Constructing an understanding of mind with peers

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Abstract: Carpendale & Lewis (C&L) stress the importance of social interaction for social understanding, but focus on the adult-child relationship. In the present commentary, we discuss the development of social understanding within early peer relationships. We argue that peer interaction stretches the limits of early social understanding, thereby providing both unique challenges and unique opportunities for constructing an understanding of others' minds.

In their target article, Carpendale & Lewis (C&L) assert that children construct social understanding through interactions with others and that social knowledge is fundamentally based on action within these relationships. In particular, they emphasize that early triadic interaction facilitates the development of language and that conversation about the mental world, in turn, permits children to perceive, understand, and reflect on their own and others' mental and emotional states. We share the authors' view that social interaction contributes to social understanding. However, we believe that the authors have limited their argument by focusing so heavily on early parent-child interaction. In the present commentary we discuss the contributions of early peer interaction to social skill and social understanding. Although C&L acknowledge the "sibling effect" (Perner et al. 1994) and the role of cooperative interactions with age-mates for children's use of mental state terms (Brown et al. 1996), they do not discuss how interactions with siblings and peers differ from adult-child interactions, and the possibly unique role played by interactions with other children in generating early social understanding.

C&L argue that "children's initial, fragile social understanding" is "at first evident when *supported* by social interaction" (sect. 4.1, para. 9, emphasis ours) and that "children gradually construct social understanding through the *regularities* they experience in interacting with others" (sect. 3, para. 1, emphasis ours). In their formulation, the young child's adult partner supports social engagement through regular and routinized interaction and, in that context, interprets and labels the child's mental states. From our view, this type of cooperative interaction, although probably initially necessary for children's nascent social understanding, can take children only so far toward a more general understanding of mind. In routinized interactions, especially with a sensitive adult who has complex mental state understanding, the child has little need to predict the partner's behavior or to communicate his own mental states. When social partners' behavior is predictable, and when routines and communication are either built-in or highly learned, having to read minds is not necessary.

Peer interaction, in contrast, provides a social context in which children's partners are unpredictable and their goals, intentions, and desires are often difficult to read even for sensitive, mind-minded mothers. The challenges of interacting with peers may require that children be explicit about communicating their intentions. Consistent with this notion, Smiley (2001) found that toddlers were more likely to state their future intentions in peer play than in mother-child interactions. Furthermore, peer interaction also provides children their only opportunity for true cooperation, defined by both Piaget (1932/1965b) and C&L as "interaction among equals." Thus, peers pose both unique challenges and unique opportunities for the growth of early social understanding.

Children are capable of establishing and maintaining positive peer interactions in play and in cooperative problem-solving by the end of their second year (Brownell et al. 2003; Eckerman et al. 1989; Howes et al. 1989). Without a way to "make sense" of other children's behavior, the toddler cannot anticipate a peer's behavior, hence cannot behave in a coordinated, reciprocal, accommodating manner, and peers' behavior will simply be inscrutable for the very young child. The fact that children can imitate one another by 14 to 18 months (Eckerman et al. 1989; Hanna & Meltzoff 1993) but cannot cooperate with one another until 24 months or later (Brownell & Carriger 1990; Brownell et al. 2003; Tomasello et al. 1993) suggests that their early understanding of others' minds, as constructed in adult-child relationships, remains too incomplete to serve interaction more generally. Moreover, the social understanding that develops in the context of early peer play is not a function of mental state talk between peers. Nonverbal interaction may be even more important for children's developing social understanding of one another. Mutual imitation, for example, may provide the first means for children to begin making sense of their peers' behavior and may be the grist for generating initial understanding of peers' intentions (Eckerman et al. 1989).

Social understanding not only arises in situations of peer cooperation and positive interaction around a shared theme, goal, or desire, but may also be adaptive and necessary in instances of competition. It is notable that possession struggles and conflicts, which constitute a relatively high proportion of early peer interactions (Brownell & Brown 1992; Hay & Ross 1982), are unique to child-child interactions. When one toddler approaches another, the child must figure out whether the peer intends to hug her, play with her, take away her favorite toy, or something else. Caregivers are not always available to assist children in interpreting and understanding such situations, and even when they are present, they often prefer to let children figure it out on their own. Hence, children may be motivated to learn how to read other children's mental states and to understand the relations between mental states and behavior so that they can effectively negotiate ambiguous, emotionally charged situations. Such interactions may also provide children insight into the nature of their own emotional experiences (Balaraman et al. 2003).

We do not mean to suggest that parent-child relationships and peer relationships are unconnected, however. Young toddlers who are more responsive to an adult's bids for joint attention are also better at coordinating their actions with a peer toward a common goal (Brownell et al. 2003). Toddlers who have more positive experiences in play with their mothers engage in more complex social play with their peers (Howes & Stewart 1987; Zerwas & Brownell 2003). Thus, the social understanding children construct during interaction with adults is also brought to bear on their interactions with peers. Nevertheless, establishing and maintaining positive interactions with peer partners pose distinctive challenges both affectively and cognitively. Peer interaction thereby stretches the limits of children's early social understanding and makes unique contributions to its development.

Authors' Response

Constructing understanding, with feeling

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Abstract: We explore three types of criticisms of our theory on the development of children's social understanding. We reject suggestions that we offer nothing new to traditional theories of development or recent "social" accounts of "theory of mind." Second, we take the point that there are grounds for improving our

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account of dyadic interaction in infancy but reject claims that we have not sufficiently accounted for how we incorporate the notions of criteria and structure into the theory. Third, we accept that the epistemic triangle, as defined, would benefit from an affective dimension and such a formulation could be used to describe the dynamic of developmental change from infancy to beyond early childhood. We still feel that the combination of Wittgenstein, Vygotsky, and Piaget remains as an antidote to the flaws in current "theories of mind" approaches to social understanding.

> *The squirrel does not infer by induction that it is going to need stores next winter as well.*
> —Wittgenstein, *On Certainty* (1969, para. 287)

For us the starting issue was that the dominant theories in the area of the development of children's social understanding, referred to as "children's theories of mind," view the problem that children must solve as inferring the presence of and learning about unobservable inner mental states. Wittgenstein's rejection of the idea of "squirrel cognition" summarizes our suspicions about an account of early human development that imputes the child with a social understanding that develops in an individualistic manner not grounded in shared practices. The dominant accounts initially made little reference to the child's experiences except to examine the young theoretician's database, although they are now accommodating to research on children's social experience. Hence, we turned to Wittgenstein and the very traditions within developmental psychology that the theory of mind tradition set itself up against. The target article was an attempt to pull together these diverse views in order to get a child's eye view of development. This we assume to be much more piecemeal *and* embedded in experience than recent accounts have suggested.

We have been both moved and inspired by the commentaries and strongly recommend them as worthwhile reading. In writing this Response, we feel we could have done more justice to about half the number and apologize to those who we simply mention; but we were restricted to about 200 words per commentary. So we have concentrated on some much more than others – and mainly those we felt challenged us more (they tended to be those we thought were closest to us in theoretical terms). The response is divided into three sections: general criticisms, specific concerns with our theory, and the many elegant extensions that were recommended. Far from criticizing our plea to make theories of the development of social understanding more socially embedded, many of these commentaries emphasize just how rich the child's social experiences are and how we did not go far enough.

R1. Is our theory needed? Or should it be upended?

There are three ways in which the commentators question whether our position is necessary: One group, who are discussed in the first subsection that follows, takes the long view and suggests that our approach is hardly new in its distillation of fine theories. The second part deals with a different view – that we have been trumped by recent socially embedded criticisms of theory of mind – and we discuss this in section R1.2. Finally, in sections R1.3 and R1.4 we reflect on the view that traditional accounts of theory of mind worked just nicely without the critical analysis that we offer, particularly the need for a Wittgensteinian analysis.

R1.1. Piaget and Vygotsky – again?

We are pleased that many of our commentators agree that a social perspective has long been neglected in the area known as children's theories of mind. And, like them, we believe it is needed. But have we simply recycled classic developmental theories in this article? Certainly, **Lourenço** assumed that we were making more grandiose claims about proposing a completely new theory than we were.[1] We make it clear that we draw on Piagetian and Vygotskyian theory – ideas with extensive histories. Thus, we agree with **Chesnokova** that the ideas we are working with have long histories, and we thank her for adding Luria's name to the list of theorists we rely on. **Sokol & Lalonde** also bring in Dewey and this list could be extended considerably to include the influence of Janet, Baldwin, Pierce, Werner, Bühler, and G. H. Mead. Tracing the extensive histories of the ideas in question was beyond the scope of our target article and we refer readers to excellent sources such as Overton (1998a) and Valsiner (1998).

So, is it "new" to draw upon Piaget and Vygotsky? **Lourenço** asserts that "without additional specifications and elaborations" Piaget and Vygotsky's theories "do not need to be repeated." Ironically, he has felt it necessary to contribute to the task of repeating these ideas to fend off common misinterpretations embedded in the scientific folklore (Lourenço & Machado 1996). In our defense, there is nothing we know of in Piaget and Vygotsky's work about issues equivalent to Wittgenstein's notion of criteria. This we draw from Chapman's (1987a; Carpendale et al. 1996) work on operational thinking and extend to children's reasoning about social matters. As many commentators point out, this is no straightforward matter and we turn to some of these issues below.

Perhaps implicitly criticizing our reliance on Piaget, **Symons** and **Fernyhough** suggest that we should instead rely more on Vygotsky. Vygotskian ideas, as **Chesnokova** rightly points out, are embedded in our approach.[2] Symons argues that we are tentative in outlining a mechanism of development and suggests we should employ Vygotsky's idea of internalization. However, this mechanism is far from fully specified, and Vygotsky himself acknowledged that "as yet the barest outline of this process is known" (1978, p. 57). Clearly, Vygotsky's theory needs to be fleshed out, as Chapman (1991) and Fernyhough (see also Fernyhough 1996) have done to some extent. Thus, we maintain that there is no necessary incompatibility between Piaget's views and Vygotsky's views on social relations and we feel that the former's account of internalization is required to flesh out the ideas of the latter.

R1.2. Are we merely repeating current social constructivist theories?

A few commentators imply that our ideas echo recent ones within the theory of mind tradition (see explicit statements in **Astington** and **Steele**). Astington questions how new our approach is and groups us with a family of similar theorists, some of whom are also commentators (Astington, **Fernyhough**, **Montgomery**, **Nelson**; see also Harris, in press). Astington is correct in pointing out areas of agreement between our article and the theorists she lists. We could also go further than Astington in identifying theorists with whom we have much in common concerning infant development, such as **Hobson**, and **Barresi & Moore**. We

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cite most of these authors in the text and acknowledge how they have influenced our ideas. However, we suggest both that there are clear differences between the authors she groups together and that we differ from all of them. For some, the differences relate to matters of detail. For example, Montgomery and Fernyhough still need an account of social development in infancy in order, among other reasons, to overcome the problems **Bickhard** points out in his commentary concerning the development of prelinguistic normativity. We agree with Astington and **Howe** that Judy Dunn's work has been seminal in establishing a need for a socially embedded model of mental state understanding and our theory is premised upon that assumption. It is work like Dunn's that stimulates a need for a detailed theoretical account and we suggest that a constructivist one provides an answer.

Many of the theorists cited by **Astington** differ from us over how children learn the meaning of mental verbs. For example, she groups us with Garfield et al. (2001). Here, if we stick to the general issue of the importance of language and social interaction, we could say we are in agreement. Indeed, Garfield et al. coordinated a great deal of evidence and advanced convincing arguments. However, we differ on the very starting problem that children have to solve. That is, are mental states some sort of hidden inner object that must be inferred? Like **Montgomery** but unlike others in Astington's list, we suggest that this is the wrong way of setting up the problem that children must solve. **Bickhard** and **Sokol & Lalonde** also present complementary positions in their commentaries. Astington acknowledges that there are differences, but considers these to be only details. However, this is where the devil is, and discussion of such differences is essential for further theoretical development.

There is an interesting implication in the fact that **Lourenço** and **Chesnokova** say the theory is old and they mean it is Piagetian and Vygotskian, whereas **Astington** says it is old but she means it is the same as a family of theories including Nelson (1996), Harris (in press), and herself. But someone has to be wrong, or not quite right. Unless we are mistaken, the theorists listed by Astington either do not mention Piaget or explicitly distance themselves from Piaget's fundamental ideas (see, e.g., Perner & Astington 1992). It seems that everyone recognizes something of themselves in our approach. Alternatively, it could be that they might be happy with the interpretation of Piaget that we draw on.

Lourenço further argues that our proposal is vague and ad hoc, and that a weakness of our approach is that we can explain anything by it. It is clear from the commentaries that this is a weakness shared by all the other theories in this area. **German & Leslie** argue that modular theory has no difficulty accommodating to the evidence that social interaction is important in social development. In fact, they now allow the inclusion of domain general processes in their model, which indicates that they are moving away from most other modular positions, apparently towards theory theory. Gerrans (2002, p. 308, original emphasis) has suggested that "on any view of modularity, the operation of a module *cannot be affected by central, domain general processes.*" Similarly, several commentators (**Bartsch & Estes**, **Montgomery** and **Ruffman**) argue that theory-theory can also deal with the evidence. The commentaries arguing that the modular and theory theory accounts explain the data that we present only discuss the data on social relationships, notably sibling interactions. We concede that these perspectives can be accommodated to such evidence, but, as noted in our target article (sect. 2.2), as the range of data widens this task becomes more difficult and their theories become more diffuse. Indeed it is getting quite hard to distinguish between these two positions:

> "theory of mind" is described as having an innate and possibly modular *basis,* not as consisting in its entirety as either innate or as a module. (**German & Leslie**, original emphasis)

> It is certainly true that there are some innately given kinds of psychological knowledge. However, it seems to us that these are most likely to be "starting state" theories. (Gopnik & Wellman 1992, p. 168)

Lourenço also claims that we have not conclusively ruled out the other theories in this area. First, even if we had devoted our target article to criticizing current theories (and more criticism is provided by commentators **Hobson**, **Mitchell**, and **Montgomery**), we still could not conclusively rule them out. They are not single theories, but rather families of theories sharing core assumptions or research programs in Lakatos's (1970) sense. Research programs cannot be simply ruled out because a modified theory based on the same core assumptions can always be proposed. Second, our criticism based on Wittgenstein is more far-reaching because it reveals flaws in an assumption on which all the dominant theories are based (Chapman 1987a; Montgomery 1997; 2002). The aim of the next subsection is to show that such an approach is a firm conceptual foundation on which to build (see sect. 2.2 in our target article).

R1.3. Is theory theory an adequate explanation? Or do we need Wittgenstein?

Ruffman's final paragraph presents some interesting attempts to reconcile the theory and constructivist perspectives, but does not bridge a fundamental difference between our approach and the theory theory in how children learn the meaning of mental state terms. **Montgomery** makes the point that the sibling data can be used to support the positions we criticize, but then goes on to elaborate on the argument from Wittgenstein. He writes that according to the theory theory (and, we would add, simulation and modular theories) children learn the meaning of mental state terms by means of a referential association with private inner sensations that are causally related to but distinct from outward behavior. Section 3.2 of the target article presents arguments like Montgomery's current and recent work (see Montgomery 1997; 2002). **Bickhard**'s discussion of the frame problem adds to the argument that beliefs are not objects that can be learned about through introspection. Bickhard points out that in the case of talk about beliefs there "is nothing unitary, no coherent kind of matter of fact, for belief talk to refer to." The frame problem shows that there is no limit to the number of beliefs a person could be said to have.

Sokol & Lalonde point out that Dewey's ideas about mental states are consistent with Wittgenstein's later work. One misunderstanding that might be possible after reading their commentary that we want to guard against is the claim that we are denying the existence of mental processes. It might appear that Wittgenstein denies mental states, and therefore he must be a behaviorist. If Wittgenstein denies that a sensation is a mental object, it may appear that he is

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saying there is no sensation, in which case the child's understanding of mind amounts to no more than learning to talk about mental states through enculturation. But that is not what he intended: "And now it looks as if we had denied mental processes. And naturally we don't want to deny them" (Wittgenstein 1968, para. 308). "To deny the mental process would mean to deny the remembering; to deny that anyone ever remembers anything" (Wittgenstein 1953/1968, para. 306). What Wittgenstein did reject was labeling mental processes as private objects in the same way we label physical objects. Thus, his argument is about how our language makes us think about psychological processes, not a denial of psychological processes.

Our use of Wittgensteinian ideas is questioned by **Shotter**, although we note that he does not challenge our philosophical interpretation. Shotter argues that Wittgenstein should not be taken piecemeal and that he was concerned with description not theory. To deal with this criticism we need to apply Wittgenstein's own method to be clear about both the task he took on and the sense in which he used the term "theory." In the paragraph cited by Shotter, Wittgenstein (1953/1968, para. 109) goes on to explain why *his* considerations could not be scientific ones. It is because the problems he addresses are not empirical but philosophical, focusing exclusively on the meaning of words, due to us being misled by the workings of our language: "Philosophy is a battle against the bewitchment of our intelligence by means of language" (para. 109). His analysis leads to the conclusion that words are torn free from their natural context, and meanings of words only exist in particular contexts. It is this crucial insight that we draw upon in our analysis. Using this idea is not incompatible with the study of human development. Indeed, there are two reasons why we feel justified in using it in our developmental account. First, we should begin from the foundation that Wittgenstein has cleared. In Wittgenstein's words: "I am not interested in constructing a building, so much as in having a perspicuous view of the foundations of possible buildings" (Wittgenstein 1980, p. 7e). This does not preclude what is built on these foundations. Indeed, secondly, after he disposes of our common misconceptions about how children learn the meaning of words for inner sensations, Wittgenstein goes on to answer his own question of how children learn the meaning of such words by writing: "Here is one possibility . . ." (1953/1968, para. 244). It is just this possibility that we are exploring in our theory. This runs counter to Shotter's relativistic stance on Wittgenstein, but we are agreed that these ideas provide a damning critique of the dominant theories of theories of mind.

We should consider why Wittgenstein felt that theory was not appropriate for the task he took on. Given his concern that words only have meaning in particular contexts, a general set of rules for mechanistically deriving meaning (i.e., a theory in one sense of the term) cannot be formulated, because meaning is not mechanistic (see Goldberg 1991). Instead, meaning is indexical. It depends on the location of words or utterances in sequences of interaction (**Budwig**; see also Suchman 1987; Turnbull 2003). This issue can be illuminated by considering a parallel debate between Searle (1992) and Schegloff (1992) on whether a theory of conversation is possible. Searle concluded that a theory of conversation is not possible. In one sense of the term "theory" Searle (1992) is correct. That is, it is not possible to form a theory that would allow us to derive meanings of utterances in conversation because such meanings always depend upon their location in particular sequences of interaction. However, this does not stop us from learning a great deal about the process of conveying meaning in conversation, as shown by Schegloff (1992). The indexical nature of meaning pointed out by Budwig is part of a general problem of the development of knowledge that leads us to a constructivist view, including how children learn the meanings of mental state terms.

R1.4. Is modular theory an adequate explanation? Reductionism and constructivism revisited

German & Leslie object to our presentation of the modular view, and they especially object to our contrast between this and the position that humans have evolved the capacity to develop an understanding of mind. Instead, they insist that it is the latter position that they endorse as the modular position. In fact, the statement that humans have evolved the capacity to develop social understanding is just a logical necessity. Within that general agreement there is obviously a lot of controversy. In support of their case, German & Leslie cite a study by Hughes and Cutting (1999) reporting a strong heritable component (60%) to false belief performance. However, in their more recent research with a sample 10 times the size, Hughes et al. (in press) found a much weaker relation: only 16% genetic influences compared with 63% environmental influences (see also Deneault et al. 2003). Furthermore, such findings tell us nothing about the *process* through which any genetic influences might work.

German & Leslie quote Roth and Leslie (1998), who state that the theory of mind mechanism is essential because it "allows the young brain to *attend* to . . . mental states despite the fact that such states cannot be seen, heard, felt, or otherwise sensed" (emphasis in original). They go on to state that what is critical is the ability to attend to mental state properties such as pretending and believing. However, what it could possibly mean for the child to attend to believing is far from clear in their account. The Wittgensteinian argument above and in section 3.2 of our target article shows that they have gone wrong from the very start because the question they address is misconceived – mental states cannot simply be "attended to" (see also **Bickhard**, **Montgomery**, **Sokol & Lalonde**). The fact that German & Leslie do not recognize our position as a proposal implies that their starting assumptions differ so much from ours that they have a different notion of just what a proposal should look like. Yet we call their starting assumptions into question.

Thus, there is an even more fundamental problem with **German & Leslie**'s account that is shared by two other commentaries. Their approach is reductionistic in their talk about how the "young brain attends" to mental states, and this reduces attending to neural activity (Brown 2003). **Craig & Barrett** take a stronger position than German & Leslie in assuming that folk psychology is innate, due to "certain devices inside us" (Millikan 1993, p. 63). Furthermore, they claim that it is a job for neuroscientists to look for those devices. **Gerrans** takes a similar tack, treating Wittgenstein's investigations as irrelevant for development psychologists who, he states, should really be concerned with discovering the "neurocognitive processes" involved in concept formation. In our view, a Wittgensteinian analysis undermines the leap that these commentators make so ef-

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fortlessly between neural activity and psychological constructs.

Neurological research will not address the questions we are concerned with because it does not deal with meaning. **Craig & Barrett** exemplify the problems inherent in a reductionist approach in their claim that the only thing they are certain of is that knowledge of the world is embodied in one's particular configuration of synaptic connections. Why stop here? Wouldn't it be even more certain to go to the level of the distribution of neurotransmitters at the synapse or at an even finer biochemical level? Synaptic connections may contain information, in some sense of the word, but not knowledge. A train schedule has information but no knowledge; a person is required to read and interpret the schedule. Therefore, a homunculus is required (Heil 1981; Kenny 1991; Müller et al. 1998a; 1998b). In addition to focusing on neurocognitive mechanisms, **Gerrans** also takes a computational approach. This is fundamentally flawed because there is no way to deal with meaning. "Computers don't understand anything, nor do they care" (Hobson 2002, p. xiv). The problem for this sort of approach is to "explain the transition from causal processes in the brain to conscious, meaningful experience" (Müller et al. 1998a, pp. 229–30. For similar arguments, see Hacker 1991).

It is for this reason that we are committed to the alternative, constructive approach to development. **Siegal** asks whether the constructive view can account for the development of specific beliefs, distinguishing these from "core cognitive architecture." Exactly the same process is involved. From a constructive view of knowledge development, concepts cannot simply be transmitted ready-made. Rather, understanding of all types must be constructed by the child in interaction. Nevertheless, our constructive approach is consistent with evolutionary theory. We take the point from **Eddy**, **Fonagy**, and **German & Leslie** that it is important to consider what biological adaptations allow humans to develop social interaction and social understanding. These make language, human history, and culture possible (Elias 1978). In addressing this problem, we follow Tomasello (1999b) in being particularly cautious about attributing innate knowledge to infants. The problem is not simply solved by saying that infants are born being able to attend to mental states. It is still necessary to explain how an ability evolved. Furthermore, if it is possible for it to have evolved we need an explanation for why it could not develop in individual ontogeny (Bickhard 2004).

Eddy considers a possible interpretation of Povinelli and Eddy's (1996) findings that although chimpanzees follow gaze they do not demonstrate an understanding of the psychological implications of seeing. He notes the possibility that the chimpanzees may have failed their task because they were responding to humans and not to other chimpanzees. Although we agree with Eddy that it is an unlikely explanation, the possibility remains to be evaluated. However, it is evident that we humans do not restrict mentalistic interpretations to our own species, because we had assumed that when chimpanzees follow gaze they also understand the psychological implications of seeing just as we do. If it were found that chimpanzees did restrict their social understanding to members of their own species it would indicate a difference in their social understanding compared to that of humans. In any case, there is additional evidence that a range of increasingly complex forms of gaze following can be observed in human infants.

R2. Should the theory be amended?

Although some of the commentators discussed in section R1 are keen to maintain that traditional theoretical positions remain intact in the face of the evidence we summarize, a second group examined the internal consistency of our theory, and we address these here. In the three parts of this section we consider claims from a number of angles that the notion of the epistemic triangle does not fit very well in describing infants' social skills (R2.1), that Wittgenstein's notion of criteria exposes us to a diversity of possible problems (R2.2), and that we have not specified the nature of the structures or processes that the child develops (R2.3). All these analyses call for amendments to the original thesis.

R2.1. The development of the epistemic-relational triangle in infancy: Getting dyadic

Do we give a full enough explanation of infant development? For a start, as **Bickhard** argues, Wittgenstein did not account for prelanguage normativity. We agree. Wittgenstein said that the use of words is built onto previous shared practices but he did not explain how these practices arise except to say that the child is "trained." Such practices are where Wittgenstein said he hit bedrock and his spade was turned. But this is where Piaget kept on digging (Müller 1999). And for this part of our argument we rely on Piagetian theory in accounting for development in infancy.

It is Chapman's (1991) notion of the epistemic triangle that helps us build a social dimension in a Piagetian approach to infancy. Does this formulation stand up to the critical scrutiny of our commentators? **Ruffman** is concerned because he suggests that there is evidence of an early developing self-other distinction. He cites two examples. One is at 18 months of age when we too would obviously expect a self-other distinction to have developed. He also refers to evidence from research with infants from 3 to 4 months of age. The study cited by Ruffman (Rochat & Striano 2002) was with 4- to 5- and 8- to 9-month-old infants. There was evidence of longer looking time in the other condition compared to the self condition, as well as development between the two age groups. However, Rochat and Striano acknowledged that there were a number of differences between the conditions in addition to self versus other that make firm conclusions difficult. And infants' ability to respond differentially tells us little about whether they have any understanding of a self-other distinction (Müller & Carpendale, 2004). However, in a developmental account such as ours, early forms of self-other knowledge must be looked for. Therefore, we agree with Rochat and Striano (2002, p. 44) that these abilities emerging at 4 to 5 months form the "foundation from which children can eventually develop the conceptual and explicit sense of themselves expressed by the middle of the second year."

The problems to be solved in infant social development are set up very well and in different ways by **Barresi & Moore** and **Hobson**. Our position is very similar to that of Barresi & Moore. We agree that social understanding develops out of engagement with others – that it is "always out of shared activity with others that advances in understanding of mental phenomena are first made." We recognize

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that two individuals have independent perspectives of the object and of each other; but there remains one issue of possible disagreement between us and this concerns the nature and development of representation. Barresi & Moore's use of terms like "reflective representation" and "integrating information" may buy into a theory of representation which is at odds with the activity-based view that we hold (Müller et al. 1998b). However, as we discuss below, their comments on emotional relations and the "we"-ness of interaction are well-taken.

Hobson holds that theories of infant social development must account for primitive modes of experiencing of self and other in interpersonal relations. One of his questions concerns how infants are aware of persons as having a mental as well as a physical dimension. This remark could be misunderstood because Hobson is usually at pains to deny that the initial task for the infant is to discern mental states (Hobson 1994). Rather, he is usually interested in accounting for the formation of the self-other distinction and, like us, he locates this in the dynamic of interaction between the infant and the other. We are also cautious about the solution Hobson proposes. He suggests that "humans are equipped with a propensity for forms of role-taking that both link an individual infant or child or adult with someone else, and at the same time register the distinctiveness of self and other." It is important to know exactly what is meant here because the term "role-taking" would usually be interpreted at a much higher level (Flavell et al. 1968). We assumed that Hobson (and he has confirmed this via e-mail) does not intend it in this way and his commentary goes on to describe this process as: "children are moved by the attitudes of others." Perhaps all that is really needed is a propensity to engage with other people (helped by infants' predisposition to be interested in others' eyes and faces, as **Ruffman** suggests) and the ability to respond to their reactions, initially without any clear idea of the other as separate from the self.

Hobson is critical of our focus on action and instead suggests that what is needed in addition is an emphasis on emotional engagement. We agree with the addition. Furthermore, he asks us how communication is grounded. From our perspective it is embedded in interaction. This starts with a pattern in which the infant emits affective signals that are punctuated in protoconversation by the adult (see, e.g., Kaye 1982; Schaffer 1977). Gradually the infant's contribution to the interaction becomes more active, in terms of responding to others on this affective level. At first this is dyadic, without reference to objects (Chapman 1991). The goal is just the reaction, there is no communication outside that. Research over the past thirty years has suggested that infants treat humans differently from objects because they react contingently (Watson 1972), and caregivers are intent on extending the length of bouts of affective engagement with very young infants (Stern 1977). We contend that a much leaner interpretation can be used instead of the one offered by Hobson. Indeed, we agree with his later suggestion regarding the ability to be "moved" by another's attitudes. This develops apace within dyadic interaction within the first year and makes possible the development of triadic interaction (subject-interlocutor–object) and the capacity for symbolic thinking.

In the target article we place much stress on the epistemic triangle as a basis for development. We need to clarify its relation with triadic interaction following **Meins**'s comments. What is referred to is interaction with other people about aspects of the world. It makes no difference if there are several other people involved – this would also be triadic in our sense. The point is to distinguish this form of interaction from forms of interaction in early infancy that are dyadic in that they involve the infant just interacting with the physical world or with a person. However, it is a misinterpretation by **Wilson-Brune & Woodward** to assume that we believe that the development of infant's social knowledge only begins with triadic interaction. This would be a completely non-developmental account. The question of how infants develop the capacity for triadic interaction from dyadic interaction must be addressed (this issue was discussed in sect. 3.1 in our target article). **Leekam** points out that children with either autism or Williams syndrome have difficulties with dyadic interaction but for different reasons. Children with autism focus on dyadic interaction with objects, whereas children with Williams syndrome are more dyadically engaged with people. Both groups have difficulty with triadic interaction. By Leekam's analysis the problems identified in these syndromes are much more basic than at the "theory of mind" level (Leekam, in press).

Wilson-Brune & Woodward suggest that our approach does not hold in the case of the emergence of pointing because infants' first pointing gestures may not be communicative but may instead index their own attention. Although there is much controversy about the origins of pointing, the evidence is consistent with our approach. We expect (as do Moore & Corkum 1994) that infants first get into forms of interaction for other reasons and then learn within such interaction. For example, we would expect that infants' first pointing gestures would not be clearly social. As Wilson-Brune & Woodward suggest (and also as stated in our article), there are clear developments in the sequence of pointing skills.

Thus, the lesson that we learn from our commentators in this section is that there is much to be gleaned from dyadic interaction in the first year of life, particularly in the level of affective engagement that the infant develops. We accept these ideas into our theory with thanks, especially as these commentaries remind us of the insights of parent-infant interaction studies in the 1970s and of Reddy's (e.g., 1991; 2003) impassioned critique of theory theory based in part on her rich descriptions of infant-parent interaction in the first year.

R2.2. Language and criteria

There are a number of points raised about our use of Wittgenstein's notion of criteria. These range from questions regarding word learning to the suggestion that the notion of criteria implies an enculturation position that we claimed to be avoiding.

The word learning problem raised by **Miller & Müller** is that there are limitless possible meanings of new words, whether they are object labels or verbs. Thus, they suggest that the problems with word learning as correlating words with things also apply to correlating words with events. They also state that learning mental state terms would be particularly difficult because psychological states are not directly observable. To solve these problems they offer an interesting, if traditional, solution – that because the problem of mapping word to world is so intractable children may use

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the structure of language as a cue to mental verb meaning. We do not deny that sentence structure is important in the child's language development in general and perhaps in learning the meaning of mental verbs. However, we have three responses to Miller & Müller's analysis. First, in section 3.2 of our target article and here above we rehearse the Wittgensteinian argument that learning meanings of mental verbs is not a matter of naming inner unobservable states (Montgomery 2002), a view that is supported in a number of commentaries (**Montgomery**, **Bickhard**, **Sokol & Lalonde**).

Second, we disagree with the assumptions about language that lead to setting up the problem of word learning in this way. **Miller & Müller** appear to assume the constraints approach to the problem of word learning, according to which word learning involves creating and eliminating many possible hypotheses about how new words map onto the world (Tomasello 2001). The solution they suggest is that syntactic bootstrapping constrains the number of possible hypotheses. In contrast to this slightly disembedded solution, we endorse the social pragmatic approach to word learning described by Tomasello (2001), according to which children are "engaged in social interactions in which they are attempting to understand and interpret adult communicative intentions – so as to make sense of the current situation (Nelson 1985)" (Tomasello 2001, p. 113). We view meaning as related to the purposes for which words are used within interaction (Budwig et al. 2000; Turnbull & Carpendale 1999a). Third, the structure of language may be important in children's learning the meaning of mental verbs, but the leap from syntax to meaning is still magical.[3] It is not clear how the word learning problem is resolved by changing it to the unpacking of syntax (which is what Miller & Müller seem to imply by referring to the "language structure") and mapping onto mental states. We endorse **Budwig**'s suggestion to study constructions as the unit of analysis, because such a usage-based unit provides a far more credible route into meaning through language (Budwig 1998; Tomasello 1998).

Our suggestion to look beyond mental state terms to study talk about human activity is reflected on by **Symons**. He argues that we should not only look at parent-child discourse as a window on the process by which children acquire such terms, but we should also consider the child's spontaneous utterances during "solitary" tasks. This is an interesting idea that is compatible with our approach, and Symons considers this as an important potential route into an understanding of the child's internal mental states. From the unpublished study that he recounts, children's "discourse in solitary tasks" correlates highly with false belief understanding. Without more information about the task and whether the children were asked to narrate while performing it, we find it hard to judge whether Symons has identified a mechanism in keeping with Vygotsky's ideas. We agree that all children's activities, whether in discussion with adults, peers, siblings, or "alone," are useful markers of their constructions of mental state terms. Like Symons, our aim is to bridge the gap between parents'/others' mental state discourse and the child's constructions. However, we would add that it is not sufficient to correlate the frequency of mental state terms with test data. We agree with **Budwig** that it is important to examine how interactants use mental state terms for various purposes. In doing so the mechanism that Symons refers to retains its triadic structure, within its interactional context. As we noted in our target article, words that are not considered mental state terms may still be important in talk about the psychological world. Although there is little research on this topic, Lohmann and Tomasello's (2003) language training study found no difference in training effect on false belief understanding between a condition in which mental state terms were used and a condition in which communication verbs such as "say" and "tell" were used.

Another commentary asks us to identify how children come to learn criteria for mental state terms. **Barresi & Moore** are concerned that our theory potentially reduces to teacher-based enculturation. We feel that their depiction of language and criteria in our target article is incomplete because in section 3.1 we are at pains to identify the centrality of children's constructions of knowledge. This discussion was set into the framework of practices and criteria in section 3.2 to identify the dynamics of the epistemic triangle. In contrast to Barresi & Moore, we suggest that our combined use of Piagetian theory and criteria protects us from an enculturation view. In addressing this criticism we refer to a distinction that **Bickhard** points out in his commentary, which is that, although children learn the meaning of words in terms of criteria, what is learned is not constituted by the criteria. Furthermore, it is not just that children learn culturally specific words for mental phenomena such as seeing; rather, these words are added to and become part of a practice. So, according to our position (and against a possibility considered by Barresi & Moore), learning practices do not depend on a theory of mind; infants develop expectations about what will happen in certain situations in their practical, lived knowledge. For example, when a child learns to point declaratively to direct others' attention she is beginning to learn something about other people, the world, and how to coordinate her directedness toward aspects of the world with other people's directedness, or attention. Then, within interactions in which words are used to accompany pointing, she may add the word "look" to this shared practice. That is, this practice of pointing is one possible criterion for the use of the word "look." And, of course, the use of words is a constructive process, it cannot simply be social transmitted (see **Nelson**'s comment about word use without meaning).

R2.3. Structure and executive functions

A third set of commentaries ask us to clarify the nature of the cognitive system that the child develops. **Miller & Müller** argue that we need to describe the knowledge structures that characterize different levels of social understanding. The term "structure" is ambiguous because it can be interpreted as causally determining thinking or as a formal description of forms of thought (Chandler 1991; Chapman 1987b). We assume that Miller & Müller would not mean it in the causal or functional sense but rather in the formal sense. However, in the list of theories they provide it is likely that a majority of theorists think of their structures as causal in the process of reasoning. In addition, one position that is missing, the theory theory, is assumed by Gopnik and Wellman (1992, p. 147) to be causal in the sense that the process of reasoning involves applying the laws of the theory. Another issue is whether structures are general across content. Most of the theorists listed by Miller & Müller would take this position; that is, once acquired,

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the structure would apply across content and in various circumstances. Our position, however, is that functionally distinct structures develop for each of the forms of joint attention. For example, pointings of different types (following pointing, following gaze, etc.) develop individually but these become reciprocally assimilated. That is, they are all activated by interacting with people and are combined gradually during social interactions.

In discussing the development of structure, **Miller & Müller** quote Piaget (1967, p. 147): "Genesis emanates from a structure and culminates in another structure." There are suggestions in some other commentaries that go beyond our original analysis and illustrate just what is implied in this quotation. **Fonagy** is in broad agreement with our position and in drawing connections between Piaget and Bowlby (as does **Symons** from a critical perspective). But Fonagy also suggests other ways to think about the connections between attachment and social understanding. We agree with him (and **Steele**, who also mentions it) that an issue which we did not discuss is the role of attachment in emotional regulation. The structure of parent-child interaction plays a role in the child's emotional regulation, and this structure is gradually internalized into the patterns we see in classic attachment relationships. Fonagy also extends the same sort of argument to effortful control and inhibition, as abilities that develop within interpersonal engagement and are fostered by secure attachment relationships. We agree and, as we argue below, this fits nicely with our position that executive functioning is influenced by the child's history of social experience, not just an unfolding in maturation.

Fernyhough also wants us to specify the cognitive processes that result from developmental processes, whereas **Gerrans** asks us to specify the cognitive processes that lead to development. This reveals the greatly differing theoretical perspectives that have been put to us. Our approach is far closer to **Chesnokova** in her useful term "agency mediation" and to Fernyhough, who says that we explain how social competence develops but not what it is. Fernyhough is right that this is an aspect that we only hinted at and did not fully develop. He further wonders if the epistemic triangle is just a context for development or whether it, in addition, plays a role in children's reasoning about the mind. Fernyhough's discussion of Vygotsky is welcome and it is consistent with our position, developed from Chapman's theory. Chapman's position, drawing on Piaget, Vygotsky, and Gal'perin, is that the epistemic triangle is interiorized as a whole:

> interiorized communicative exchange between the subject and the interiorized interlocutor is the origin of reflective awareness. The coordination of cognitive operations by the subject and the interlocutor is the source of the "operational compositions" that characterize concrete operations. Such coordinations are the interiorized counterpart of interpersonal cooperation. (Chapman 1991, p. 219)

Piaget (1962/2000) agreed with Vygotsky's view that reflective thought arises due to the interiorization of egocentric speech. Thus, in response to **Miller & Müller**, development "emanates" from the structure of triadic interaction which includes the structures that the child brings to the interaction, and it "culminates" in the interiorized structure of the epistemic triangle. As skills become consolidated, further levels of the structure involve the ability to reflect upon this interiorised epistemic triangle. These develop in a local, piecemeal way, not – as hinted at by Miller & Müller – by the application of a new rule or representation system.

The issue of structure has been made more complex by recent work, which links social development with what are taken to be domain general skills under the banner of executive functions. **Hala** suggests that our position is compatible with executive function accounts. We agree that an aspect of our approach that needs elaboration is in relation to the notion of executive functions, and in our target article we could only acknowledge that in a brief note. Two key issues are raised. First, as has often been pointed out (Monsell 1996; Rabbitt 1997; Towse et al. 2000), the notion of executive functions is complex and ill-defined even in adults, so we must be immediately cautious about describing individual skills or the ragbag of "executive" skills as given. However, recent structural equation modeling analyses suggest that in both adults (Miyake et al. 2000) and children (Shimmon et al. 2003) there is distinctiveness between areas of executive test performance (e.g., working memory is distinct from set shifting), but individual constructs like these fit best into a unified latent variable "executive function." However, performance on test batteries does not reveal conceptual links between, or a developmental model of, such skills. An advantage of an activity-based approach is that we do not have to think of something like "attention" or "attention-shifting" as independent characteristics of the child which the child brings to the interaction. Instead the child's "attentiveness" may well be influenced by the interaction of the dyad (Hobson, in press).

Therefore, second, we do not think it is helpful to assume that executive functions simply unfold with maturation. Rather, we agree with **Hala** that the development of these particular skills is likely to be complex, embedded within the nature of children's experiences. She points out that there might be a deeper relation between executive skills and different aspects of social understanding and that there is a bidirectional relationship between the child and other people. We would take the analysis further to examine a number of other bidirectional patterns of influence within the epistemic triangle – including interactions between each executive skill. It is these links that we take to be the more complex structures that are developing in the child and which are hinted at in the research cited by Hala. Although we acknowledge that such ideas are in need of supportive evidence, we note recent data which show that social interaction (maternal verbal scaffolding of the child) at age 3 years is linked with the child's language at age 4 and search retrieval skills at age 6 (Landry et al. 2002). Such findings suggest that the development of executive skills is embedded in the child's social experiences.

R3. Should the theory be extended?

Several commentators have offered suggestions about how our theory can be improved. In our response on infancy, above, we agreed that an understanding of social development must have as its starting point the dyadic exchanges between individuals. One of the points that we understated in the target article was that much of this interaction is highly affectively laden and what the child constructs within the interaction is charged with emotion. In the main, the commentaries discussed in this section reflect upon the ways in which the theory we present can be improved by

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embedding the epistemic triangle within a wider and more enriched social process – the reason why we are prepared to refer to it as the epistemic-relational triangle. The target article suggests that this framework is needed in order to understand the dynamic between the subject-interlocutor and subject-object interaction involved in the development of knowledge. We admit that we couched our analysis in a way that enabled an exchange and critique of the traditional approaches in this area that we have just discussed – approaches in which mental states are segregated from anything that remotely resembles human experience. The commentaries we discuss in this section mainly point out that there is more to the development of social knowledge than the emergence of the child's ability to reflect on cognitive mental states like beliefs. These other aspects of development hinge on issues that we hinted at in the target article, surrounding the nature and importance of affective engagement both in infancy and, indeed, all human social understanding. We welcome such a move, as long as the dynamic of subject-interlocutor and subject-object is kept in place.

R3.1. The role of relationships in social development: Let's get emotional

We are impressed by the ways in which several of the commentators extend our analysis of the role of the individual's constructions within interactions into specific types of relationships such as siblings and peers, and the importance of affect engagement, particularly in attachment relationships and emotional relatedness. However, as a result of comments from **Gerrans**, **Meins**, and **Steele**, we first need to clarify the relational position we endorse.

Gerrans seems to think that rejecting individualism means we cannot talk about the characteristics of individuals, like those with autism. To the contrary, rejecting individualism does not mean that we ignore the fact that individuals vary in the characteristics they bring to interaction. **McGeer** asks us to clarify that a rejection of individualism does not imply rejecting internalism. We agree. We do not just focus on the individual or the social. Relationism or systemism takes the strengths of both individualism and collectivism. Our "mechanism" is the internalization of action and interaction. That is, our explanation for the development of social understanding cannot be individualistic because it depends on the relations between people. But neither is it holistic because obviously individuals interact and bring their characteristics to that interaction. **Gerrans** is right that children with disorders such as autism have some neurological impairment. However, the whole story cannot be told on the basis of the individual. Instead, the relations between people are essential, because children with other difficulties such as deafness or severe social deprivation may also develop an autism-like syndrome (Hobson 2002).

In contrast to **Gerrans**, **Meins** claims that we focus too much on the child and that "the child's attributes may initially be rather unimportant." We should not focus on either the child or the other person; rather it is the relations between them that are important. Of course, both interactants will influence those relations due to their characteristics. The claim that individual differences in caregivers are important is embedded in our position, and therefore we do not assume that all mother-infant and child-sibling relationships are the same, contrary to Meins's reading. Meins is right that the effects of siblings and of parenting style have been found only with preschoolers but this reflects the nature of the available data, not necessarily that we are trying to explain the wrong evidence. Within our framework it is important to examine the nature of sibling relationships (the quality of interaction, etc.), not just whether or not the child has a sibling.

Meins notes that we mention the link between attachment security and social understanding and implies that we believe this is a direct link. In fact, we mention this as evidence that requires an explanation, not as an explanation itself. However, we do clearly agree with Meins that mere exposure to mental state language is not enough for children's social development. What she calls for in her final paragraph regarding an integrated system is just what we mean by a focus on relations.

Howe is in agreement with our position and suggests extensions by further focusing on children's sibling relationships, their friendships, and their pretend play. We agree, and further suggest that in such extensions we need a theory with which to make sense of various forms of interaction. We wanted to make it clear that the Piagetian distinction between constraining and cooperative relationships was originally introduced by Piaget (1932/1965b, 1977/1995) by contrasting parent and peer relations. **Steele** views Piaget's distinction between constraining and cooperative relationships as a "dichotomous model." However, the target article states clearly that Piaget's distinction refers to a continuous model because any relationship (i.e., parent-child, siblings, and peers) is some mixture of these two types.

We acknowledge **Steele**'s claim that other aspects of social relationships should be considered to capture the complexities of human relationships. Some of his suggestions appear to refer to the content of discussion. In our article we recognize the importance of both what is being talked about and the influence of the relationship on the child's understanding of that talk. Steele extends this analysis to describe the types of attachment relationship, but not explicitly to the processes that constitute such types. We feel that a more profitable extension of our analysis within attachment theory would be to follow the suggestion of **Fonagy** and **Thompson & Raikes**, who focus more closely upon the *dynamics* of attachment and emotional relationships. Both these perspectives are constructive, affective additions to the approach we propose. We feel that, just as cognitively based models like ours need an emotional dimension provided by these theoretical perspectives, attachment-based accounts also need an analysis of triadic interaction in our sense, particularly into which language is integrated. So we hope that the argument is more "new" than Steele suggests.

We also concur with **Banerjee** who discusses the role of social experience, or self-regulation, in advanced social understanding, such as the role of peer interactions in learning about interpretation. **Zerwas**, **Balaraman & Brownell** (**Zerwas et al**.) provide an elaboration of the role of peers in social development and the special challenges and opportunities afforded by peers, involving opportunities for cooperation as well as conflict. The role of conflict is also discussed by **Howe** and **Thompson & Raikes**. At least one important aspect of conflict between mothers and their children concerns how mothers deal with and talk about inevitable conflicts that occur with young

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toddlers (Laible & Thompson 2002). This line of evidence fits with the evidence reviewed in our target article.

Most of the commentaries discussed in this section suggest that we should extend our framework to include different types of social relationships. Two commentators (**Nelson**, **Vinden**) feel that an understanding of such relationships can be achieved only within a wider cultural framework and that we did not go far enough in this direction. Vinden advances a constructivistic view of enculturation, albeit using terminology slightly different from ours. We do not disagree with her suggestion. Our purpose was to distinguish our position of construction and reconstruction from a view of passive socialization or the transmission of knowledge. We do not dismiss the importance of culture. We agree that the child will influence the relationship with her caregivers, but we do not think that enculturation is the right word for this, given that it is often taken to consist of the passive passing on of beliefs, conventions, and so on, from one generation to the next. Nelson also thinks that we did not go far enough in the cultural-historical direction. We agree that such an approach should be taken and we believe that ours could easily be expanded in that direction. However, we would emphasize that "culture" should not be reified. It is created and recreated by interactants within everyday interactions (Berger & Luckmann 1966), thus the epistemic triangle is a means of understanding the dynamic between individual action and everyday practices. In the next section we elaborate on further possible developments of our theory raised in the commentaries.

R3.2. Broadening our conception of social understanding: Let's get more emotional (and normative)

In this section we add four perspectives from the commentators which are consistent with and further extend our theory. First, we must broaden the age span examined in the target article, as suggested by **Mitchell**. **Banerjee** and **Jenkins & Oatley** helpfully list further development in social understanding beyond five years of age. We want to emphasize our strong agreement that our list of issues beyond false belief needs to be extended, because **Lourenço** appeared to have the impression that we might be thinking that development in social understanding stops at age eight years with the beginnings of an interpretative understanding of knowledge. Jenkins and Oatley provide examples of distributed cognition within the crew of a navy ship to hint at the use of the epistemic triangle in adult learning, as each specialist tries to negotiate meaning with his or her colleague. Their term "prioritization of joint goals" can be applied well after age eight years. Banerjee further broadens the conceptualization of social understanding to include irony (see also Yasui 2003), self-presentational motives, and emotions such as embarrassment in a wider variety of relationships, particularly with peers. To this broadening of how we think about social understanding and social competence we could add the development of competence in social interaction, such as politeness (Brown & Levinson 1987; Turnbull 2003). Our theory provides a framework with which to incorporate these extensions. Only if developmental psychology extends the focus of the field in directions like these will we be able to address **McGeer**'s question of what is included in "social understanding." We agree with her suggestion that what is developing is not just a body of knowledge, but rather a set of practices and that this involves the development of social agency and the ability to interact within a community. The issue of agency is also hinted at by **Sokol & Lalonde** (see Sokol & Chandler 2003). Russell (1996) has identified the depth of the role of agency in the development of knowledge, but he downplays analysis of its social dimension.

The second issue concerns the extension of our framework to include other aspects of cognition, not usually linked to a social dimension. **Smith** questions whether we can account for the development of normativity from the perspective of the epistemic triangle. We recognize that the issue of normativity, or how we get from the causal conditions of knowledge to normative knowledge, is a neglected problem in developmental psychology, and it is also a test of an adequate theory of development. We did not deal with the issue of normativity, but we believe that our approach could be extended to do so for the reason that our position is based on Piagetian ideas about the development of knowledge. (Note that the computational approach advocated by **Gerrans** and the neuro-reductive approach championed by **Craig & Barrett** cannot recognize normativity as a topic of study.) As Smith (1993) argues, one of the goals of Piaget's genetic (i.e., developmental) epistemology was to ground normativity in the natural world – that is, to "show how to secure a place for the normative within the natural" (Kitchener 2004, p. 61). In Kitchener's words, Piaget's solution to this problem was that "epistemic norms emerge from certain kind of social relations between individuals – those involving cooperation" (p. 62). Therefore, a necessary component for normativity is social interaction of particular types. By this token, all of Smith's examples of normativity are rooted in social interaction because they are justifications. We suggest that an act of judgment does not exist outside of human practices. For example, Smith's case [E] appears to be an instance of the epistemic triangle in action.

Third, several commentators argue that we need to include emotions more fully (**Banerjee**, **Fonagy**, **Hobson**, **Izard**, **Thompson & Raikes**). We agree with Izard that there are a number of ways in which our account could be further extended to integrate the role of emotions. She points out many areas of literature that could enrich our perspective. As we argue below, with reference to a similar point made by Fonagy, such additions will more fully incorporate the role of emotions in social development beginning in infancy, and they are entirely consistent with our approach. Like Izard, we would avoid any hard and fast line between cognition and emotion because many emotions cannot be simply separated from cognition, as in surprise, envy, and jealousy. Second, as Izard suggests, emotions may play a role in influencing the relationships young children engage in, which in turn may influence the development of their social understanding. Such emotion may be partially attributed to the child's characteristics but must take into account the interaction between the child and the other person. We agree with Banerjee that the notion of self-regulation provides a crucial link with our own framework for understanding the child's internalization of the epistemic triangle. From our perspective it follows that children learn about emotions in the same way that they learn about beliefs and intentions. Finally, Thompson & Raikes extend the need to study emotional development in children's early understanding of mental states within a broader attachment framework.

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The fourth area of research suggested in the commentaries is a focus upon more intricate details of the child's conversations with others (**Astington**, **Nelson**, **Siegal**, **Thompson & Raikes**, **Walker-Andrews & Hudson**). We find Nelson's label of "collaborative construction" helpful as it places the child's developing understanding within a network of social relationships and the ebb and flow of everyday discourse. It provides clear guidelines for how research on children's understanding of the mind should be conducted. This is exemplified in the research areas described by Walker-Andrews & Hudson: We agree that the work they review on emotion perception, pretence understanding, event knowledge, and autobiographical memory will add to the richness of our account of mental state understanding. Thompson & Raikes suggest that we neglect several explicit and implicit features of conversational discourse such as provocative questions, elaborative detail, and mothers' reference to emotions. We agree that it is important to consider other aspects of relationships beyond cooperation and constraint, as well as to examine cooperative interaction in concrete detail. However, we also feel that when considering factors such as these, the notion of cooperation may be useful because such forms of discourse may facilitate development more in cooperative relationships than in constraining relationships.

Astington makes the interesting suggestion that the child's linguistic ability and experience in social and communicative interaction may play different roles in social development, although she acknowledges that the two factors are related. However, in the view we take of language as activity, it would seem to be very hard conceptually to separate such factors. There are also potential empirical problems with the way theory of mind data are collected, which we refer to in the target article's concluding section. To use Astington's research examples, we need to know about the quality of *interactions* of deaf children with hearing parents, and the parent-child dyads in hearing children with no siblings but high language ability, in order to reveal the possible reasons why these children appear to be different in their false belief performance compared to other groups. Nevertheless, Astington's commentary underlines the importance of analyzing language and communication in detail, as well as our conceptions of language (**Budwig**, **Nelson**). The important issue is that reference to "conversation" alone is not sufficient either to undermine our position or to explain the influence of interaction on cognition. Indeed, it can lead to the problems that are inherent in **Siegal**'s account. He appears to suggest that (1) the child's theory of mind is situated within her or his cognitive architecture, and (2) the false belief performance is essentially an understanding of conversation. The fact that Siegal offers no definition of "conversation" stands in contrast to the accounts of **Nelson** and **Thompson & Raikes**. His description offers little in the way in which the architecture develops or how the term conversation helps in his analysis. The target article and many commentaries attempt to grapple with these links.

R4. Feeling our way forward

We expected more criticism of both our use of a Wittgensteinian analysis of word-world relations and the blend with a mix of Piagetian and Vygotskian constructivism. We are slightly puzzled by the relative lack of explicit critiques of either these individual theories or how we put them together. We see this cocktail as providing a fundamental critique of the study of social development, or indeed any developmental or cognitive theory, in which meaning is slipped in before the theory is articulated. We welcome further discussion on this issue because we agree with **Nelson** that the implications are potentially profound.

The stumbling block for many of our commentators was the lack of a "mechanism" in our theory. One implication of the theoretical origins of our approach is that it is activity-based and grounded in practices, not internal representations. Structure arises as a result of the process. Thus, the mechanism and the process are the same, and each is based on the child's constructions of social interactions, where meanings are negotiated. We realize that this unsettles some commentators but it follows naturally from our critique of their assumptions. The implications for research are first that we need more (not less) neurotic task fixation (although not just on false belief tasks), in which we try to work out how children construct meaning in a wider range of settings and tasks beyond false belief. Second, we need to conduct research on the development of children's shared practices upon which mental state understanding develops. The studies of Judy Dunn and the ideas outlined in the conclusion to our target article provide examples.

It was clear from the commentaries that we need to extend the theory, and most of the commentators felt that this was a relatively straightforward task. Although we need to add affective engagement in infancy and beyond, we agree with many (e.g., **Banerjee**, **Fonagy**, **Izard, Steele**) that cognition and emotion should not be completely separated – both are aspects of the individual's reaction to and constructions of events. But we maintain that the epistemic triangle is a vital formulation for understanding how individuals develop knowledge and it adds to the affective engagements described by many, including those from an attachment perspective. We have not described how our framework might work at higher levels of the structure, and we need to go further in explaining the development of structure by providing an account of how the epistemic triangle within particular domains of thought itself becomes an object of reflection.

We thank the referees of the original submission, the editors at *BBS*, and the commentators for their work, their challenges, and their constructive criticism. They have forced us to assimilate some points and painfully to accommodate others, thus reassuring us that the constructive process continues in development. As **Astington**'s commentary suggests, it seems that the field is changing. We look forward to a further consolidation of socially grounded theory of children's social understanding.

NOTES

1. Lourenço also suggests that our theory is not necessary because it does not go beyond theories such as Selman's (1980). Selman's theory is compatible with our position, but there are some important differences. Selman already assumes much of what we are trying to explain because he does not provide an account of infant social development. Furthermore he does not account for the role of language in social development. What he does do well is provide a description of the levels of complexity of different situations of social understanding, and integrating his theory could potentially extend our position.

Lourenço also asserts that we claimed to differ from Thelen and Smith because of the "ideas of interactionism and transac-

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tionalism." In fact, we did not say this. Rather, in Note 3 in our target article we state that although we have much in common with dynamic systems theories we appear to differ from Thelen et al. (2001) in the starting point in infant development. That is, they seem to assume objectivity rather than explain it (Müller & Carpendale 2001).

2. We refer anyone still convinced of fundamental differences between Piaget and Vygotsky to Smith's (1996) test of 10 statements from each of these theorists, which stump even experts (see also Amin & Valsiner 2004).

3. Miller & Müller note that in other work (Turnbull & Carpendale 1999a) we reject the code model of language (i.e., the view that language works by encoding meaning in words and utterances that are then transmitted to others for decoding) and they argue that the "language code" is also important. Our reasons for rejecting the code model is that the same word or utterance can be used to convey different meanings when used within different contexts of ongoing sequences of interaction (Turnbull & Carpendale 1999a). That is, as **Budwig** notes, language is indexical (e.g., Budwig et al. 2000; Suchman 1987; Turnbull 2003). This suggests that rather than thinking of language as having fixed meanings attached to representations (the view of language that Wittgenstein argued against), we should think of language as a means for directing and coordinating attention with others within shared social settings (Tomasello 2001; 2003). Miller and Müller are right that a word such as "think" can be used in many different ways, in many different language games. From this we take the point that we need to describe in more concrete detail how children might learn about the many ways the word "think" can be used.

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Letters "a" and "r" appearing before authors' initials refer to target article and response, respectively.

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