons both a *think* and a *look* version of the test questions were included here as follows:

"So now, Anna will want to get the fruitbears when she comes back."

1. Look question: "When Anna comes back where will she first *look* for the fruitbears?",

and

2. Think question: "When Anna comes back where will she *think* the fruitbears are?"

Subjects participated in a single trial which included both test questions. The order of these test questions was alternated across subjects. Finally, the two test questions were followed by a last control question meant to establish whether subjects still remembered the actual current location of the fruit bears.

Results

Control questions and Order effects

In order to assess children's understanding of the possibility of false belief in this assessment context it was judged to be essential to first determine whether they had followed and remembered the sequence of events correctly. On the whole children did very well on the various control questions which served as a measure of their engagement in and understanding of the task. Of the 43 children tested, only 3 failed to respond correctly to any of the control questions. Of these 3 children, 2 came from homes in which English was not the first language, raising the

prospect that they may have had some difficulty in understanding the questions and procedures. These 3 children were excluded from the following analysis.

As an additional check on the effectiveness of the experimental arrangements a record was kept of whether subjects spontaneously "chose" the second and only remaining container as a hiding location or whether some additional prompting was needed. Only 4 subjects did not initially choose the second container as their preferred hiding location, attempting first to hide the fruitbears in alternate locations in the room. When reminded to stay within the boundaries of the pretend kitchen, however, all four went on to choose the alternate container. It should also be noted that, in line with the general findings in the theory of mind literature, no sex differences were found for correct performance in Study 1, or any of the subsequent studies reported in this thesis.

With reference to the more crucial "look" and "think" questions, some previous studies have shown order effects when more than one such test question was asked (e.g., Freeman et al., 1990; Wimmer & Perner, 1983). Consequently, it was important to determine whether any such order effects were present here. A Chi square analysis of these data indicated that responses to the *look* question were unaffected by the order in which the questions were asked. Subjects did respond in ways that might have suggested some minimal tendency towards better performance on the *think* question in those instances in which it followed the look question (74% correct versus 65% correct), but this difference was not significant.

False-belief test questions

Scoring. As in the more standard unexpected transfer measures (e.g. Perner et al., 1987; Wimmer & Perner, 1983), a dichotomous scoring procedure was employed. Subjects were scored as passing the *look* question if they correctly identified, either through pointing or verbally stating the location, that Anna would wrongly go to retrieve the treats from the container in which she had originally placed them. Subjects who incorrectly indicated that Anna would look for the treats in their new location were scored as having failed. Similarly, for the *think* question, subjects were scored as passing if they responded that Anna would think the treats were still in the location she

had left them whereas subjects who incorrectly responded that Anna would think the treats were in their current location were scored as failing. In those few instances where subjects did not immediately answer, or initially responded with "I don't know" the particular test question was repeated. All of these subjects went on to give a response that was scored as pass or fail.

"Look" question. As can be seen in Table 1, the proportion of correct responses was very high when these subjects, all of whom had taken an active hand in hiding the fruitbears, were asked to predict where E2 would mistakenly attempt to retrieve the treats. Overall, 87.5 % of these 3-year-old subjects correctly responded that the protagonist would go to retrieve the fruitbears from the container in which she had originally placed them. A Chi square analysis indicated that this level of performance was significantly better than would be expected by chance if subjects as a group were simply guessing (X^2 (1) = 22.5, $p < .0001$.)

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place Table 1 about here

Because some recent studies have found differences between the performance of "young" versus "old" 3-year-olds (e.g., Moses, 1993; Moses & Flavell, 1990; Sullivan & Winner, 1991), further analyses were carried out after dividing subjects into two age groups: 20 young 3-year-olds (*M*=3-3, range = 2-11 to 3-5; 10 males, 10 females) and 20 older 3-year-olds (*M*=3-9, range = 3-6 to 3-11; 9 males, 11 females). These younger and older 3-year-olds performed nearly identically on this test question (90% correct for the younger and 85% correct for the older 3-year-olds). Chi-square analysis indicated that this difference was not significant.

"Think" question. As with the *look* question, the majority of subjects, 70% overall, correctly responded that the protagonist would mistakenly "think" the fruitbears were in their original location, a difference that was again significantly different from chance ($X^2(1) = 6.4$, $p < .01$.) These results are reported in row 2 of Table 1. Although there was what might have proven to be a trend towards better performance for the older 3-year-olds (75% as compared to 65%) this difference was again not significant (X^2 (1)=.48, $p = .49$ *n.s.*).

Comparison of "Look" and "Think". Although the majority of subjects consistently passed both test questions (67.5%), a level of performance that differs significantly from that expected by chance ($X^2(1) = 20.75$, $p < .001$), the *think* question was marginally more difficult for subjects than the *look* question. A within subject comparison showed that whereas 8 subjects passed the *look* question but failed the *think* question only 1 subject showed the reverse pattern. McNemar's binomial test of proportions indicated that this difference was significant (N=40, $p < .05$).

Discussion

In contrast to the usual poor performance of young children less than 4 years of age, the findings from this first study strongly suggest that, when asked about the results of their own active attempts to deceive another person, even young 3-year-old children correctly responded that another would be taken in by their hiding efforts and so would falsely believe that the fruitbears were untampered with and still in their original location. This is true for both types of false-belief test questions. Even if we adopt the most conservative stance and count only those subjects who passed *both* the *think* and *look* question, the subjects of this study clearly out-performed those reported in most previous studies using less interactive measures. Such earlier studies, that have regularly cast young subjects in essentially passive "observer" roles, have typically reported that 3-year-olds consistently demonstrate a so-called *realistic* bias by responding incorrectly that, a protagonist who had no way of knowing about the transfer of the object, would somehow mysteriously know where the object was currently located. No such realistic response bias was evident in the typical 3-year-old tested in this study.

A part of what still remains unsettled despite these positive results, are the likely reasons as to why the 3-year-olds in this study had more difficulty with the *think* question than with the *look* question. At one level nothing about this finding appears especially mysterious for the same reasons that originally led investigators to deem *think* questions to be semantically more encumbered than *look* questions (Wimmer & Perner, 1983). Despite the seemingly reasonable expectation that mentalistic questions about what others might "think" might well confuse young

persons, nevertheless, there is a body of empirical evidence that suggests that children actually respond to *look* and *think* questions in more or less the same way (e.g., Hogrefe et al., 1986; Perner et al., 1987). One possible reason for the apparent equivalence of the *look* and *think* questions in earlier studies may lie in the fact that in those other less interactive procedures 3-year-olds consistently perform very poorly on *both* test questions whereas 4-year-olds consistently perform well. The possibility remains, therefore, that, whereas the more interactive measures introduced here may succeed in assisting young children in expressing their best false-belief understanding, the emergent character of these developing abilities may leave such young children especially vulnerable to having these fledgling abilities derailed by any confusion introduced through particularly obscure test questions. That is, although the 3-year-olds of this study clearly appear to understand the possibility of false belief, as was evidenced by their good understanding that incomplete information often leads people into mistaken thoughts and courses of action, the trend toward better performance on "look" than "think" questions may reflect a tendency for these emerging abilities to be overridden by unnecessarily semantically complex assessment procedures that feature direct questions about mental state phenomena (see also Lewis, in press).

Following Lewis and Osborne (1990), and Siegal and Beattie (1990), another possible explanation for the present subjects' somewhat better performance on the *look* question is that such questions are simply better temporally marked than are *think* questions. That is, questions about looking refer to a definite action, and consequently, provide a concrete reference to the exact moment in time being inquired into. By contrast, it is possible that "think" questions are understood as being less precise, by referencing a broader span of time. As such, young children could very well suppose that questions concerning where someone would think an object is located could represent an inquiry concerning where the protagonist would *eventually* think the object might be. Although such possibilities may help to explain the slight difficulty some subjects had with the *think* question, such prospects should not be allowed to obscure the general fact that even at their worst, the 3-year-olds in this study did in fact perform considerably better than chance, better than they have typically performed on more passive measures of false-belief understanding,

and that they gave no evidence of being handicapped with any apparent "realistic bias".

STUDY 2

Whereas Study 1 is modeled after standard measures of false-belief understanding in which some unexpected change in the *location* of an object results in a protagonist's false belief, Study 2 employed a different but related set of measures patterned after tasks in which such false beliefs are brought about as a result of the surreptitious switching of the usual contents of a box for something unexpected. This "unexpected contents" measure (also commonly referred to as the "deceptive box" task), developed by Hogrefe, Wimmer and Perner (1986), most typically proceeds by presenting a subject with a familiar container, such as a Smarties box, or M&M's bag, which eventually proves to hold some "unexpected" contents such as a pencil. In order to demonstrate false-belief understanding on such tasks, subjects who first have been let in on the unexpected contents are later required to anticipate what someone who sees only the closed Smarties box would mistakenly think. Those who fail at these measures ordinarily do so by mistakenly attributing privileged knowledge of the box's unusual contents to others who have no reason to know about the substitution of the more unusual contents. Tasks of this sort remove some of the potentially taxing narrative features characteristic of the earlier unexpected transfer measures and, consequently, one could anticipate that performance would be better using these less procedurally encumbered measures. Somewhat surprisingly however, Hogrefe et al. (1986) found that 3-year-olds' performance typically remains poor on such perhaps simplified tasks. So far, the best performance on standard versions of this procedure has been reported by Gopnik and Astington (1988), where half of their 3-year-old subjects responded correctly to these measures. This finding, although suggesting competencies that are somewhat superior to those previously reported, are, nevertheless, still no different from what might be expected by chance.

The second study to be reported here was designed to determine if the introduction of the procedural modification of actively involving subjects in planning and carrying out a deception, as was employed in Study 1, would have a comparable facilitating effect on the performance of young

preschool children when applied to this second, commonly used standard measure of false-belief understanding. Although findings based on *standard* versions of the unexpected transfer task and the unexpected contents task have on the whole proven to be comparable, (e.g., Hogrefe, Wimmer & Perner, 1986), it remained possible that the procedural differences between these two tasks, when transposed into more active measurement strategies like those employed in Study 1, might nevertheless produce disparate results. Study 2 was designed to check for this possibility as well as to provide a replication and generalization of the findings obtained based on the active measures employed in Study 1. To this end, Study 2, as follows, employed procedures that closely parallel standard *unexpected contents* measures but, like Study 1, required subjects to play an active role in carrying out a deception by first *hiding* the stereotypical contents of a box and then themselves *replacing* the original contents with some unexpected object.

Before proceeding to a description of this second study, however, it should be mentioned that, concurrent with the research efforts reported in this thesis and earlier elsewhere (Hala, 1991), Sullivan and Winner (1991; 1993; Winner & Sullivan, 1993) have also independently carried out several studies aimed at assessing the consequences of involving subjects in the task by more or less actively assisting an experimenter carry out a deceptive version of the unexpected contents task. Most recently, Sullivan and Winner (1993) have required their subjects to help "play a trick" on someone else by having them replace the stereotypical contents of some familiar packaged product (e.g., crayons) with an unusual item (e.g., red string). When subjects were asked to play this more active role, the majority of even young 3-year-olds were reported to have correctly answered standard false-belief test questions about the likely contents of these well-marked boxes (69% of young 3-year-olds and 77% of older 3-year-olds succeeded at this task).

Based on these newer results, Sullivan and Winner have reversed their own earlier position (Sullivan & Winner, 1991) in which they concluded that young 3 year-olds generally lacked the metarepresentational competence to understand the possibility of false beliefs. Instead, in this more recent publication, they seem to argue that the previous work did not adequately involve subjects in the role of actively bringing about another's false-belief and as such likely underestimated the

competence of young 3-year-olds. The findings from Sullivan and Winner's most recent work (1993) are more consistent with the results obtained in the present study (see also Hala, 1991) and with the pattern of results to be described in the following section. In spite of this congruence, a number of important procedural confounds in the present Sullivan and Winner studies make it difficult to determine exactly what their evidence has to say about the places of deception and active involvement in false-belief understanding.

In the Sullivan and Winner (1993; Winner & Sullivan, 1993) studies, for example, subjects themselves always begin by first being deceived in the very same manner in which they were later meant to assist in deceiving someone else, and were provided with feedback about the correctness of their claims about their own prior false belief. Although in more standard unexpected contents tasks this first-hand experience has not been shown to have a direct facilitating effect (e.g., Gopnik & Astington 1988), it is possible that in the case where subjects are themselves later required to behave deceptively, such earlier experience might provide some additional cues as to the likely effects of their own deceptive efforts. The present study avoids this potential confound because in this procedure subjects have no immediately prior experience themselves of being taken in by a deception before going on to deceive another person.

In addition, in this "pretest" portion of Sullivan and Winner's (1993) task the protagonist was described as having "guessed" that the stimulus box contained some totally improbable item unrelated to the way in which the box was decorated. In one case, for example, the protagonist stated that a box that had a picture of smarties on the outside actually contained raisins. Such idiosyncratic response on the part of the protagonist could affect subjects' own later responses about the protagonist's beliefs in either a positive or a negative direction. Subjects might assume that the protagonist was not very bright and that he or she was likely to be incorrect in *all* subsequent guesses about the likely contents of a box. If this turned out to be the case then subjects could respond that the protagonist would guess wrongly about the contents of the box, not necessarily because they understood that the protagonist had a legitimate reason to hold to an incorrect belief but rather because he or she might be assumed to always subscribe to improbable

beliefs. Alternatively, such apparently irrational guessing might simply serve to confuse subjects as to the process by which the protagonist ordinarily draws inferences, further interfering with their ability to succeed on measures of false-belief understanding. The active version of the unexpected contents task reported in Study 2, which follows, avoids this potential confusion by structuring events so that both the child and the protagonist are made to start with clear and confirmed knowledge that the container initially holds what it is advertised to hold.

Finally, it is not clear in the Sullivan and Winner (1993) task just how much of an active role subjects were required to play in bringing about the deception upon which the protagonist's false belief was meant to be based. In their procedure it is the *experimenter* who first hides the box's original contents and only then are subjects induced to place a substitute object in the now empty box. The substitute object itself is one left readily to hand and in those cases in which subjects did not spontaneously carry out this substitution the experimenter explicitly provided them with the solution. In contrast, in Study 2 that follows, subjects were set the task of *both* hiding the original contents of the box and then settling upon some substitute object of their own choosing to place in the box they had emptied.

The design of Study 2 thus avoids the procedural confounds present in the subsequent independent studies carried out by Sullivan and Winner by providing a more systematic examination of the role that active involvement in a deception might play in increasing subjects' levels of performance on false-belief test questions. It was hypothesized that when actively engaged in the process of bringing about a protagonist's false beliefs in this way, the majority of a new sample of 3-year-old subjects, like their counterparts in Study 1, would have little difficulty in correctly attributing false beliefs to a protagonist they themselves had set out to deceive.

Method

Subjects

A new group of subjects was once again recruited from the same local urban daycare centers and preschools as were used in Study 1. A total of 27 3-year-olds participated in Study 2 (*M*=3-6, range = 2-11 to 4-0; 11 males, 16 females).

Procedure

As was the case with Study 1, subjects were seen individually by two researchers, an experimenter (E1) who remained with the child throughout the procedure and an assistant (E2) who played the role of the protagonist (see Appendix C for a complete script of the procedure). Again as in Study 1, subjects were invited to help set up a "kitchen game". As distinct from Study 1, however, the main prop employed on this occasion was a box of "Teddy grahams" (small cookies in the shape of teddy bears with which all the subjects were familiar). The box was opened and the subject and E2 were shown that it did in fact contain the expected cookies. As with Study 1, E2 then left the room for the contrived purpose of washing the plates.

While E2 was absent from the room, E1 removed the teddy grahams from the box and invited the subject to hide them from the protagonist. Subjects were free to hide the teddy grahams wherever they chose in the testing room and a number of suitable locations were readily available. After this hiding operation was completed subjects were asked control questions similar to the ones included in Study 1, as well as the following key test questions:

1. "Look" question: "When Anna comes back where will she *go to get* the teddy grahams?" and
2. "Think" question: "When Anna comes back where will she *think* the teddy grahams are before she looks in the box?"

It should be noted that the *think* question used in this second study was modified to a reflect clearer temporal referent than was the case in Study 1. This was done to attempt to reduce some of

potential ambiguity associated with the more standard wording (Siegal & Beattie, 1990; Lewis & Osborne, 1990). Again as with Study 1, these two test questions were asked within a single trial with their order alternated across subjects.

Up to this point this second experiment closely replicates all of the essential details of Study 1. Here, however, the procedure continued by providing the subjects with a further opportunity to deceive the protagonist. The subject was then shown that a large unmarked box which previously had served as the "table" in the first part of the procedure actually contained several small toys including: a slice of plastic pizza; a rubber lizard, spider, and snake; a set of keys; a plastic giraffe; and a plain wooden block.

The experimenter then invited the subject to "play a really funny trick" on the protagonist by choosing a toy from this array of items to put in the teddy graham box. After the subject had made the substitution, usually with a great deal of giggling, the experimenter asked the following test question:

> "Now when Anna comes back, before we let her open the box, what will she *think* is in there?"

While the clause marking the temporal reference in this test question is placed prior to asking what Anna will think is in the box, and the earlier *think* question, asking where Anna would believe the treats to be, placed this marker clause after asking what Anna would think, there is no reason to suspect that this different word ordering would affect subjects' performance. This is supported by research carried out by Gopnik and Astington (1988) in which they found no difference in subjects' performance when test questions were asked in either of these two ways. The test question was followed by a final control question meant to determine whether subjects remembered what the box now actually contained.

Results

All subjects who participated correctly answered all the control questions, indicating that the task was generally well understood. Only one subject did not spontaneously choose a hiding location for the "teddy grahams" and was prompted by the experimenter saying "Think hard, where could you hide them?" "You can hide them anywhere you like." The remaining results of this study are presented in two parts. The first part reflects subjects' responses to test questions in the "hiding" or transfer phase of the task, which stands as a more or less direct replication of Study 1. The second part represents subjects' responses to the false-belief test question regarding the "unexpected" object they themselves had chosen to substitute in place of the original teddy grahams.

Part I -- Object transfer

The data obtained in Part I of these procedures provide further support for the findings from Study 1. As can be seen in Table 2, when subjects were asked the more or less standard false-belief question about where the protagonist would *go to get* the teddy grahams, 81% responded correctly that E2 would go directly to the teddy graham box where she had last seen them.

Insert Table 2 here

This high level of performance closely corresponds to that found in Study 1 and is again significantly different from what would be expected by chance (X^2 (1) = 10.70 $p < .001$) and is superior to levels typically reported when based on standard assessment measures. As in Study 1, in order to assess any potential age differences in performance subjects were once again divided into two age groups with 13 young 3-year-olds (*M*=3-2, range = 2-11 to 3-6,) and 14 older 3-year-olds (*M*=3-9, range = 3-7 to 4-0). Also in accordance with the findings from Study 1

comparable levels of performance were found for both "young" and "old" 3-year-olds (77% and 86% success respectively). Chi-square analysis indicated no significant age differences on this or any of the other test questions employed in Study 2.

Results from the question as to where the protagonist would *think* the cookies were are also comparable to those found in Study 1. The majority of the subjects (70%) clearly understood that the protagonist would mistakenly think that the teddy grahams were still in the teddy graham box. This level of performance is once again better than would be expected by chance (X^2 (1) = 4.48, $p < .034$). Again, no significant age differences were found with 69% of the young as compared to 71% of the older subjects responding correctly. Note that although this version of the *think* question is better temporally marked than was the case is Study 1, there was no corresponding increase in correct responses. Although there still remained a tendency for the *look* question to be answered correctly more often than the *think* question, based on a McNemar within-subject analysis this difference failed to reach significance in this second study.

Part II -- Object substitution

Although the children tested clearly enjoyed participation in the hiding procedure of Part I, Part II, in which they were allowed to substitute some object of their own choosing for the teddy grahams, was even more popular. Incidentally, the objects chosen by the subjects most often were of the "creepy" variety (the snake, spider and lizard), with the fake pizza following close behind. It appears as though by 3 years of age children have reached some consensus on what they think makes a good joke. When asked "Now when Anna comes back, before we let her open the box, what will she think is in there?" 81% of the subjects responded correctly that the protagonist would think there were still teddy grahams in the box. Chi-square analysis indicated that this level of performance is significantly different than would be expected if this group of 3-year-old subjects were responding at chance (X^2 (1) = 10.70, $p < .001$).

Discussion

The new methods introduced here were chosen, not only with an eye toward matching as closely as possible standard unexpected transfer and contents tasks, but also in a effort to blunt the point of previous criticisms that the earlier deception tasks employed by myself and my colleagues (Chandler, Fritz & Hala, 1989; Hala, Chandler & Fritz, 1991) may have operated to somehow artificially scaffold the performance of our previous young subjects. The active procedures used in Studies 1 and 2, while substantially different in form from the earlier hide-and-seek paradigm, nevertheless produced results which closely parallel those earlier findings. As with these previous deception studies, the current findings stand in contrast to those obtained using more standard "unexpected transfer" and "unexpected contents" measures where subjects were cast in passive or non-interactive roles. What does remain procedurally intact from these earlier deception studies, however, is the fact that here, as before, subjects were involved in an interpersonal interaction and were required to play a key part in the process of planning and carrying out an act of deception. In contrast to the view offered by those employing less interactive measures, it appears that having children take an active part in such planning efforts may provide researchers a clearer window onto early competence in false-belief understanding than has been previously found.

Before too confidently concluding in favor of the organizing hypothesis that more standard, non-interactive measures of false-belief understanding systematically underestimate 3-year-olds' emerging grasp of counterfactual beliefs, it was thought to be critical to first undertake still further steps to rule out other potential alternate explanations of the results of Studies 1 and 2. These action-based measurement strategies also differ from most standard tasks on a number of dimensions in addition to involving subjects active participation and it seemed essential to take steps to ensure that any performance gains already observed were not a result of these other minor procedural modifications.

One such "incidental" procedural modification arose from the necessity of employing the subject and other "real" persons in these more interactive tasks. By contrast, in Wimmer and

Perner's original unexpected transfer task (Wimmer & Perner, 1983), and in the subsequent variations on this procedure introduced by others, subjects are not only relegated to the role of passive observers, but the scene they observe is usually portrayed through the use of dolls or puppets. It is possible that the introduction of real people into the procedures of Study 1 and 2, while coincidental to the main purpose of allowing subjects some more active and strategic role, may have actually served to tip the scales in the direction of better performance by helping the whole procedure to achieve a better measure of what Donaldson (1978) describes as "human sense". That is, it could be argued that the inclusion of real actors in the unexpected transfer script embeds the assessment procedure in a social context which is more relevant to subjects and that this manipulation, rather than their active and strategic participation, may account for the performance rates of subjects in Studies 1 and 2.

Counting against any such possibility is the fact that, multiple versions of at least the unexpected contents tasks using real people (e.g., Hogrefe, Wimmer & Perner, 1986; Perner et al., 1987) or videos of real people (e.g., Moses & Flavell, 1990) have already been carried out without yielding significant gains in the performance of 3-year-olds. The few studies that have included the use of real people to act out such unexpected transfers (e.g., Leslie & Frith, 1988; Russell, Mauthner, Sharpe & Tidswell, 1991) have found that young subjects performed no better than they did in the more usual procedures involving doll figures or puppets.

Despite these several lines of evidence, all of which cast doubt on the possibility that the good performance of the 3-year-old subjects in Studies 1 and 2 are the sole consequence of employing real persons rather than doll figures as protagonists, the presence of certain procedural anomalies, especially in those few studies dealing with the unexpected transfer task, still leaves this matter open to some doubt. For example, Russell et al. (1991) found an unusually high proportion of failures on the control questions (35%) among their 3-year-old subjects which could be taken as an indicator that these young children had a more general difficulty following the chain of events in the procedure than is usually the case in standard unexpected transfer tasks. In light of these procedural differences, Study 3 was designed, in part, to guard against the remote possibility that

simply having subjects witness real people carry out the unexpected transfer task would have a facilitating effect on their performance on false-belief test questions.

A more central purpose that guided the construction of Study 3 was to address the question of whether the good performance by the 3-year-olds in Studies 1 and 2 on tasks involving planned deception should be assigned to the fact that they were being asked to be strategic or to the fact that the procedure was embedded in a deceptive context. Against the second of these possibilities is the evidence from the Russell et al. (1991) study which suggests that simply framing research tasks as problems of deception is not sufficient, in and of itself, to increase performance. A somewhat different pattern of results emerged from a study carried out by Sullivan and Winner (1991), however, who found that older, but not younger, 3-year-old subjects improved in their correct responses to false-belief questions in a task that employed a deceptive version of the standard unexpected contents task.

As noted earlier, these authors have recently carried out a series of studies which, like the present study sequence, has been aimed at exploring the role played by personal involvement in otherwise standard false-belief procedures (Sullivan & Winner, 1993; Winner & Sullivan, 1993). In contrast to the conclusions pointed to by Studies 1 and 2 reported here and elsewhere (Hala, 1991) these authors have so far concluded that whatever benefits toward a better understanding of false beliefs 3-year-olds might enjoy in tasks involving planned deception could be assigned to the fact that the context of measurement involved deception, and not at all to the part played by their own active involvement.

In both Studies 1 and 2, and in Sullivan and Winner's (1993) study it is impossible to determine the relative contribution of deception and active involvement due to the fact that the procedures in all of these studies include both. One way of providing a control for this potential confound is to eliminate the active role played by the subject, while retaining the deceptive context. Winner and Sullivan (1993) attempted to provide such a control through including a condition in which children listened to fairy-tales which involved deception (e.g., Red Riding-hood). On the strength of the finding that subjects responses to standard false-belief test questions following

these fairy-tales was comparable to their performance using an "active' deception, Winner and Sullivan concluded that measurement tasks that involve *deception* are in and of themselves sufficient to enable 3-year-olds to produce their best performance and that actually requiring subjects to themselves actively take part in a deception is unnecessary.

There are, however, several reasons to question this conclusion. First, although the performance of young 3-year-olds in Winner and Sullivan's fairy-tale procedure is somewhat better than on standard tasks it still falls seriously short of the high level of performance demonstrated by the 3-year-olds who actively participated in a deception in Studies 1 and 2 reported here, and of those in Sullivan and Winner's (1993) own earlier study . That is, only 50% of the young 3-year-old subjects who heard the deceptive fairy-tale correctly answered the false-belief test questions. Second, a number of studies, including earlier work by Sullivan and Winner themselves (1991), have in fact included a deceptive component but have demonstrated little or no improvement over standard tasks for young 3-year-olds (e.g., Perner, Leekam & Wimmer, 1986; Russell et al., 1991; Sullivan & Winner, 1991).

Finally, although controlling for active involvement on the part of the subject, Winner and Sullivan (1992) introduced another potentially facilitating factor in their procedure. Placing the deception within the context of a fairy-tale may have provided subjects with some additional cues as to the possibility of false belief over and above any facilitating effect derived from simply witnessing a deception. The fairy-tales which are commonly told to children often involve disguise or deceit and a great deal of "make-believe" (e.g., Puss in Boots; Hansel and Gretel; The Three Little Pigs; Snow White) with the potential result being that when faced with deception in the familiar venue of such fairy-tales children may somehow be more alerted to the possibility of false belief in that context. This observation is in no way meant to detract from the potential usefulness of fairy-tales as an assessment procedure for false-belief understanding. On the contrary, using fairy-tale contexts or other narrative procedures (e.g., Lewis, in press) may provide another valuable method for assessing very young children's false-belief competence. What is left in doubt, however, is whether the good performance of Winner and Sullivan's (1993) young subjects

can, as they propose, be attributed to the result of being tested in a procedure involving the passive witnessing of deception or, alternatively, whether the apparent early false-belief understanding demonstrated is a product of the fairy-tale framework in which this testing was carried out.

In contrast to Winner and Sullivan's roundabout attempt at isolating the contribution of deception through the use of fairy-tales, Study 3 was designed to examine the role of deception more directly. Specifically, Study 3 more or less directly replicated the procedure of Study 1, with the important exception that subjects were cast in the role of passive *observers* rather than *actors* directly involved in the relevant deceptions. It was hypothesized that, although some improvement in performance over that ordinarily obtained with standard unexpected transfer measures of false-belief understanding might be obtained because the resulting testing situation necessarily made more "human sense" (Donaldson,1978; Freeman et al. 1990), it was still anticipated that any such gains observed would still fall short of the good performance obtained when subjects are themselves actively engaged in acts of strategic deception meant to bring about the false beliefs in others.

STUDY 3

Study 3 was designed to evaluate the possibility that the good performance of the 3-year-olds in Studies 1 and 2 was not due to the active involvement of these subjects in acts of planned deception but could be explained more parsimoniously by simply pointing to the fact that the procedure employed always made use of real people engaged in real acts of deception, quite apart from any consideration of the active involvement of the subjects themselves. Specifically, the aim here was to determine whether simply observing other *real people* behave *deceptively* might prove, in and of itself, sufficient to facilitate performance on modified versions of standard false-belief tasks. To this end, the procedure for Study 3 was a close copy of Study 1 with the key exception that rather than have the subject actively participate in the deception, the subject simply observed one of the experimenters carrying out a deception on another.

Although the intentionally "interactive" versions of both the unexpected transfer task and the unexpected contents task elicited good performance from the subjects in Studies 1 and 2, the unexpected transfer task was chosen as a framework upon which to build the following control study for several reasons. Study 1 was designed to be an interactive version of the original unexpected transfer task, which, although not the only available false-belief assessment procedure, has come to be taken as something of an "industry standard". Given this, Study 2 was included primarily in an effort to ensure that the good results obtained in Study 1 would generalize across other (i.e., unexpected contents) false-belief measures. If the results on these interactive versions had differed substantially, then it would be important to continue to include both measures in subsequent control procedures. As it is, both young and old 3-year-old subjects performed more or less identically on both of these tasks, thereby providing sufficient empirical justification for basing further questions regarding the relative merits of active involvement on only one of these measures. Of the two, the unexpected transfer procedure was judged to be more amenable to the sorts of

procedural modifications necessary to carry out the subsequent control studies and so was chosen as the focus of Study 3.

As a means of further assessing the impact of having subjects witness a real deception carried out by real people, Study 3 also included a *standard* version of the unexpected transfer task. Like the classic Wimmer and Perner (1983) task after which it was patterned, the standard task employed in Study 3 substituted doll figures for real people and the unexpected transfer was not brought about through any deceptive intent on the part of any of the story characters. If it proved to be the case that the levels of performance obtained on the *observer only* task fell short of the high levels of performance found in Study 1 but were in line with performance on the *standard* task, then further support would be obtained for the claim put forward in this thesis that it is the subjects' active strategic involvement in the deception and not the deceptive context on its own that facilitates performance on measures of false-belief understanding.

Method

Subjects

A new group of subjects was once again recruited from the same local urban daycares and preschools as were used in Study 1. A total of 21 three-year-old subjects completed the two experimental procedures included in this study. Of these subjects one responded incorrectly to the essential control questions and was later excluded from the results. The remaining 20 subjects ranged in age from 3-0 to 3-11 with a mean of 3-6 (10 males and 10 females).

Procedure

Subjects first participated in an *observer-deceptive* version of Study 1 in which the unexpected transfer of an object of interest to the participants is brought about through the deceptive efforts of one experimenter and directed towards a real person (a second experimenter). This procedure was then followed by a standard non-deceptive puppet version of the usual

unexpected transfer task. The two procedures were always presented in this order to avoid the transfer effects that have been found in related studies when the standard unexpected transfer tasks were administered first (e.g.. Freeman, Lewis & Doherty, 1991). (See Appendix D for a complete script of the procedure.)

Observer deceptive condition. As with Study 1, subjects were once again seen individually by two experimenters who visited their classroom on a regular basis, and once again one of the experimenters (E1) remained with the child throughout the procedure to enact the deception and to ask the necessary control and test questions. The second experimenter (E2) acted as the protagonist and was the target for the first experimenter's deceptive efforts.

As was the case with Study 1, the setting for the procedure was again an imaginary "kitchen", but this time, rather than engaging the subject in "setting the stage" for the play, all the set-up was completed by the two experimenters. Once again E1 brought out the "fruitbears", offering one to the subject, and explaining that the treats would serve as "lunch" in their game. E2 again placed the fruitbears in one of the opaque containers before leaving to wash the toys plates, remarking upon the location while doing so.

As was the case with Study 1, after noticeably making sure E2 was not in earshot, E1 removed the fruitbears from the original container and placed them on the blanket. At this point, in contrast to the procedures followed in Study 1, rather than requiring subjects themselves to take responsibility for the hiding of the fruitbears, E1 instead announced, "I'm going to do something. I'm going to *hide* the fruitbears from Ingrid (E2). I'm going to hide them in the red (blue) cupboard," while placing them in the second container. At this point the following control questions were asked: "So, now I've put the fruitbears in this cupboard over here. Did Ingrid see me move them?" "Where did Ingrid put the fruitbears before she left the room?".

Subsequent to these *control* questions, but just prior to the protagonist's return, subjects were asked the two *test* questions regarding their understanding of the consequences of the experimenter's deceptive action:

"So now, Ingrid will want to get the fruitbears when she comes back

1. *Look* question: When Ingrid comes back where will she first *look* for the fruitbears?"

2. *Think* question: "When Ingrid comes back where will she *think* the fruitbears are before she opens the container?"

The order of these "look" and "think" test questions was alternated across subjects and within age groups. Finally, the two test questions were followed by a last control question included to check whether subjects still remembered the actual location of the fruitbears.

Standard unexpected transfer task. The version of the unexpected transfer task that was administered here is a replication of that used in Hala et al. (1991) and which is closely based on a standard unexpected transfer task employed by Perner et al. (1987). In this task subjects were invited to watch a short puppet show. Two puppets, "Katie" and "Sam", were shown playing with a small toy car until being called away for snack. The puppets together put the car away in one of two available opaque containers (red or blue) and left the room. One puppet (Katie) returned first to retrieve the car, playing with it briefly before being called away again. In response, Katie then put the car away again, but this time she placed it in the second container with no justification given for doing so, after which she was made to leave the room. Sam was then shown about to come back into the room and his desire to play with the car once again was explicitly stated. Before his return, however, subjects were asked the necessary control and test questions. The control questions were similar to those asked in the *observer-deceptive* condition and were meant to determine whether subjects had followed the essential sequence of events. The critical test questions were also comparable to those asked in the *observer-deceptive* condition and included both a *look* and a *think* version:

1. *Look* question: "When Sam comes back where will he first *look* for the toy car?" and

2. *Think* question: "When Sam comes back where will he *think* the toy car is before he opens the container?"

The order of these test questions was again alternated across subjects. A final control question was asked to ensure that subjects remembered the current location of the toy car.

Results

The fact that only one participant failed any of the control questions associated with this testing sequence suggests that the procedures employed were clear and generally well followed by these young three-year-old children. The remaining results all speak to the question of whether having subjects simply *observe* another individual carry out an act of deception is, in and of itself, sufficient to enable 3-year-olds to succeed at otherwise seemingly impenetrable false-belief test questions.

Observer-deceptive condition. As can be seen in Table 3 the proportion of subjects' correct responses to the crucial test questions after passively witnessing a deception carried out by someone else fell substantially short of the performance of their counterpart subjects who, in Study 1, were actively engaged in bringing about such deceptions themselves.

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insert Table 3 here

In contrast to the results obtained in Study 1, only 40% of these three-year-olds correctly answered the *look* question, a performance that was not significantly different from what would be expected by chance (X^2 (1)= .8, p =.37). A similar level of performance was found for the *think* question for which only 35% of the subjects answered correctly. Again this level did not differ from that expected by chance (X^2 (1) = 1.8, p = .18). In comparing performance on the *look* versus the *think* question a McNemar within-subject test of proportions was also not significant, indicating that subjects were responding consistently across test questions. When performance across the two test questions was combined, 60% of these 3-year-old subjects failed *both* test questions. Chi-square test against chance indicated that this level of performance was significantly worse than what would have been expected by chance (X^2 (1)= 13.07, $p < .001$).

As with the previous studies in order to assess any potential age differences in the performance of subjects within this intentionally restricted age group, subjects were divided into

"young" and "old" groups (i.e., 10 young three-year-olds, range = 3-0 to 3-6, *M* = 3-3; 6 males, 4 females; and 10 older three-year-olds, range = 3-6 to 3-11, *M* = 3-8, 4 males, 6 females). Fisher's exact tests indicated no significant age differences in the performance of these two age groups on either the *look* or the *think* questions.

Standard unexpected transfer task. Whereas the results just summarized indicated that the poor performance of the majority of the three-year-old subjects who simply observed a deception being carried out fell significantly below the good performance of an equivalent group of 3-year-olds in Study 1, where subjects played a more central role in carrying out the deception themselves, the second set of results to be presented here takes up the question of how this inferior performance compared to the successes and failures of these same subjects on a more typical standard unexpected transfer task -- that is a false-belief task that relied on the use of puppet figures and lacked any evidence of deceptive intent. Performance on the two crucial test questions for the *standard unexpected transfer* condition was remarkably similar to performance on the *observer-deceptive* condition. As shown in table 3, only 40% of these three-year-old subjects correctly answered the *look* question with an identical level of poor performance for the *think* question. Again, no significant age differences were found. Not surprisingly, a within-subject McNemar test of proportions demonstrated that no significant difference existed between performance on the *standard unexpected transfer* condition as compared to that on the *observer-deceptive* condition for either test question.

Finally, it should be noted that because the *observer-deceptive* and the *standard* conditions were not counterbalanced for order, the possibility still remains that witnessing a deception might in fact have some small facilitating effect which might carry over to performance on the standard false-belief task, masking the possibility of a small difference. This possibility seems unlikely however in light of the finding that in the Hala et al. (1991) study, carried out on children drawn from the same pool of subjects, and in which tasks were counterbalanced, no such facilitating effects were found for performance on the standard task when it was administered subsequent to the deception task.

Discussion

The results obtained in this third study suggest that the procedural manipulation of simply using real people in a deceptive context is not sufficient to account for the good performance obtained by subjects in Study 1, or their counterparts on Study 2. This finding is consistent with earlier work by Russell et al. (1991) which found that 3-year-olds performed poorly on false-belief test questions even after witnessing a deception acted out by real persons. The fact that these results fail to support the claims put forth by Winner and Sullivan (1993) who maintain that deception in and of itself plays the essential facilitating role in their own series of studies suggests that other, previously overlooked factors in their study, such as having embedded acts of deception in a fairy tale context, are likely responsible for the good performance of their young subjects.

To summarize, the performance of the 3-year-old subjects who participated in the active versions of the deception based false-belief assessment procedures employed in both Studies 1 and 2 outstripped the performance of their age mates who participated either in the more standard, passive-observer or the modified deceptive versions of the unexpected transfer task administered in Study 3. Based on these results it appears that having 3-year-old subjects engage in goal-directed deceptive action does indeed make a difference to their ability to successfully pass various measures of false-belief understanding. What remains at issue at the completion of these three studies, however, is exactly what it is about subjects' active involvement in such deceptive practices that enables them to more accurately predict the likely actions and beliefs of another.

The sort of behavior engaged in by the subjects who participated in Studies 1 and 2, it is argued here, was not behavior of just any sort, but represents instead those kinds of "intentional actions" carried out to fulfill a specific goal. Such intentional action is not a single faceted event but is instead comprised of several constituent components (Chapman & Skinner, 1984). A discussion

of these different components and how they come into play in the studies reported thus far is provided in Appendix E.

What is proposed here is that the good results obtained in the active versions of false-belief tasks included in Studies 1 and 2 are primarily due to having provided subjects with the opportunity to generate a plan of action aimed at manipulating another's beliefs, and that the physical act of actually carrying out this plan by transferring the object to a new location did nothing to further enhance their performance. The final study in this thesis (Study 4) was designed to test this hypothesis by developing a testing procedure closely patterned after Study 1, but which allowed the *planning* component of the intentional actions featured in Studies 1 and 2 to be isolated from the actual *physical execution* of those plans. That is, Study 4 aimed to distinguish any consequences that might arise from the physical act of carrying out a hiding operation from those owed to the working out of a plan about hiding.

In addition to the central purpose of assessing the relative impact of subjects' involvement in the planning of a deception on their performance on false-belief test questions, Study 4 was also meant to help standardize a number of minor procedural differences that exist across the three studies already reported. Specifically, Study 4 was meant to ensure that the differential results obtained in Studies 1 and 3 were not due to specific sample characteristics or time of measurement effects. Although this was seen as unlikely, as subjects were drawn from the same pre-school populations, Study 4 provided a necessary check for this remote possibility by randomly assigning subjects across 3 different comparison conditions. As well, there are other minor differences across the procedures in Studies 1 and 3 which make a direct comparison of the results problematic. For example, whereas in Study 1 subjects helped to "set the scene" for the kitchen game, in Study 3 subjects simply watched while the experimenters set out the necessary materials. In addition, the wording of the specific test questions differed slightly for the two studies, and there is evidence to suggest that even minor changes in such wording might produce different levels of performance (e.g., Lewis & Osborne, 1990; Siegal & Beattie, 1991). Thus Study 4 was

designed to eliminate the potential confounds arising from all such unnecessary procedural disparities.

Returning to the question of which aspects of active involvement in a strategic deception actually contributed to the increased levels of performance displayed by the 3-year-olds of Studies 1 and 2, this study attempts a partial answer by distinguishing between the *planning* and *physical execution* of deceptive actions. The untying of these components allows for three distinct measurement conditions, as follows. In the first of these, the *active* condition, the subjects were involved in both *planning* where to hide an object and in the manifest physical act of hiding -- or *execution* of the plan. This condition essentially replicates the active versions of the false-belief tasks reported for Studies 1 and 2. Specifically, for the *active* condition in this fourth study, subjects were once again invited to participate by both choosing the hiding location and carrying out the actual hiding of the target objects.

The second condition, the *observer only* condition, replicated the *observer-deceptive* condition in Study 3 and the standard conditions found in more usual versions of the unexpected transfer tasks. In this condition subjects were prevented from having a role in *either* the planning or the execution of the plan. Thus subjects in this *observer only* condition simply watched the experimenter transfer a treat from its original location to some second location with the view of hiding it from a protagonist.

In the third, and novel condition of this study, the *planning only* condition, subjects were given the task of personally constructing a plan concerning how best to hide an object from a protagonist but were prevented from acting directly and merely watched as their plan was physically put into effect by the experimenter. In this sense the subjects provided the "brains" for the deceptive operation while the experimenter provided only the "brawn".

I should point out that for the purposes of symmetry it might be imagined that one could also construct an additional fourth condition in which subjects merely carried out a set of hiding operations exclusively authored by the experimenter. Actually fulfilling this condition in a real testing situation would require somehow preventing subjects from thinking planfully about what

they were doing, however, a restriction that would be next to impossible to implement. That is, because deception is the explicit goal in these tasks, one could not offer any iron clad guarantees that subjects were not themselves entering into some co-conspiracy and spontaneously formulating a plan in line with the experimenter's. Unlike the *planning only* condition in which it was relatively easy to prevent subjects from physically carrying out the hiding operation, it would be impossible to conduct an experiment about deceptive action in which subjects mindlessly carried out only the experimenter's plans.

In summary, the prime purpose of Study 4 was to assess whether engaging subjects in only the *planning* of a deception while restricting them from physically carrying out their deceptive plans would have a facilitating effect on their responses to false-belief questions comparable to that obtained when subjects were provided with the opportunity to both plan and execute such deceptions themselves. Given that children this age are certainly capable of engaging in what Piaget called "interiorized action" it is likely that "goal-directed action" need not necessarily be supported by physical activity to be effective. Specifically, it was hypothesized that the subjects of Study 4 who were required to frame, but did not actually carry out, strategic deceptions would perform equivalently to subjects who not only had a role in the planning but also were involved in the physical execution of the plan, but altogether better than their age mates who completed standardized *observer-only* measures of false-belief understanding.

STUDY 4

Method

Subjects

A new group of subjects was once again recruited from local urban daycares and preschools. Of the 64 three-year-olds who participated in this study, four were later excluded due to incorrect responses to key control questions. The remaining 60 subjects ranged in age from 2-11 to 3-11 (*M* = 3-5, 31 males and 29 females). Subjects were randomly assigned to one of three conditions: an *active* condition (*M* = 3-5, range = 2-11 to 3-11, 10 males, 10 females); a *planning only* condition (*M*= 3-5, range = 2-11 to 3-11, 10 males, 10 females); and an *observer only* condition (*M*= 3-5, range = 3-0 to 3-11, 11 males, 9 females).

Procedure

As with the previous studies reported here, subjects were seen individually by two experimenters who had visited the classroom on previous occasions. Again, one experimenter (E1) remained with the child in the testing room throughout the procedure and the second experimenter (E2) played the role of a protagonist who served as the target for the deception (see Appendix F for full details of the procedural script).

As in Study 1, subjects in each condition were invited to play a "kitchen game" with the two experimenters. Once again, the setting and experimental materials included a blanket meant to define the play area of the "kitchen", two opaque containers and a transparent jar of fruitbears. All three conditions closely follow the script used in Study 1 up until the point of E2's departure. As was the case with Study 1, E2 offered to wash the plates, placed the fruitbears in one of two opaque containers, explicitly commenting on their location, and then left the room. In all three

conditions, E1 then made a deliberate show of checking to make sure that the protagonist was out of earshot. At this point the procedure diverged across the three conditions.

Active condition. Subjects assigned to this condition, like their counterparts in Study 1, were invited by E1 to begin by first hiding the treats from E2. This was accomplished by E1 removing the treats from the container that E2 had earlier placed them in, saying in conspiratorial tones, "I want you to help me do something". "Let's hide the fruitbears from Ingrid." "Here you find a spot". At this point E1 handed the treats to the subject.

Fourteen of these 20 subjects spontaneously chose the second container as their preferred hiding location, as did 4 of the remaining 6 when reminded that they were to choose a hiding location within the blanket that served to mark out the parameters of the pretend kitchen. Two of these subjects appeared somewhat confused and indicated they wished to return the treats to their original location. At this point they were prompted with "Remember, that's where Ingrid (E2) first put the fruitbears, now we want to *hide* them from her. Where could we hide them?"

As was the case in all three conditions, after the treats were hidden, but prior to E2's return, subjects were asked a series of control questions to ensure that they understood that E2 had not seen the treats being moved from one location to another, and that they themselves remembered the current whereabouts of the treats. Similarly, in this and the subsequent two conditions, these "control" questions were once again followed by the questions of primary concern, the critical false-belief test questions. Once again subjects were asked both the "look" question and the "think" question. Because there were no effects for order of test question in Studies 1, 2, or 3, the *look* question was always asked first: "Where will Ingrid *look* for the fruitbears when she first comes in?"; followed by the *think* question, "Where will Ingrid *think* the fruitbears are when she first comes in?" In all conditions, these test questions were followed by a final control question meant to ensure that subjects still remembered the actual current location of the fruitbears.

Observer condition. This condition, like standard unexpected transfer tasks, and like its counterpart in Study 3, reduced the role of the subject to that of a passive spectator, diverting to the experimenter all responsibility for both developing and carrying out a specific plan for hiding the

fruitbears. As with the *active* condition described earlier, and *planning only* condition, to be reported on below, E1 once again made a show of first determining that E2 was no longer able to see or hear what was happening in the testing room before once again removing the fruitbears from the original container and placing them on the blanket. At this point E1 announced in conspiratorial tones "I'm going to do something. I'm going to hide the fruitbears from Ingrid (E2)." E1 then ceremoniously placed the fruitbears in the second container, saying "I'm going to hide the fruitbears in the blue (red) cupboard."

Planning only condition. In this condition subjects were invited by E1 to begin by constructing a plan for hiding the fruitbears so that E2 couldn't find them, but were prevented from personally carrying out their plan because E1 physically placed the fruitbears in the new location they had chosen. As with the *active* condition, after noticeably making sure the protagonist was not in earshot, E1 removed the fruitbears from their original container and announced "I want you to help me do something". "Let's hide the fruitbears from Ingrid." At this point, however, instead of handing the fruitbears to the subject, E1 picked them up and held them saying "Where shall we hide them. You choose a spot." After the subject had chosen a spot, E1 herself then took the necessary steps to place the fruitbears in the location chosen by the subject. As had been the case in the *active* condition, if subjects suggested locations other than one of the two available containers, they were instructed that the fruitbears must be hidden somewhere in the "kitchen". As with the *active* condition, a small number of subjects (n=4) first suggested a hiding location that was not part of the experimental setup. All of these subjects went on to spontaneously choose the second container, however, when they were restricted to selecting a hiding location within the confines of the pretend "kitchen". The experimenter then placed the fruitbears in the chosen hiding location and went on to ask the necessary control and test questions.

Results

Study 4 was designed, in part, to provide replications of Studies 1 and 3. More focally, however, the obvious point of Study 4 was to help determine more precisely which of the various aspects of the so-called "active involvement" condition of Study 1 were primarily responsible for the unusually good showing of the 3-year-old subjects in that study. The replication aspect of these results are reported first, followed by results which address more central questions meant to sort out exactly what particular ingredients of a well executed plan of deception were actually responsible for the subjects' good performance on these false-belief tasks.

Like their counterparts in Study 1, the 3-year-old children who participated in the *active* condition of Study 4 displayed a high proportion of correct responses to the false-belief test questions. As can be seen in Figure 1, 80% of these 3-year-old subjects correctly answered that E2 would mistakenly *look* for the hidden fruitbears in the original container in which she had placed them. Similarly, 70% of these same subjects were correct in their judgments about where E2 would mistakenly *think* the fruitbears were located. A McNemar test of proportions indicated that there was no significant difference in proportion of correct responses across these slightly different measures of false-belief understanding. That is, subjects were generally consistent in their responses, and 70% were correct on *both* test questions, a performance that was significantly better than what would have been expected if subjects were responding randomly (X^2 (1) = 21.6, $p < .001$).

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insert Figure 1 here

In contrast to the good performance evidenced by subjects who participated in the *active* condition, where they both planned and actively carried out a strategic deception, but consistent with performance on the more standard unexpected transfer tasks after which this testing procedure

was patterned, subjects in the *observer only* condition more often than not responded *incorrectly* to the false-belief test questions. As can be seen in Figure 1, only 35% of subjects in this condition correctly answered the *look* question and a comparably low 30% correctly answered the *think* question. As with other of these experimental conditions, subjects tended to be consistent in their responses to the *look* and *think* questions, and a McNemar test of proportions showed no significant difference between responses to these two question forms. Whereas most subjects in the earlier *active* condition had correctly responded to both test questions, the majority (65%) in the present *observer only* condition responded incorrectly to both, a result that is significantly worse than would be expected by chance (X^2 (1)= 17.07, $p < .001$). These results are in line with the performance of the 3-year-old subjects who took part in a similar *observer* condition in Study 3, adding strength to the claim that simply including real people in a deceptive context is not sufficient to access the best performance of these young preschool children.

Whereas inclusion of an active and *observer only* condition in Study 4 was meant to provide a replication of those same methods as those featured in Studies 1 through 3, the addition of the key *planning only* condition was the main concern of the present effort, and was introduced as a direct test of the unique role that strategic planning alone (stripped of the usual action component) might actually play in accounting for subjects' good performance. As can be seen in Figure 1, subjects' performance on the critical test questions in the *planning only* condition was all but identical to the performance of other 3-year-olds who not only planned, but actively carried out their plans for hiding the elusive fruitbears. A total of 75% of the 3-year-old subjects in this condition also correctly answered the *look* question and 70% were correct on the *think* question. Again, subjects were generally consistent in their responses and 70% were correct on *both* the think and the look questions -- a result that was significantly better than would be expected only by chance (X^2 (1) = 21.6, $p < .001$).

In order to compare levels of performance across the three tasks logistic regressions were carried out on responses to both the *look* and the *think* questions across the three conditions, and for comparisons between younger and older 3-year-olds on these measures. For the *look* question

the only significant main effect was for condition type (X^2 (2) = 8.74, $p < .01$). Paired contrasts indicated that there was no significant difference between levels of performance for the *active* and the *planning only* conditions. In contrast, performance on the more classic *observer only* task was significantly different from performance on both the *active* and the *planning only* tasks (X^2 (1) = 7.11, $p < .01$ for *active*; X^2 (1)=5.71, $p < .02$ for *planning only*).

Similarly, for the *think* question, logistic regression indicated a main effect for condition (X^2 (2) = 8.39, $p <.02$). Once again, paired contrasts demonstrated no significant difference when the *active* (planning plus execution) and *planning only* conditions were compared, whereas the standard *observer only* condition was shown to produce significantly poorer performances among these 3-year-olds for both the *active* and *planning only* conditions (X^2 (1) = 5.78, $p < .005$ for *active*; X^2 (1) = 7.12, $p < .01$ for *planning only*).

Logistic regression for the *think* question also indicated a main effect for age group (X^2 (1)= 6.81, $p < .01$). Fisher's exact tests were carried out for each task to check for potential age differences. Only the standard *observer only* condition, in which young 3-year olds performed especially poorly on the think question (9% correct as compared to 56% correct for older 3-year-olds), showed a significant age difference ($p < .05$). For the *active* and *planning only* conditions, while there was a slight trend towards better performance in the older group (*active* condition -- young = 60%, older = 80%; *planning only* -- young = 60%, older =90%), this difference failed to reach significance.

In short, then 3-year-old subjects who supplied only the "brains" behind proposed acts of deception not only evidenced a clear ability to understand the possibility of false beliefs in others, but also performed as well on these measures as did other of their age-mates who not only planned but who had actually carried out such strategic deception.

DISCUSSION

Details and Implications of the Research Findings

Overall, the theoretically driven expectations outlined in the Introduction to this thesis were strongly supported by the results from the sequence of four studies reported here. The most general of these claims was that "standard" sorts of false-belief assessment tasks do not provide sensitive enough means to assess all that young 3-year-old children evidently do understand about their own and other's mental lives, in general, and the possibility of counterfactual beliefs, in particular. The new evidence reported in this thesis adds to the growing body of literature indicating that, contrary to certain earlier claims (e.g., Gopnik, 1993; Gopnik & Astington, 1988; Hogrefe, Wimmer & Perner, 1986; Perner, 1991; Perner, Leekam & Wimmer, 1987; Wimmer & Perner, 1983), young 3-year-old children do in fact understand a good deal about what it means to hold a false belief.

More broadly, the findings reported here are seen to add new weight to an accumulating body of evidence from a variety of sources (e.g., Chandler, Fritz & Hala, 1989; Freeman & Lacohee, 1993; Freeman, Lewis & Doherty, 1990; Hala, Chandler, & Fritz, 1991; Fritz, 1992; Lewis & Osborne, 1990; Mitchell & Lacohee, 1991; Moses, 1993; Siegal & Beattie, 1990; Sullivan & Winner, 1993; Zaitchik, 1991) all of which portray 3-year-olds as capable of understanding far more about the possibility of false-beliefs than has been indicated by such children's generally poor showing on more standard false-belief measures. Instead the general picture that is emerging from this study sequence and the second generation of false-belief measures with which it conforms, is that it now seems increasingly unlikely that children younger than four do in fact suffer some general cognitive deficit blocking them from any possibility of false-belief understanding. Rather, this new line of evidence is more in line with findings obtained from the everyday talk and behavior of 3-year-olds (e.g., Bartsch, 1990; Bartsch & Wellman, in press; Bretherton & Beeghly, 1982; Dunn, in press; 1988; Dunn & Munn, 1985; Reddy, 1990; Moore & Furrow, 1991; Moore, Pure & Furrow, 1990; Shatz, Wellman & Silber, 1983; Wellman

& Bartsch, in press), all of which suggest that false-belief understanding has a considerably earlier onset. It is still possible, of course, to attempt to discount the cumulative results of these and other related studies by suggesting that the early false-belief competence reported is somehow due to either having relied upon especially gifted study populations, or to this or that misleading procedural artifact. This discounting strategy becomes less convincing, however, as the number of such early-competence studies rapidly mounts.

The alternative stance, and one favored here, is to take a more process oriented approach that views false-belief understanding as only gradually becoming more coherent and organized, as opposed to appearing all of a piece at some magical threshold age. Adhering to such a process oriented approach has the effect of shifting the focus of research attention away from the issue of *whether* we can pin down some singular, absolute threshold of false-belief understanding, and instead reinvesting these energies in the potentially more productive task of mapping out the conditions under which developing children eventually come to demonstrate increasingly robust forms of false-belief understanding (Woodfield, 1993).

As Freeman and Lacohee (1993) and others point out, however, early evidence for false-belief understanding of the sort brought out by the results of this and other recent study sequences is not the same thing as a license for ignoring the fact that 3-year-old subjects regularly perform less well than their counterpart 4-year-olds on a variety of false-belief measures. Clearly there remains some unfinished business that needs to be explained. What is important for the purposes of this thesis is to come to some understanding of whether such differential success rates reflect fundamental differences in false-belief understanding, or whether such variability is better understood to reflect difficulties with other, perhaps more task-specific sorts of information processing practices.

At least two obvious possibilities suggest themselves. One of these is that 3-year-olds already have an all-or-nothing grip upon everything that a dichotomous reading of the possibility of false-belief understanding could imply, but are somehow unable to uniformly showcase that understanding because some of the testing instruments that they are obliged to contend with are

unnecessarily complex or procedurally roundabout. This amounts to an attempt to lay responsibility for the fact that 4-year-olds ordinarily perform better than do 3-year-olds entirely at the door of our various uneven measurement difficulties. Alternatively, having already dismissed the possibility that the whole body of evidence supporting some kind of false-belief understanding in 3-year-olds could be artifactual, the only remaining prospect is that the variable performance being turned in by 3- and 4-year-olds actually demands that we abandon the restricted set of either/or possibilities usually envisioned, and requires that we begin moving toward a construction of the problem that allows children to be seen as coming to an understanding of misrepresentation by degrees.

Addressing the first of these possibilities, one line of evidence that speaks against the idea that false-belief understanding comes on line all of a piece comes from the fact that, given sufficiently difficult response alternatives, even 4-year-olds, who are broadly recognized to have a full grasp of the concept of false-belief (Perner, 1991) are often prompted to give incorrect responses to false-belief test questions. As has already been pointed out in the Introduction, even in the original Wimmer and Perner (1983) study, which lacked now standard prompts and probes, a substantial proportion of 4-year-old subjects incorrectly stated that Maxi would search for the chocolate in its actual current location rather than correctly attributing a false-belief to him. In support of the same conclusion, other more recent research efforts also have demonstrated that, under testing conditions only slightly different from those employed in the so-called standard measurement tasks, the average age at which most subjects give clear evidence of false-belief understanding is considerably later than four. Using a "droodles" task in which subjects were shown a restricted view of a larger picture and asked to predict what another person seeing only that restricted view would believe the bigger picture to be, Lalonde, Chandler and Moses (1992), for example, found that most 4-year-olds incorrectly attributed their own privileged knowledge to other less well informed persons. More recently, Lewis (in press), found that when subjects were asked false-belief test questions about familiar fairy-tale characters such as "Goldilocks and the three bears", even the 4-year-olds subjects regularly failed to take account of the fact that the bears

were initially ignorant of Goldilocks' presence in their house. In spite of these and other instances where 4-year-old subjects have failed to demonstrate unequivocal success on measures of false-belief understanding, however, no one in the current theory-of-mind literature is seriously claiming that 4-year-olds ordinarily lack any understanding of false-belief whatsoever. Such evidence of apparently delayed onset does serve as a useful caution against any over reliance on one single assessment procedure to determine the absence or presence of belief understanding at a particular threshold age (Woodfield, 1993).

In general, the view taken and defended in this thesis is better characterized by a brace of conclusions both of which work against a view that 4-year-olds do and 3-year-olds do not have a theory of mind. The first of these is that 3-year-olds already have some real, if fledgling, understanding of the possibility of false-beliefs, and so should be credited with having at least the rudiments of a bona fide first "theory-of-mind". The second is that, although this competence likely first appears as early as 2 or 3 years of age, or perhaps even earlier (see Reddy, 1990) children continue to develop in their expertise in detecting and predicting false-beliefs, at least throughout their preschool years.

The new research reported in this thesis was meant in part to reflect this move away from the notion of an all-or-nothing watershed previously imagined to occur at 4 years of age, and to constitute a step towards a more fine grained analysis of those sorts of conditions under which younger preschool children might best display their understanding of the nature of beliefs. More specifically, the findings of this thesis clearly demonstrate that when actively engaged in the strategic process of carrying out a deception, younger, as well as older 3-year-olds gave strong positive indications of their having understood that such efforts would bring about false-beliefs in another person. As was reported for the *active* conditions included in Studies 1, 2 and 4 , when subjects were provided with an opportunity to both *strategically plan* and *carry out* an act of deception, nearly all of the 3-year-olds went on to correctly answer questions about what that other would mistakenly do and falsely believe. As a step towards more precisely determining what aspects of such strategic involvement are important for facilitating young 3-year-olds' performance

on false-belief measures, the results of Study 4 suggest that what matters most in this procedural manipulation was that successful subjects were those provided with an opportunity to be actively involved in the *strategic planning* of the deception and that , at least in this experimental setup, no additional gains were achieved when subjects also went on to *physically carry out* the plan themselves.

Alternative interpretations

Procedural Scaffolding

Despite the good performance of 3-year-old subjects on the *active* and *strategic planning* versions of the more standard false-belief tasks introduced in this study sequence, skeptics might still raise one or more of the following criticisms or alternate interpretations for these findings. One such criticism which has frequently been leveled against a number of the studies reporting evidence of false-belief understanding in children younger than 4 years of age (e.g., Chandler et al., 1989; Freeman et al., 1990; Hala et al., 1991), is the claim that these newer procedural modifications are somehow acting to artificially "scaffold", or prop-up, proto-abilities rather than accessing some genuine stand-alone false-belief competence (e.g., Gopnik , 1993; Perner, 1991; Sodian et al., 1991). According to these scaffolding arguments whatever abilities are brought out using these newer procedures are somehow not the same thing as what is measured by more standard tasks, or exist in such fledgling forms that it takes a great deal of encouragement on the part of a skilled experimenter to bring them out in what are discounted as extraordinarily supportive contexts.

In possible support of this view is the fact that young 3-year-olds continue to display, more than do older children, difficulties even on some of these newer measures (e.g., Sullivan & Winner, 1991; Wellman, 1990). At the same time, however, 4-year-olds are also susceptible to such contextual effects and this fact is not ordinarily used as a reason to demote their abilities or to call their knowledge of false beliefs into serious doubt. In addition, as the number of alternate measurement procedures producing positive findings for false-belief understanding in 3-year-olds

increases, it becomes less and less likely that such positive results are simply due to specific procedural artifacts.

While, as indicated above, scaffolding arguments of a very general sort have been leveled against all studies that are seen to work against the view that 3-year-olds are marked by some cognitive deficit that wholly blocks them from the possibility of false-belief understanding, a related set of more highly focused criticisms have been leveled against those studies that have explored young children's understanding of deception. With specific reference to the Chandler et al. (1989) and Hala et al. (1991) hide-and-seek measure of false-belief understanding, for example, it has been argued that the presence of the tracks left by the puppet in the hide-and-seek task must have somehow artificially inflated the proportion of correct responses by providing some sort of additional cueing. If this were the case, then the high level of performance found on the false-belief test questions might be a spurious result of particular demand characteristics of the task rather than be indicative of any representational understanding of the nature of beliefs. The new research presented here provides strong evidence against such reductive interpretations of the earlier Chandler et al. (1989) and Hala et al. (1991) results. In this current series of studies, when subjects were actively involved in the strategic planning of a deception, even in the absence of potentially strong visual cues, such as the tell-tale tracks of the hide-and-seek task, most of even the youngest 3-year-olds went on to correctly answer questions about another person's false-belief.

Even in the absence of potentially facilitating visual cues, however, it could still be argued that some sort of scaffolding of performance is afforded through the use of the term "hide" in the current testing procedure. In all of the active conditions of the studies reported here subjects were invited to "hide" a treat from a protagonist. Because hiding games are a familiar activity in the lives of most young children it could be argued that "hiding" is understood merely as a behavioral routine which always results in the other person searching in the "wrong" location. On this reductive view it could be argued that the hiding operations employed by the 3-year-olds in this

series of studies were merely learned behavioral routines carried out without any understanding that this search was the result of the protagonist somehow subscribing to a false-belief.

Potential criticisms of this sort can be countered by several lines of evidence present in this current series of studies. First, the *observer only* conditions built into Studies 3 and 4, like the *active* and *planning only* conditions, also included explicit language as to the hiding of a treat. If "hide" was being read off by the 3-year-old subjects as a behavioral routine blindly associated with wrong headed behaviors on the part of the protagonist, then we should expect that those subjects in the *observer only* conditions, who heard the experimenter refer to her actions as a hiding operation, should have more often predicted that the protagonist would search in the wrong location than is ordinarily the case on a standard unexpected change task, where no such reference to hiding is made. This turned out not to be the case. Subjects who heard the unexpected transfer described using the term "hiding" performed no differently than they did on the more typical standard false-belief task where no such potential cueing was given.

A second line of argument against attempts to reductively reread the good performance of the 3-year-old subjects of these studies who were assigned an active or planning role as simply behavioral arises as a result of the decision to include in the design of Study 2 an active version of the unexpected contents task. In this procedure, in addition to being asked to predict *where* the protagonist would mistakenly assume the cookies to be, subjects were also required to predict *what* the protagonist would wrongly believe the contents of the cookie box to be. If the young subjects who were successful at predicting that the protagonist would search in the wrong location merely as a result of some simple appreciation of the fact that the "script" of hiding games automatically includes a tendency to search in the wrong location, then we would expect them to have more difficulty when faced with the largely unscripted questions about beliefs regarding the unexpected contents of the box. As it turns out, performance levels for questions about where the protagonist would search for the now hidden object and questions about the protagonist's false-belief as to the contents of the box were identical.

Finally, if subjects were responding correctly to false-belief questions on the basis of their having simply computed that "hiding" scripts automatically include searching in the "wrong" location behaviors, then we would expect subjects might do well on questions about where the protagonist would *look*, but be less able to correctly predict what the protagonist would mistakenly *think*. Instead what was found in this current series of studies was that, across all conditions where subjects were required to play an active role in the planning of the hiding operation, the majority of even the youngest 3-year-olds correctly predicted that the protagonist would not only *search* in the wrong location but, would also think wrong thoughts about the true location of the treats.

All in all, then, these several results are seen to go a long way toward discounting the reductive possibility that the procedures employed in these studies somehow left room for subjects to succeed without also having some genuine grasp of the possibility of false beliefs.

Making procedures more "social"

A second alternative explanation for the high levels of performance found in response to the false-belief test questions employed in this study sequence is that all of the measurement procedures introduced here were embedded in social interactions in which the players were real people interacting in realistic ways. In a recent review, Raver and Leadbeater (1993) assert that a part of what divides many of the tasks on which young 3-year-old children demonstrate some understanding of the possibility of false-beliefs from those in which they fail to do so is whether the task is or is not a social one. This social explanation is not seen as a *reductive* interpretation of the results presented here, but rather is viewed as being too broad to account for the particular pattern of findings in this thesis and elsewhere. While it seems feasible that simply placing the task in a social context could very well serve to better engage the interests of young subjects, there are also good reasons to suppose that explanations which rest their case solely on such social embeddedness arguments cannot adequately account for all of the good results obtained across the various modified false-belief tasks employed in this and other study sequences.

While it is true that some of the newer assessment procedures which have found evidence for false-belief understanding at early ages are also more social in character than are the more standard false-belief tasks, it is also the case that 3-year-olds have been shown to perform well on measures that are no more or less social than more standard measures. For example, procedures that were aimed at manipulating the salience of reality or of mental events did so, not by making the testing situation more social but by attempting to alter the conceptual weight that subjects assigned to these different factors (e.g., Fritz, 1992; Freeman & Lacohee, 1993; Mitchell & Lacohee, 1991; Russell & Jarrold, 1992; Zaitchik, 1991). Conversely, a number of studies have increased the social nature of the measurement tasks but still failed to demonstrate improved performance for younger 3-year-old children (e.g., Sullivan & Winner, 1991; Woolley, 1993). Similarly, and more specific to the purposes of this thesis, the *observer only* conditions introduced in Studies 3 and 4 also were highly social in nature, involving subjects in realistic social interactions with the experimenters. Although the young subjects seemed to clearly enjoy the social interchanges occasioned by these conditions, they did not perform any better on the false-belief test questions than when such questions were based on the altogether less social standard task. The claim, then, that is being put forth here is that simply saying that subjects do better on these sorts of modified false-belief tasks because they are more "social" in nature or because subjects themselves are included in the social interaction, is too global an explanation, and one which tells us little about what is actually operating to improve performance. What is needed, and what the research in this thesis was meant to provide is some more fine grained account of just what aspects of the interactive nature of these tasks could account for the facilitating effects observed.

Deceptive context

Related to explanations that point to the potentially facilitating effect of social embeddedness is the claim recently put forward by Winner and Sullivan (1993) to the effect that any assessment task involving explicit deceptive practices serves to alert subjects to the fact that they need to be on the look-out for false-beliefs. In Winner and Sullivan's view it is this

heightened awareness that is responsible for subjects' good performance on these tasks. As with the claims for social embeddedness, however, explanations meant to turn upon some benefit thought to adhere to all deceptive matters once again fails to adequately account for the pattern of results found in this thesis. While it is the case that all of the conditions in which young 3-year-old subjects performed well in the series of studies reported here were in fact housed in explicitly deceptive contexts, not all such deceptive conditions produced comparably good performance. Specifically, the *observer only* conditions in Studies 3 and 4, were just as strongly deceptive in terms of both explicit statements about deception and in terms of the general deceptive atmosphere created by the experimenter as were the *active* and *planning* versions of the same task. In all of these conditions the experimenter made an obvious display of making sure that the absent protagonist could neither see nor hear the events taking place in the testing room, spoke in conspiratorial tones, and explicitly talked about hiding the treat. Even with these strong contextual cues, however, most of these young 3-year-olds -- none of whom were actively involved in carrying out deceptive plans -- incorrectly responded to questions about the protagonists' false-belief. This finding, taken together with findings from other independent research efforts (e.g., Russell et al., 1991; Woolley, 1993) suggests that attempts to explain the good performance of subjects who played some active role in a deception solely on the basis of the procedure providing a deceptive context fails to tell the whole story.

Having made a case for why such alternate explanations do not adequately account for the pattern of results reported in this thesis, attention will now be turned to the question of why participating in the strategic planning of a deception does appear to work to improve performance on otherwise standard false-belief assessment tasks.

Temporal marking of test questions

Finally, it should be mentioned that the good performance of subjects who participated in strategic planning in this series of studies cannot be attributed to use of a temporally well specified test question. Particularly in Study 4, where the test questions in all conditions included an

identical temporally marked wording, only the *active* and the *planning only* conditions produced good performance. Based on these results, it would seem at first glance that the Lewis and Osborne (1990) and Siegal and Beattie (1990) claims that temporal marking works to increase correct performance on false-belief test questions lack support in this Study. In spite of finding no obvious facilitating effect for temporal marking, however, because the present series of studies was not designed with the purpose of assessing any potential differences as a result of the alternate phrasing of test questions, the procedures not provide a fair test of their hypothesis as no direct comparisons are made here between temporally marked versus unmarked test questions.

Relevance of representational states

The findings of the four studies reported here provide support for the proposed hypothesis that when provided with the opportunity to be personally involved in the strategic planning of a deception, even young 3-year-olds would have little difficulty commenting upon a protagonist's resulting false-belief. Having determined that actively constructing such a plan of action does in fact improve subjects' performance on false-belief measures, what remains now is to try to further clarify why such planning is so effective.

One potential reason for this effectiveness is that in order to successfully plan a deception subjects must hold in mind the representational state of the person they wish to deceive right from the beginning. The goal of deception is to manipulate another's beliefs. In order to achieve this goal the deceiver must construct a plan that takes account of the target person's original true belief. When subjects are invited to participate in planning a deception, the target person's current representational state is thus afforded a certain immediate relevance. Similarly, subjects must continue to hold this representational state in mind as they work out their deceptive strategy. In this way the initial true beliefs of the target person remain relevant throughout the testing procedure.

In the typical standard task, by contrast, subjects have no such initial cues that the especially relevant feature of the task is the representational state of a given person or story character. In these more standard versions of false-belief tasks the beliefs of the protagonist are not

clearly cued as relevant until most of the real action is complete. At this point subjects must reconstruct the details of the story to retrieve the original representational state of the protagonist, a view that has since become false. As Sperber and Wilson (1986) point out, in communicative and social interactions people tend to seek out relevance, but are not always accurate in their attempts to do so. As children observe the unfolding of an unexpected transfer task they may be searching in the wrong place for relevance, only belatedly coming to the realization that beliefs are what are at stake. In marked contrast, planning a deception emblazons the representational state of the target person with an unmistakable relevance.

An important part of what might be operating to facilitate performance on the more strategic tasks employed in this thesis is that these deceptive efforts serve to imbue the representational state of the protagonist with a clear relevance that facilitates their efforts to retrieve the original (now false) belief when posed the key test questions at the end of the procedure. It is unlikely, however, that the good performance of the young subjects in the studies reported here is solely due to somehow facilitating their retrieval memory. For one thing, even in standard false-belief tasks usual control procedures designed to test subjects' memory of critical story details already demonstrate that young children have no particular difficulty in remembering the essential facts of the procedure. For example, most 3-year-old subjects correctly remember the original location where Maxi first placed his chocolate, but still go on to incorrectly predict that Maxi will search in the new location where the chocolate now actually resides. Similarly, in all of the conditions included in the series of studies that form this thesis, subjects performed at ceiling on comparable control questions included to ensure they had remembered the critical details of the events and those few subjects who responded incorrectly to the control questions were excluded from the analysis. As such, all the relevant differences in subjects' responses across those tasks in which subjects played some strategic role as opposed to those in which they simply watched the deception take place cannot be laid off to straightforward difficulties with remembering what had taken place.

In a recently completed analysis of potential memory explanations for 3-year-olds typically poor performance on standard false-belief tasks, Freeman and Lacohee (1993) have proposed that

the difficulty for young subjects on such tasks stems from the fact that the details of subjects' current recollections of past beliefs now known to be false are not different from their earlier remembrances of those "same" beliefs before they had been discredited. On their analysis such beliefs have been transformed from an accurate representation of the world to a *misrepresentation* and as such reporting on this now false-belief requires more than simply remembering the details of an unexpected train of events. According to these authors, young children fail at false-belief tasks not because they lack the *ability* to search for a record of this misrepresentation, but rather because their habitual search patterns overlook the false-belief because it has since been tagged as a misrepresentation, and so is deemed unworthy of serious consideration. Freeman and Lacohee's procedural way around this hypothesized limitation was to alert subjects to the representational status of beliefs by providing them with a retrieval cue that could itself be categorized as having a more legitimate representational status. More specifically, building on earlier work by Mitchell and Lacohee (1991), these investigators modified a standard unexpected contents, or "Smarties" task, by providing subjects with a retrieval cue meant to assist them in keeping track of their own original belief regarding the contents of the Smarties box -- a cue which they were asked to "post" in a mailbox. In line with their predictions, Freeman and Lacohee report that the most effective cues were ones that, in and of themselves, had some representational status. For example, posting a picture of Smarties as a way of helping subjects to keep clear traces of the fact that, at the outset, they had believed the Smarties box to contain such contents, effectively increased correct performance on test questions, whereas posting an actual sample of the Smarties as such did not. At least a part of what appears to be operating in Freeman and Lacohee's procedural modifications, then, is that subjects are being alerted to the relevance of their own representational states through the use of representational media, such as pictures as memory cues. This representational cueing hypothesis, while consistent with the general pattern of results these researchers obtained, nevertheless, requires some additional analysis. What is particularly germane to the claims put forward in this thesis is the fact that in the Freeman and Lacohee studies when subjects were simply handed the appropriate picture that corresponded with their current beliefs, without having

more actively selected it for use, the otherwise facilitating effects of posting these representational cues was diminished. Rather, the most effective experimental conditions in their studies were those in which subjects themselves were required to actively choose the picture to serve as a retrieval cue. Although the facilitating effects of having subjects actively choose such representational cues is the finding which most directly relates to the claims being made here, it is also important to note that such action was not on its own sufficient to facilitate performance. When subjects actively chose from an array of irrelevant pictures or when they were allowed to choose a real sample of what they supposed were the contents of the box, no comparable facilitating effects were found.

The proposed relevance of Freeman and Lacohee's findings is that in their studies, like those reported in this thesis, subjects failed to benefit from being passively assigned a retrieval cue or made to passively observe a deceptive strategy. By contrast, actively choosing one's own retrieval cues, or actively planning a deceptive strategy had the same facilitating effect. In both cases these procedures served to move the relevant mental states from the periphery of concern to the center stage of these subjects' awareness. With particular reference to the studies reported here it is hypothesized that the process of keeping track of the changing belief state of the protagonist no longer only comes to assume relevance after-the-fact. Instead involving subjects in the more constructive process of creating a false-belief, as was the case in the *active* and *planning* conditions in this thesis, likely facilitates their later reporting on such misrepresentations because, in planning their deceits subjects are faced with questions about their opponent's belief states from the beginning.

The poor performance of subjects in the *observer only* condition offers additional insight into how this proposed relevance hypothesis might be operating. In this condition, subjects were explicitly told that the experimenter intended to deceive the protagonist. By providing this deceptive context the *observer only* condition, in contrast to the standard unexpected transfer task, did in fact provide cues from the beginning of the procedure that the important feature to keep track of was the protagonist's beliefs. In spite of these cues subjects fared no better than 3-year-olds typically do when tested using more standard measures of false-belief understanding. Thus simply

pointing out to subjects at the beginning of the procedure that beliefs might be an important feature to pay attention to was insufficient in the studies reported here to access their best performance. It was only when subjects themselves were required to work to construct their own understanding of what was relevant that any facilitating effects were found.

A further parallel to the results reported here can be found in the recent research carried out by Lewis and his colleagues (Lewis, in press) on the use of narrative construction as a means of better accessing early false-belief understanding. Like Freeman and Lacohee (1993), Lewis also proposed that the difficulties young children have with standard false-belief tasks were not ones of straightforward memory deficits. He points out that subjects correctly remember all the individual details of the story, but proposed that the particular stumbling block for young 3-year-olds is that they don't judge specific incidents as worthy of being integrated into the narrative account they are attempting to construct. In other words, in line with the claims made in this thesis, Lewis claims that under the circumstances afforded by standard false-belief tasks, young subjects don't deem certain aspects of the unfolding of an event sequence, most notably the story character's mental state, to be especially relevant. In a series of studies in which subjects were told stories, using a book format, Lewis found that when subjects themselves were involved in the active retelling of the story even young 3-year-olds correctly answered subsequent false-belief test questions. In constructing their own narrative about the unexpected turn of events, Lewis argues subjects had to focus on the relevant details as a whole, including the story character's changing belief states. Thus, like subjects who constructed a strategic plan of action to deceive another person, the subjects in Lewis' experiments were required to themselves determine the relevance of a story character's representational state from the beginning of this retelling.

Having completed an examination of potential alternate explanations for the pattern of results obtained in this thesis, and having made a case for why being involved in planning a deception was so clearly effective in facilitating subjects' performance on otherwise standard measures of false-belief understanding, the task that remains is to outline some of the limitations of the current research findings, and to suggest future directions for subsequent research efforts.

Limitations and Suggestions for Future Research

Despite the clear grasp of the possibility of false beliefs evidenced by even the youngest subjects of this study sequence, and the clear indication that this early competence was promoted by their own planning efforts, more work is obviously need to better map out precisely which features of such planning activities actually work to promote such improved performance. The most plausible explanation, and the one suggested in this thesis, is that the performance of the young subjects of these studies resulted from their having to take account of the relevance of the other person's current and future belief states in order to successfully perpetrate a deception. The findings from Study 4 clearly demonstrate that engaging 3-year-old subjects in planning with specific reference to the goal of deception is sufficient, in and of itself, to facilitate performance on otherwise standard false-belief tasks. Among the things that remain unsettled, however, at the completion of this series of studies is whether subjects need to be involved in planning some act of deception or whether planning of other sorts will do just as well. A clue that such planning is most effective when in direct reference to another's beliefs comes from the postbox studies reported by Freeman and Lacohee (1993). As was pointed out earlier, the subjects in the Freeman and Lacohee studies were at their best when they themselves actively chose the picture that corresponded with their belief as to the contents of a familiar marked box. This active selection, however, was helpful only when the choices available contained a picture *relevant* to that belief. No comparable facilitating effect was found when subjects chose from an array of irrelevant pictures.

A next logical step in the long range program of research begun in this dissertation would be to carry out a series of studies in which planning still figured prominently, but in which the purpose is no longer to intentionally deceive another person. For example, subjects might be asked to find an alternate location for the treats, not because of some intent to mislead, but because, for some altogether more mundane reason, the original container is no longer suitable. Such a procedure would help to isolate the relative contributions of planning per se as compared to

planning with specific reference to manipulating representations. Although the results from the *observer only* conditions included in Studies 3 and 4 rule out the possibility that providing a deceptive context is sufficient in and of itself to increase the performance levels of young 3-year-olds when asked about the resulting false-beliefs, it might still be the case that the constructing of a plan of action only works when such a plan not only is in explicit reference to another's mental state but also when it has the purpose of altering that mental state in misleading ways.

As discussed in the Introduction, for both methodological and conceptual reasons, the literature on young children's belief understanding has taken as its central focus the question of when it is children first come to understand the possibility of *false* beliefs or *misrepresentations* (for notable exceptions see Wellman, 1990; and Wellman & Bartsch, 1988). Given these constraints, it is difficult to imagine a procedure that provides subjects with an opportunity to construct a plan with reference to altering another's beliefs without somehow including a deceptive goal. There is one existing experimental paradigm, however, that would lend itself to the sorts of procedural modifications necessary to determine if subjects' active role in planning would work to help them report on another person's representations even in the absence of any deceptive goals. Research carried out by DeLoache and her colleagues (e.g., DeLoache, 1989) has demonstrated that by the age of three, children readily make use of information obtained from a scale model to guide their search for an object in a larger room. Based on these findings, DeLoache concluded that 3-year-olds understand the representational relation between the scale model and the larger room. While DeLoache's work on scale models examined whether subjects make use of the information from the scale model in order to guide their own search for the hidden object, what has yet to be tested is whether these same subjects would also understand that another person could also be brought to have particular representations or beliefs as to the location of the hidden object as a result of information available through the scale model. A potential way in which planning with reference to another person's representations could be brought into play in such a procedure is by setting up a testing situation in which subjects were themselves required to place the small object in the scale model of the real room with the stated purpose of either *informing* or *misleading* another

person about the location of the larger object in the larger room. In this procedure, a protagonist would initially be in ignorance, or lacking in any firm belief, about the location of the object. Subjects would be given responsibility for planning where to place the small object so that the protagonist would be led to hold either a true or a false belief about the location of the large object. In addition to these deceptive and non-deceptive planning conditions a third and fourth condition in which subjects simply watch as the experimenter carried out such operations meant to either inform or disinform another person could serve as control conditions, comparable to the *observer only* conditions included in this thesis. In all such conditions, subjects might be asked to comment upon another person's beliefs about where in the real room the corresponding object was located. This suggested program of research would thus take up the further question triggered by the findings of this thesis concerning whether planning need necessarily have some Machiavellian objective in order to create a measurement context that best accesses young subjects' understanding of the representational nature of beliefs.

A further question that would also seem to deserve future exploration is whether embedding false-belief testing procedures in a real social context as was done in the present study sequences, provides some additional benefit for subjects over and above those gains brought about through their engagement in framing strategic plans of action. The claim made in this thesis is that simply taking steps to add new notes of social realism to the testing situation by substituting real people (including the subjects) for the doll figures of most studies is insufficient to account for the good performance observed for those subjects who took part in the *active* and *planning only* conditions included here -- a claim which is supported by the poor showing of those subjects assigned to the equally "social" *observer only* conditions. The methodological tactic at work in the series of studies that forms this thesis was to control for any potential effects due to social factors by *maximizing* such social involvement across all experimental conditions. An alternative approach would be one of *minimizing* any social involvement across all conditions while at the same time working to systematically isolate the effects of planning as compared to observing a deception. One could, for example, present subjects with a deceptive unexpected transfer task in a "choose-your-

own-ending" type storybook format that is currently popular with many children. In this way subjects could be set the task of planning the story events in such a way that the story character ends up having been misled into holding a false-belief. This new procedure would provide a means of further isolating the role of planning from other competing explanations for 3-year-olds' good performance on the *planning only* conditions reported here.

Despite the kinds of unfinished business listed out above, the present series of studies, nevertheless, constitutes one of the first systematic attempts to examine the effects of strategic planning on 3-year-old subjects' abilities to demonstrate their conceptual grasp of the possibility of false-beliefs. In addition to corroborating evidence from other sources that portrays such young children as having much greater facility with understanding their own and others' minds than has sometimes been thought to be the case, the research reported here goes on to demonstrate the importance of providing subjects with a central and active role in assessing the relevance of another's representational state in arriving at especially apt testing procedures. As discussed above, ultimately a very long series of studies is necessary if one were to entirely run to earth how such active involvement in planning ends up working its effects on young children's ability to display their understanding of mental states. Over and above those positive efforts undertaken here to better define the role of strategic planning in facilitating young children's progress toward some more fully fledged or adult-like theory of mind, it is clear that there remain numerous and interesting unanswered questions concerning the likely additional steps that children pass through in the process of developing a fully-fledged adult theory of mind.

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TABLES

Table 1

Active Unexpected Transfer Task

Percent correct responses to false-belief test questions

Age = 3 years

Question type	
Look	87.5%
Think	70 %

(*N*=40)

Table 2

Active Unexpected Contents Task

Percent correct responses to false-belief test questions

Age = 3 years

Question type	
Look	81%
Think (location)	70 %
Think (contents)	81%

(*N*=27)

Table 3

Observer Unexpected Transfer Task
and
Standard Unexpected Transfer Task

Percent correct responses to false-belief test questions

Age = 3 years

Question type	Observer Task	Standard Task
Look	40%	40%
Think	35%	40%

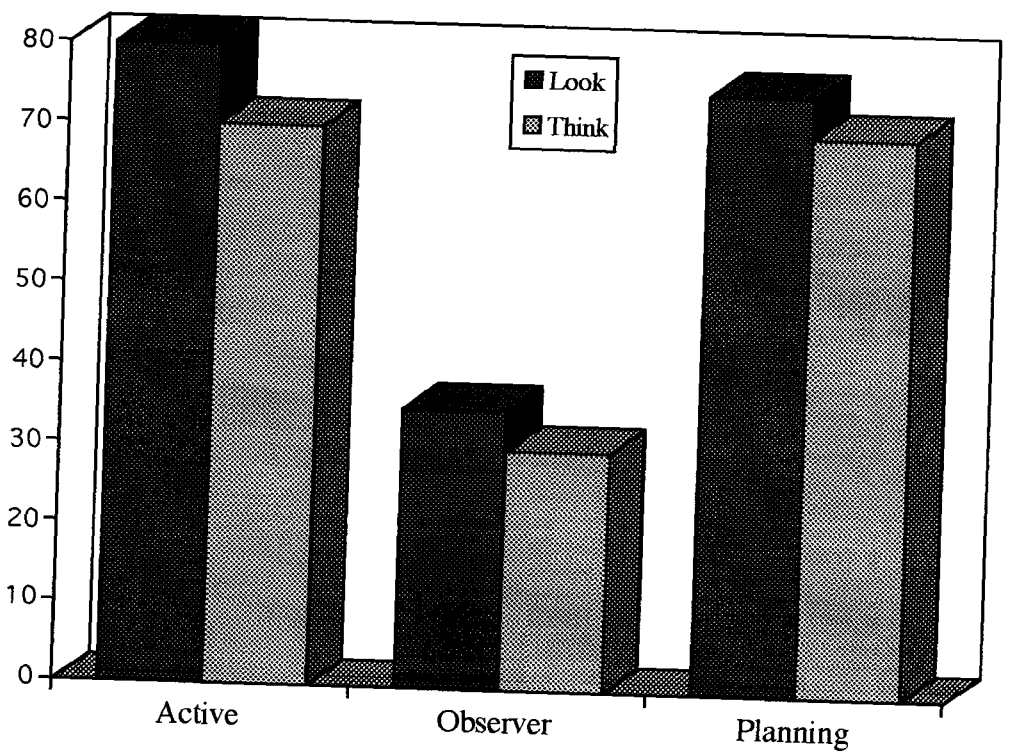
(*N*=20)

FIGURES

FIGURE 1

Percent correct responses to belief question
across 3 conditions

Age = 3 years



(*N*=60)

APPENDICES

APPENDIX A

Action as purposeful behavior

Although not all of those who have undertaken to make a study of action, precisely agree on what constitutes such actions, there is broad agreement that not all "movement" or even all "behavior" warrants being classified as action. In his book, *Explaining behaviour. Reasons in a world of causes*, Dretske (1988) outlines a hierarchical classification system moving from the very general category of *movement* to the specific subcategories of, first *behavior* and, finally *action.* According to Dretske "movement" encompasses all motion that an organism partakes in. The difference between mere movement and behaviour lies in the origin of the *cause* of the motion. If the locus of the cause is something *external* to the organism the resulting movement is not behavior. If, however the movement results from causes *internal* to the organism such movement merits the classification of behavior.

If, for example, *I* take our family's pet iguana and move his tail in such a way that it strikes my arm, the cause for the movement is external to the iguana. By contrast, if when I pick our iguana up he lashes at me with his tail the cause of the movement is internal to the iguana and, at the very least, warrants being called behavior.

As Dretske (1988) points out, this is a very crude distinction and greater refinements are necessary. One such refinement is evident in the distinction Dretske and others make between *behaviour* and *action.* Action, according to Dretske, is a special kind of behaviour wherein not only is the cause of movement internal to the organism, but the behaviour is *voluntary* or deliberate. By this definition, movements which are internally produced but lack voluntary control (for example, sneezing, blinking, breathing, or coughing) are relegated the lesser status of behaviour. In contrast, if one *chooses* to cough in order to call attention to oneself, or voluntarily holds their breath, then these behaviours would be elevated to the status of *action.*

Although this distinction between action and behaviour is a useful starting point, for my purposes it is still too crude a portrayal of the role of intention in action. Implicit in Dretske's classification is the notion that all action at some level involves intention. Simply intending to cause movement, however, remains too broad a definition for the sort of action I wish to focus on as playing a central role in the carrying out of deception. In his account of intentionality, Searle (1983) addresses just this point. Searle points out that even though all actions in some sense include an intentional component there are many actions that can be considered *unintentional action* in the more specific sense that the action was not intended to produce the resultant outcome. For example, if I raise my arm to reach an object but instead my arm connects with your chin, although I *intended* to raise my arm (voluntary behaviour) I did *not intend* for my arm to connect with your chin (unintended consequence). So although the action of raising my arm involved some intention it does not involve the *prior* intention specific to bringing about the outcome of striking your chin.

What is of interest for the purposes of this program of research is action which is both "intentional" and "intended", that is, action which is undertaken to fulfill a specific goal. This notion of goal-directed or reflective action is central to European based Action Theory (e.g., Chapman & Skinner, 1985; Frese & Sabini, 1985) and although there exists little concert of opinion across different proponents of this theory, the basic tenets of Action theory may provide a useful framework for discussing how it is subjects are afforded an "active" role in the procedures used in the studies presented here. In Action theory people are viewed as acting (and interacting) in ways that are reflective and intentional.

A further detailing of what is meant by an Action theory framework may provide some clarification as to how the notion of action is employed in the study sequence in this thesis. In "Action theory", although the general components of intention and action are commonly seen to be intimately intertwined, there remains a gap between the *intention* and the *act* which must be further closed by a more specific *plan of action* (Chapman & Skinner, 1985; Frese & Sabini, 1985; Kreitler & Kreitler, 1987; Scholnick & Friedman, 1987). According to Scholnick and Friedman, the formulation of such a plan of action would, at a minimum, require that the actor be able to

represent both what the problem is and what the goal is. Further, in order to decide on the plan the actor must have certain beliefs about the chain of causal events which might lead to a successful attainment of the goal and about their own ability to carry out the plan.

This framework for goal-directed action can readily be applied to the hide-and-seek procedure developed by my colleagues and myself. In order to be successful in their hiding endeavors, the subjects of these earlier studies were required to devise a plan of action, including, representing the goal (to hide the treasure so their opponent would not find it), defining the problem (if hidden in the usual way the opponent would easily find the treasure), and choosing the means to achieve this end (taking steps meant to lead their opponent astray). The section which follows in the main text of this thesis is meant to build upon this earlier work by offering reasons as to why engaging subjects in goal-directed actions might provide us with a clearer understanding of the competence hypothesized to already characterize very young children's understanding of the mental lives of others.

APPENDIX B

STUDY 1

Protocol for Unexpected Transfer Task -- Active Condition

Participants -- two experimenters and the subject

The prime experimenter (E1) remains in the room with the child throughout the procedure.

The second experimenter acts as the protagonist (E2) and the target for any deceptive action.

PROTOCOL

The child is told that he/she will be playing a pretend game with the experimenters.

E1 *"We're going to play a game today. We're going to play a kitchen game."*

The participants are all involved in "setting the stage" for the play --A blanket is brought out and the prime experimenter (E1) suggests using the blanket for a pretend kitchen.

E1 *"Let's use this blanket for our kitchen. Help me set this blanket out. Here, E2 , you take one corner and (subject's name) you take this corner and let's spread it out flat."*

The experimenters and the subject lay out the blanket to form the boundaries of the pretend kitchen.

E1 *"Good. Now let's all sit down in the kitchen."*

All three participants enter the kitchen and sit down.

E1 *"Now we need some things for our kitchen. Here are some containers we can use for the cupboards"*

The experimenter (E1) produces 2 opaque containers to be used for kitchen cupboards. E1 hands one container to the child saying:

"Here (subject's name) will you please put this blue (or red) container somewhere in the kitchen."

The subject places the first container in a location of his or her own choice.

E1 *"Good. Now would you (subject) put the red (or blue) container in a different spot in the kitchen."*

The subject places the second container in a second chosen location.

E1 *"Good. Now we need something else for our kitchen. We need some lunch. I have something here we can have for our lunch."*

E1 then brings out a transparent (glass) container of fruitbears and shows the child clearly what is in the jar, offering one to the child at this point. Then E1 puts the lid back on the jar and puts the jar of fruit bears down on the blanket.

E1 *"We're going to have some fruitbears for our lunch in a minute but first we need some plates.* (brings plates out) - *"Here are some plates."* (Looks at plates) -- *Uh oh, they're dirty. We can't eat on these dirty plates."*

E2 *"Oh, I'll go and wash them* (takes plates from E1. *But before I go I'm going to put the fruitbears in the red (blue) container."* (E2 chooses container, places fruitbears in it and puts the lid on).

E2 leaves and closes door

E1 (after making sure E2 is not in earshot), *"I want you to help me do something."* (E1 takes the fruitbears out of the original container and places them on the blanket.) *"Let's hide the fruit bears from E2 ! Here you hide them!"*

E1 hands the jar of fruitbears to the subject.

If the subject attempts to hide the fruitbears in any location other than the second container, E1 explains that they have to stay "in the kitchen". (Although other hiding spots would be entirely appropriate this was done to ensure that the procedure closely matched the standard unexpected transfer task in providing only two possible locations for the object -- its original location and a single second location to which it is moved.)

After the subject has hidden the fruitbears the following control questions are asked:

E1 *"So, now you've put the fruitbears in the red (or blue) cupboard over here. Did E2 see you move them?"*

"Where did E2 put them before she left the room?"

These control questions are followed by the test questions:

E1 *"So now, E2 will want to get the fruitbears when she comes back."*

"When E2 comes back where will she first look for the fruitbears?"

"When E2 comes back where will she think the fruitbears are?"

The order of these test questions was alternated across subjects and within age groups.

A final control question is then asked to ensure the subject remembers the actual current location of the fruitbears.

E1 *"Where are the fruitbears really?"*

After the subject responds to the test questions E1 checks to see if E2 is coming yet -- this is E2's cue to return. E2 enters the room carrying the clean plates and goes to get the fruitbears from the original container. E2 reacts with surprise to the empty container. After the fruitbears are brought out from their hiding location the participants are invited to have one on their plates. And the testing session is ended.

APPENDIX C

STUDY 2

Protocol for Unexpected Contents Task -- Active Condition

Participants -- two experimenters and the subject

The prime experimenter (E1) remains in the room with the child throughout the procedure.

The second experimenter (E2) acts as the protagonist and the target for any deceptive action.

PROTOCOL

The child is told that he/she will be playing a pretend game with the experimenters.

E1 *"We're going to play a game today. We're going to play a kitchen game."*

The participants are all involved in "setting the stage" for the play --A blanket is brought out and the prime experimenter (E1) suggests using the blanket for a pretend kitchen.

E1 *"Let's use this blanket for our kitchen. Help me set this blanket out. Here, E2 , you take one corner and (subject's name) you take this corner and let's spread it out flat."*

The experimenters and the subject lay out the blanket to form the boundaries of the pretend kitchen.

E1 *"Good. Now let's all sit down in the kitchen."*

All three participants enter the kitchen and sit down.

E1 *"Now we need some things for our kitchen. We need a table for our kitchen. I have something we can use as a table."*

E1 brings out a closed cardboard box (a box which previously held a pair of children's rubber boots but which has no identifying features on the outside).

E1 *"Here, let's use this box for our table."* (Places box on blanket).

E1 *"Now we need something else for our kitchen. We need some lunch. I have something here we can have for our lunch."*

E1 picks up a box of "teddy grahams" and shows it to the child and E2.

E1 *"Look at this box. What do you think is in here?"* (If child hesitates then E1 prompts with -- *"look at the picture on the box...what do you think that's a picture of... so what do you think is inside?"*)

Child responds -- if correct E2 agrees, if not correct E2 says *"I think it's teddy grahams"*.

E1 *"Let's look and see."* (assists child in opening the box).

E1 *"Yes you were right, there are teddy grahams in here."* (allows child to have one at this point if he or she wishes). *"Let's put the lid back on again and save them for later."*

E1 *"We're going to have some teddy grahams for our lunch in a minute but first we need some plates."* (brings plates out) -*"Here are some plates."* (Looks at plates) -- *Uh oh, they're dirty. We can't eat on these dirty plates."*

E2 *"Oh, I'll go and wash them."* (takes plates from E1).

E2 leaves and closes door.

E1 (after making sure E2 is not in earshot), *"I want you to help me do something"*. *"While E2 is gone, let's take out the teddy grahams and hide them from her (him)."*

E1 takes the teddy grahams out (they are in a bag inside the box) and places them on the floor.

E1 *"Where shall we hide them? You find a spot to put them!"*

E1 gives the teddy grahams to the subject to hide.

Subject hides teddy grahams. (Subject is free to hide teddy grahams any location in the room). If the subject does not spontaneously choose a hiding location E1 prompts by saying:

E1 *"Think hard, where could you hide them?"*
"You can hide them anywhere you like."

After the subject has hidden the teddy grahams E1 asks the following control question:

E1 *"Did E2 see us take the teddy grahams out of the box and hide them?"*

Followed by the first 2 test questions:

E1 *"Now when E2 comes back she/he is going to want to get the teddy grahams."*

"When E2 comes back where will she/he go to get the teddy grahams?"

"When E2 comes back where will she/he think the teddy grahams are before she looks in the box?"

The order of these test questions was alternated across subjects and were followed by the following control question:

"Where are the teddy grahams now?"

The next stage in the procedure involves the subject in the substitution of an unexpected object for the original teddy grahams.

E1 *"Now before E2 comes back let's play a really funny trick on her/him. We've already hidden the teddy grahams -- let's put something else in the box to really fool her/him."*

E1 opens up the cardboard box that has served as a table, revealing the inside to contain several small items. The objects include: a slice of plastic pizza; a small rubber lizard, spider and snake; a set of plastic keys; a small plastic giraffe; and a plain wooden block.

E1 *"Look what I have in here! -- you choose something to put in the teddy graham box to trick E2 ."*

Subject chooses from the array of objects and puts one in the box.

E1 "*Good! now let's close it all up again.*" (if child has not spontaneously done so).

E1 then asks the following control question:

"Did E2 see us put _____ in the teddy graham box?"

Followed by the test question:

"Now when E2 comes back, before we let her/him open the box, what will she/he think is in there?"

And the final control question:

"What is really in the box?"

After the subject responds to the test questions E1 checks to see if E2 is coming yet -- this is E2 's cue to return. E2 enters the room carrying the clean plates and goes to get the teddy grahams from the teddy graham box only to discover the replacement object. E2 reacts with surprise and laughter. After the teddy grahams are brought out from their hiding location the participants are invited to have one on their plates. And the testing session is ended.

APPENDIX D

STUDY 3

Protocol for Unexpected Transfer Task -- Observer Condition

Participants -- two experimenters and the subject

The prime experimenter (E1) remains in the room with the child throughout the procedure and carries out the deception on the protagonist.

The second experimenter acts as the protagonist (E2) and the target for the experimenter's deceptive action.

PROTOCOL

The child is told that he/she will playing a pretend game with the experimenters.

E1 *"We're going to play a game today. We're going to play a kitchen game. At first you get to watch while E2 and I set everything up."*

The 2 experimenters set up the kitchen while the subject watches.
A blanket is brought out and the prime experimenter (E1) suggests using the blanket for a pretend kitchen.

E1 *"Let's use this blanket for our kitchen. (To E2) Help me set this blanket out. Here, E2 , you take that end and let's spread it out flat."*

The experimenters lay out the blanket to form the boundaries of the pretend kitchen.

E1 *"Good. Now let's all sit down in the kitchen."*

All three participants enter the kitchen and sit down.

E1 *"Now we need some things for our kitchen. Here are some containers we can use for the cupboards."*

The experimenter produces 2 opaque containers to be used for kitchen cupboards.

E1 *"I'm going to put the red (blue) one over here (places 1st container). Now I'll put the blue (red) one over here. There we have cupboards for our kitchen."*

E1 *"Now we need something else for our kitchen. We need some lunch. I have something here we can have for our lunch."*

E1 then brings out a transparent (glass) container of fruitbears and shows the child clearly what is in the jar, offering one to the child at this point. Then E1 puts the lid back on the jar and puts the jar of fruitbears down on the blanket.

E1 *"We're going to have some fruitbears for our lunch in a minute but first we need some plates. (brings plates out) -* "*Here are some plates.*" (Looks at plates) -- *Uh oh, they're dirty. We can't eat on these dirty plates."*

E2 *"Oh, I'll go and wash them* (takes plates from E1). *But before I go I'm going to put the fruit bears in the blue (red) container."* (chooses container, places fruitbears in it and puts the lid on).

E2 leaves and closes door.

E1 (after making sure E2 is not in earshot), *"I'm going to do something."* (E1 takes the fruit bears out of the original container and places them on the blanket.) *"I'm going to hide the fruit bears from E2 !"*

"I'm going to hide the fruitbears in the _____ cupboard." (container other than original location)

After experimenter "hides" fruitbears in new location the following control questions are asked:

E1 *"So, now I've put the fruitbears in the red (or blue) cupboard over here. Did E2 see me move them?"*

"Where did E2 put the fruitbears before she left the room?"

This is followed by the test questions:

E1 *"So now, E2 will want to get the fruitbears when he/she comes back."*

"When E2 comes back where will she first look for the fruitbears?"

"When E2 comes back where will she think the fruitbears are before she opens the container?"

The order of these test questions was alternated across subjects and within age groups.

A final control question is then asked to ensure the subject remembers the actual current location of the fruitbears.

E1 *"Where are the fruitbears really?"*

After subject responds to the test questions E1 checks to see if E2 is coming yet -- this is E2 's cue to return. E2 enters the room carrying the clean plates and goes to get the fruitbears from the original container. E2 reacts with surprise to the empty container. After the fruitbears are brought out from their hiding location the participants are invited to have one on their plates. And the testing session is ended.

APPENDIX E

STUDY 3

Protocol for Unexpected Transfer Task -- Standard Condition

In Study 3 the standard unexpected transfer task was always administered following the observer condition.

As a transition between the two procedures the materials that had served as props in the kitchen set up were packed away by the experimenters and subject.

The prime experimenter then invited the subject to watch a short puppet show.

The prime experimenter operated the puppets and asked the control and test questions while the second experimenter recorded the subject's responses.

The two puppets were introduced by the experimenter:

> *This is Katie and this is Sam. They are friends at school and like to play together. Right now they are playing with a toy car.*
>
> *They take turns playing with this car for a little while.*

The puppets are shown to play with the car.

> *They play with the car together.*
> *Katie takes a turn and then Sam takes a turn.*
> *But soon they hear their teacher calling them.*
>
> *"Katie, Sam. Time to clean up and come for snack."*
>
> *So Katie and Sam put the toy car away so that they can come back and play with it after snack. They look for a place to put the car.*
>
> *Sometimes they put it in the red container. Sometimes they put it in the blue one.*
> *Right now they put it in the red one.*

The puppets are then shown together putting the toy car in the red container (choice of container was alternated across subjects).

> *Together Katie and Sam put the car in the red container .*
>
> *Then they leave for snack.*

The puppets are made to leave the playroom.

> *Katie finishes her snack first and comes back to play with the toy car.*

Katie is shown returning to the playroom.

She goes to the red container and takes out the toy car and starts to play with it again. But then she hears her teacher calling again,

"Katie, you didn't clean up your snack things. Please come back."

Katie then puts the toy car away again but this time she puts it in the blue container.
Sam doesn't see her put it there.
The Katie goes out of the playroom again.

At this point the experimenter asks the following control questions:

Did Sam see Katie move the toy car to the blue container?

Where did Sam and Katie first put the toy car?

This is followed by the test questions:

Now Sam is about to come back.
And he wants to play with the toy car again.

When Sam comes back where will he first look for the toy car?

When Sam comes back where will he think the toy car is before he opens the container?

The order of these test questions was alternated across subjects and within age groups.

A final control question is then asked to ensure the subject remembers the actual current location of the toy car:

Where is the toy car really?

APPENDIX F

Intentional action and performance on false-belief tasks

In order to help flesh out in greater detail just what it is about the goal-directed action that allowed subjects in Study 1 and Study 2 to better display their false-belief understanding I wish to return to a useful heuristic provided by the Action Theory perspective. In their discussion of cognitive performance as an intentional action (i.e., as a goal-directed action) Chapman and Skinner (1985) propose a model of five "phases" said to compose the components of goal-directed action:

> "(1) goal setting; (2) plan generation and selection; (3) operation or implementation of the plan; (4) evaluation of outcomes relative to the goal, and (5) regulation of subsequent performance to close the gap between goal and outcome." (pp. 131-132).

This model provides a useful framework to map out the relative responsibility that the subject and experimenter have for fulfilling the intentional goal of deceiving a protagonist in this series of studies. As Chapman and Skinner point out, these are not entirely distinct phases since they frequently overlap to some extent. Nonetheless, they can be applied in this case to assess the relative merits of participation in each phase of the carrying out of the deception.

Table E-1 portrays how the studies already presented here as well as Wimmer and Perner's original procedure (1983) explicitly engage the subject at each of these five levels of deceptive action (see Table F-1). As can be seen in this table, the

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Table F-1

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initial phase of *goal setting* has always been carried out by the experimenter, who sets the task of hiding the object from the protagonist. Similarly, phase 4, *evaluation of outcome*, was initiated by the experimenter in a somewhat indirect way through the use of the test question, "Where will the protagonist look?" (although certainly it is likely that children were carrying out their own internal

evaluation). In addition, phase 5, *regulation of subsequent performance* is not included in this table as all children were successful at hiding the object in Study 1 and Study 2 which resulted in no need for any adjustment of strategies.

Whereas subjects' participation at each of the above mentioned phases of action remained relatively constant across studies, the two remaining phases *plan generation and selection* and *operation or implementation of plan* were manipulated as to the degree to which they fell under subjects' control. Specifically, for the action-based procedures in Studies 1 and 2, both of these phases were designed to be under the subject's control. Subjects were required to both generate the plan of action as well as to physically execute that plan. In contrast, in control Study 3, where the subject is simply an observer rather than actor, responsibility for generating and executing the plan, as with the original Wimmer and Perner procedure, once again falls solely upon the shoulders of the experimenter.

By using this model to break down the action sequence we can see that the action-based procedures used in Studies 1 and 2 differed from the procedures in Study 3 across more than one phase. What this means is that it is not possible, based solely on the results of Studies 1 and 2, to determine the relative merits of requiring subjects to formulate a plan as compared to simply physically carrying out a plan already formulated by someone else. What is not clear is whether subjects need to be participants at both phases or whether having the role of *either* planning the deception or physically carrying out the actions which lead to the deception would be sufficient on its own to result in improvement on standard false-belief test questions.

If we take into account just the *plan generation* and the *execution* phases of intentional action then we can construct a new table which shows four potential procedural conditions which could result from systematically combining these phases (see Table F-2).

Table F-2

As discussed above, conditions A and B are already met by the first three studies of this report, wherein either the subject or experimenter has control over *both* the planning and implementation phase. Conditions C and D could be met only if a procedure could be constructed which allowed the subject control over one, but not the other of these phases.

Turning first to condition C, what becomes apparent is that while we could in principle construct a hypothetical procedure in which a subject could be induced to physically execute an experimenter's plan without at the same time concerning themselves with the construction of the plan, in a concrete testing situation involving deception such a procedure becomes next to impossible to implement. Across all studies the stated goal is to hide the object from the second experimenter, with the end result that the experimenter is deceived as to its whereabouts. To meet condition C, after stating the initial intention to hide the object, the experimenter would need to bypass the plan generation phase by somehow inducing the subject to mindlessly place the object in a location of the experimenter's choice. Any conspiratorial steps taken on the part of the subject to do more than simply comply with the first experimenter's request would need to count, however, as a joint venture involving shared, or at least parallel, intentions. Although in the specific case of deception we can attempt to reduce the explicit opportunity that the subject might have for generating such plans of action we could not offer any iron clad guarantees that such subjects were not themselves entering into a co-conspiracy and spontaneously formulating a plan in line with the experimenter's. Without such guarantees we would be unable to determine how important it is for subjects to invent their own solutions to the task of intentionally instilling a false belief in another. In a word, control condition "C", as outlined above, is something of a practical impossibility.

Given the practical difficulties associated with teasing apart participation in the formulation and implementation phases of the plan of action within a procedure in which deception is the stated goal, it would appear that any systematic examination of the potential contributions of involvement in these two phases would need to be carried on outside the realm of deception. Although removing deception as the goal would provide a means of ensuing the subjects were not engaging

in their own goal-directed planning such a modification would unfortunately also introduce an additional confound in the existing series of experiments. The removal of the goal of deception in such a condition would prevent any direct comparisons with the other conditions in which the prime motivation for transferring an object is to manipulate the beliefs of another person.

In contrast, a relatively easy alternative way to explore whether the *physical activity* component plays an integral role in children's performance on the deception tasks is to restrict subjects from such activity during the procedure. Returning to table F-2, notice that fulfilling condition D is one way of assessing this role. What is required to meet this condition is that the subject be engaged in the *generation* of a plan but restricted from *physically executing* the plan themselves. This can be accomplished within the same deceptive context that has already been portrayed in Study 1. In this instance, however, rather than the subject being required to both decide on an appropriate hiding location and to carry out the actual hiding, the experimenter simply asks the subject to decide where the object is to be hidden and then hides it in that location herself.

This manipulation provides a test of whether engaging in the physical activity of executing a plan is necessary in order to demonstrate improved performance over standard measures of false-belief understanding or whether being made responsible for the planning plays the only or prime role.

APPENDIX G

STUDY 4

Protocol for Unexpected Transfer Task -- Active, Observer and Planning Conditions

Participants -- two experimenters and the subject

The prime experimenter (E1) remains in the room with the child throughout the procedure.

The second experimenter acts as the protagonist (E2) and the target for any deceptive action.

PROTOCOL

The procedure is identical across the 3 conditions up until the point of the protagonist's departure.

The child is told that he/she will be playing a pretend game with the experimenters.

E1 *"We're going to play a game today. We're going to play a kitchen game."*

The participants are all involved in "setting the stage" for the play --A blanket is brought out and the prime experimenter (E1) suggests using the blanket for a pretend kitchen.

E1 *"Let's use this blanket for our kitchen. Help me set this blanket out. Here, E2 , you take one corner and (subject's name) you take this corner and let's spread it out flat."*

The experimenters and the subject lay out the blanket to form the boundaries of the pretend kitchen.

E1 *"Good. Now let's all sit down in the kitchen."*

All three participants enter the kitchen and sit down.

E1 *"Now we need some things for our kitchen. Here are some containers we can use for the cupboards"*

The experimenter produces 2 opaque containers to be used for kitchen cupboards. The experimenter hands one container to the child saying:

"Here (subject's name) will you please put this blue (or red) container somewhere in the kitchen."

The subject places the first container in a location of his or her own choice.

E1 *"Good. Now would you (subject) put the red (or blue) container in a different spot in the kitchen."*

The subject places the second container in a second chosen location.

E1 *"Good. Now we need something else for our kitchen. We need some lunch. I have something here we can have for our lunch."*

The experimenter then brings out a transparent (glass) container of fruitbears and shows the child clearly what is in the jar, offering one to the child at this point. Then E1 puts the lid back on the jar and puts the jar of fruit bears down on the blanket.

E1 *"We're going to have some fruitbears for our lunch in a minute but first we need some plates."* (brings plates out) - *"Here are some plates."* (Looks at plates) -- *Uh oh, they're dirty. We can't eat on these dirty plates."*

E2 *"Oh, I'll go and wash them* (takes plates from E1). *But before I go I'm going to put the fruitbears in the red (blue) container."* (E2 chooses container, places fruitbears in it and puts the lid on).

E2 leaves and closes door

After E2 leaves the room , one of three procedural scripts is followed depending on which condition the subject has been randomly assigned to.

ACTIVE CONDITION

E1 (after making sure E2 is not in earshot), *"I want you to help me do something."* (E1 takes the fruitbears out of the original container and places them on the blanket.) *"Let's hide the fruitbears from E2 ! Here you hide them!"*

Then E1 hands the jar of fruitbears to the subject.

If the subject attempts to hide the fruitbears in any location other than the second container, E1 explains that they have to stay "in the kitchen".

After the subject has hidden the fruitbears the following control questions are asked:

E1 *"So, now you've put the fruitbears in the red (or blue) cupboard over here. Did E2 see you move them?".*

"Where did E2 put the fruitbears before she left the room?"

OBSERVER CONDITION

E1 (after making sure E2 is not in earshot), *"I'm going to do something."* (E1 takes the fruitbears out of the original container and places them on the blanket). *"I'm going to hide the fruitbears from E2 !"*

"I'm going to hide the fruitbears in the _____ cupboard." (cupboard other than original container)

After the experimenter "hides" the fruitbears in the new location the following control questions are asked:

E1 *"So, now I've put the fruit bears in the red (or blue) cupboard over here. Did E2 see me move them?"*

"Where did E2 put them before she left the room?".

PLANNING CONDITION

E1 (after making sure E2 is not in earshot), *"I want you to help me do something."* (E1 takes the fruitbears out of the original container and places them on the blanket.) *"Let's hide the fruit bears from E2 ! "*

Then E1 picks up the jar of fruitbears but rather than giving them to the subject she holds onto them saying:

E1 *"Where shall we hide them?"*
"You choose a spot."

If the subject chooses any location other than the second container, E1 explains that they have to stay "in the kitchen".

After the subject has chosen a hiding location for the fruitbears E1 places them in the new location and asks the following control questions:

E1 *"So, now we've put the fruit bears in the red (or blue) cupboard over here. Did E2 see us move them?"*

"Where did E2 put them before she left the room?"

ALL CONDITIONS

After the fruitbears have been hidden and the subject has answered the control questions the following test questions are asked:

E1 *"So now, E2 will want to get the fruitbears when she comes back."*

"Where will E2 look for the fruitbears when she first comes in?"

"Where will E2 Think the fruitbears are when she first comes in?"

A final control question is then asked to ensure the subject remembers the actual current location of the fruitbears.

E1 *"Where are the fruitbears really?"*

After the subject responds to the test questions E1 checks to see if E2 is coming yet -- this is E2 's cue to return. E2 enters the room carrying the clean plates and goes to get the fruitbears from the original container. E2 reacts with surprise to the empty container. After the fruitbears are brought out from their hiding location the participants are invited to have one on their plates. And the testing session is ended.

TABLES FOR APPENDICES

TABLE F-1

Phases of Action by Locus of Responsibility
across Studies 1, 2 &3

Phase of Action	Responsibility for Action Phase	
	Studies 1 & 2	Study 3 & Standard Task
1. Goal Setting	experimenter	experimenter
2. Plan Generation	subject	experimenter
3. Operation	subject	experimenter
4. Evaluation	experimenter initiates	experimenter initiates

TABLE F-2

Plan Generation and Operation Phases

	Condition			
	A	B	C	D
Phase of Action	Active	Observer	*	Planning
Plan Generation	subject	experimenter	experimenter	subject
Operation	subject	experimenter	subject	experimenter