and cultural experiences becomes irrelevant: The false belief switch gets turned on anyway.

Language provides some of the architecture necessary for false belief understanding (Astington & Jenkins 1999; Jenkins & Astington 1996), and insofar as social factors do speed up the acquisition of false belief understanding, this probably occurs through the effect of social influences on language development. Recent evidence suggests that this may occur through the linguistic structure of complementation (de Villiers & Pyers 2002). Language also contributes to our next proposed component: the content of domain-specific knowledge.

Domain-specific knowledge. Successful generative thinking and planning between minds requires content knowledge of the domain under discussion. We distinguish two aspects. One is the subject of the discourse: cars, dance, engineering, genetics, and so on. Hutchins (1987) investigated distributed cognition on a navy ship, and reported that successful performance in a seven-person navigation team required that team members had experience of the different roles in the team. This gave them content knowledge to understand ambiguous utterances and to solve problems in a way that was close to the perspective of others. A second aspect is talk about mental processes. If two people misunderstand each other, they may introduce process talk to clarify meaning, intention, and so on. Mental state talk is essential in such clarification: "Do you remember?" "What did you think?" "Are you angry?" The social process suggested by C&L is clearly central to the development of such content knowledge (Jenkins et al. 2003; Ruffman et al. 2002).

Prioritization of joint goals. Only if one cares enough about the internal experiences of others can the joint goal achieve prominence over one's own goals. Oatley and Larocque (1995) have shown that a whole class of errors in joint planning derives from one person being committed to goals of the self rather than joint goals. Bowlby (1971) described what he called goal-corrected partnerships, in which toddlers represent and act upon the goals of others through having experienced the same thing in their parents' treatment of them. In support of this, Herrera and Dunn (1997) found that those children who had had their own goals recognized by parents and siblings earlier were more likely to play cooperatively with peers two years later. This aspect of theory-of-mind understanding is thus strongly influenced by the social processes outlined by C&L.

Conclusion. Our delineation of components of theory-of-mind understanding allows for increased conceptual clarity of social antecedents in development. False belief understanding occupies a pivotal role in theory-of-mind development because it enables a type of interaction between minds not otherwise possible (Jenkins & Astington 2000). False belief and theory of mind are not synonymous. Once false belief has been acquired, other components of theory of mind become more important in explaining why individuals differ in their capacity to enter into the minds of others.

Reconstructing children's understanding of mind: Reflections from the study of atypical development

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Abstract: Carpendale & Lewis's (C&L's) theoretical reconstruction of the "theory of mind" problem offers new hope but still has far to go. The study of atypical development may provide some useful insights for dealing with the work ahead. In particular I discuss three issues – the boundary problem, the question of end states, and the issue of the centrality of triadic interaction.

Carpendale & Lewis (C&L) proclaim the end of theories of "theories of mind" as we know them. Hoorah! Social interaction wins the vote. Out goes individualism, in comes relationalism. Old-fashioned accounts of understanding mind – theory formation, introspectionism, enculturation, and modularity – are all dead. Long live constructivism! This view is good news for anyone who has ever questioned the primacy of a cognitive account to explain social reasoning. It is also good news for anyone who has ever questioned the lack of developmental emphasis in the study of children's understanding of mind. So C&L's theoretical reconstruction brings new hope to the theory-of-mind weary.

Now, the next question is how to pull it off. There is much work that still needs to be done. Below I take up three issues – the boundary problem, the question of end states, and the issue of a central role for triadic interaction. For each I suggest ways in which research on atypical populations might give some cautionary insights on how to proceed with the work ahead.

The first issue is the boundary problem – a common theme in the field. C&L aim to explain how children acquire "social understanding." This broad term refers to all manner of social knowledge, including knowledge of the mind. When it comes to detailing how knowledge is actually constructed within social interaction, wouldn't it help to be specific about the boundaries of that knowledge? Understanding the mind as a representational device may be a very different kind of knowledge than understanding of social rules, or the ability to talk about inner states and the role of social interaction may differ for each of type of knowledge. C&L seem to agree that different types of social interaction experience may relate differently to particular types of social knowledge when they refer to Peskin and Ardino's (2003) study. But if we want to try to understand better how social interactions actually work in helping to construct different types of knowledge, it may help to study an atypical group with impaired social interaction – children with autism. For example, if children with autism lack basic social relatedness, is it possible that they are ever able to fully grasp the representing function of a symbol or representation, even when such symbols or representations are removed from a social context?

A second problem is how to avoid the notion of "end states." In section 4, C&L make developmental links between children's early social interaction experiences and later social understanding. This makes social understanding look like an end state or developmental outcome. I do not think this is the intention. On the contrary, what C&L want to emphasise is "progressivity in development," the idea of a movement away from an initial starting point rather than a directionality towards a predetermined endpoint (Chapman 1988a). But given the way the account is currently formulated, it is difficult to get a sense of the nature of this progressivity. The process of development, as they point out, is a problem. Ideas about development being embedded in activity, regularity, social practice, or conversations and involving transactions between self and others have a long way to go. And we probably need to start at the beginning and look forward rather than trying to explain social understanding backwards.

The study of atypical development may offer some insights here. The way that development is modelled in neuroconstructivist accounts of atypical development (Thomas & Karmiloff-Smith 2002) is to examine how precursor states may be related to particular outcomes given different sets of constraints or circumstances. Although the idea of predetermined end-states might not appeal, connectionist dynamic systems and transactional accounts all argue that development is emergent, that influences are bidirectional rather than unidirectional, and that development involves a series of changes in a self-organising system. Capacities may disappear and then reappear later, may peak and then decline with time, and may start as general and become more specific. Organismic and systems theories also propose changes that include integration of earlier accomplishments as experiences and abilities are integrated into the subsequent reorganisation of the system.

A fuller account is still needed of the way in which developmental change proceeds over time from the starting state of social interaction. However, the idea that development involves an integration or reorganisation of earlier accomplishments is high-

lighted in C&L's discussion of the epistemic triangle. Triadic interaction is given a central role in this account as it incorporates and integrates more basic dyadic skills. The centrality of triadic interaction is consistent with a view of development in which one form of knowledge is considered more complete or more adequate than other forms of knowledge. But how central is the role of triadic interaction? Does it supplant other forms of activity and knowledge? Does it remain central throughout development? Our research in autism (Leekam & Ramsden 2003) indicates that while triadic interaction difficulties are critically important, dyadic interaction difficulties alone are a very powerful indicator of language and cognitive skills. Atypical groups, therefore, may give further insights into the continuity and discontinuity of link between dyadic and triadic skills. For example, children with autism have high levels of engagement with objects, whereas, in contrast, children with Williams syndrome have high levels of dyadic engagement with people and poor non-social knowledge. Both groups have difficulties with triadic interaction. What kind of understanding can be constructed from these different starting points and how is the ongoing development of this understanding constrained, facilitated, or transformed by experiences across time? C&L's account may not be able to answer these questions yet but it provides a good starting point for constructing a new understanding of children's understanding of mind.

Rich interactions and poor theories

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Abstract: Carpendale & Lewis's (C&L's) critique of traditional accounts of "theory of mind" is well taken, but the alternative theory they propose is premature at its best, unconvincing at its worst. The proposed theory is ad hoc and confirmatory in its findings; vague and generic in its claims; and unjustified and unnecessary in its (putative) novelty.

Critic John Horgan (1999) recently commented discouragingly about progress in psychology: "Theories of human nature never really die; they just go in and out of fashion" (pp. 6–7).

I am extremely sympathetic to Carpendale & Lewis' (C&L's) paper because it presents a scholarly review of the literature on children's understanding of the mind. However, the authors' alternative theory seems so ad hoc and vague that it conforms to Horgan's critique.

Ad hoc theory. When a new theory is proposed, one expects that its authors will contrast the theory's ability to deal with empirical findings against the ability of alternative theories. When this is not the case, the findings invoked to support the new theory are not critical, but *confirmatory.* In the present case, the findings invoked by C&L are consistent not only with the authors' new theory, but also with the approaches they criticize, and even with theories the authors do not mention. For example, if the "sibling effect" (with all its qualifications) found in children's social understanding may be interpreted in terms of C&L's account, it may be interpreted also in terms of (1) the theory-theory view, which states that theories are revised because, among other things, "children . . . depend on a social world . . . for much of the information they will use in theory construction" (Gopnik & Meltzoff 1998, p. 19); (2) the simulation theory, which states that interactions among children help them understand their own and others' mental states (Harris 1991); (3) the enculturation perspective, which claims that "children internalize the folk psychology of their particular culture" (Astington & Olson 1995, p. 184); (4) the experiential approach, which "takes into account both the conditions of experience in the specific social and cultural world, and also the phenomenology of experiencing on the part of the child" (Nelson et al. 2003, p. 25); and (5) even modular theories for which "there is nothing in the notion of modularity that prevents even matured modules from learning and developing" (Scholl & Leslie 2001, p. 698). More generally, when findings are interpreted *a posteriori* they can be made to accord with almost any theory. Hence, C&L's theory reminds us more of an approach to which data have to conform than one that conforms to the data.

Vague theory. C&L's main proposition is that the development of children's social understanding occurs within a triadic interaction involving the child's experience of the world as well as communicative interaction with others about their experience and beliefs (i.e., Chapman's [1991] epistemic triangle). Although true and of some heuristic value, this proposition is inherently vague. Without further qualitative or, better still, quantitative specifications, the proposition can be used, as is often the case throughout the paper, to make almost any claim, to generate almost any prediction, to explain almost any finding, to describe almost any psychological process, and to be applied to almost any developmental phenomenon. For example, it is hardly new – perhaps it is plain common sense – that the psychological development of children involves an activity matrix made up of biological, social-cultural, and psychological dimensions. In the same vein, to expect that children do better on false belief tasks when they are actively involved; to say that conversation about the mental world may be essential for the development of social understanding; and to declare that the extent and nature of social interactions experienced by children facilitates their development of social knowledge, hardly raises above common sense. What seems to be the greatness of C&L's theory – its apparent consistency with countless findings, authors, and theories – may be the hallmark of its fragility, for only at the cost of remaining at a generic level can the theory accord with almost anything psychological.

Consider the following example. C&L maintain that although Thelen and Smith's dynamic systems approach is in many ways consistent with their account, the ideas of interactionism and transactionalism make their account distinctly different. For, in contrast with traditional children's "theories of mind," C&L's account integrates the social and individual dimensions of development. However, because the two key concepts of interactionism and transactionalism were not specified, the authors' claim would certainly be classified as vacuous by Thelen and Smith: "Interactionism and transactionalism are everyone's comfortable buzzwords, and the proffered 'solution' to the nature-nurture dichotomy [and many others]" (1994, p. xv).

Unnecessary theory. C&L's account boils down to the idea that children's social understanding occurs within social interaction, is gradual, and involves an active subject. But when these claims remain at a simply verbal level they will not be powerful enough to sustain an alternative theory of individuals' social understanding, regardless of how flawed previous accounts may be. Without additional specifications and elaborations, Piaget's appeal to constructivism and gradualism and Vygotsky's focus on social interaction do not need to be repeated. In addition, they do not justify yet another theory on children's social understanding. Because all subjects behave and while behaving must perforce be active, we should be careful when proposing theories whose essence relies on the contrast between an active and a passive individual. What counts as a subject's passive or active role in his or her psychological functioning may be more a semantic problem – all too common in disputes among weak psychological theories – than a substantial problem (see Lourenço 2001).

Regarding the necessity of C&L's account, I believe it does not go any further in terms of claims, processes, or predictions than previous developmental theories, such as those of Piaget, Vygotsky, or Selman. For example, because Selman's (1980) theory identifies five levels in individuals' interpersonal understanding, and also appeals to the idea of an active and interactive child, it goes well beyond C&L's seemingly distinctive idea of the emergence of an interpretive theory of mind, which according to these authors would be the second (and last) level in individuals' understanding of the mind.

It might be argued that even if C&L's approach were reducible

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