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AUTHOR'S RESPONSE

The Mindreading System: New directions for research

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GENERAL REMARKS

I am grateful to all of the commentators for their scholarly responses to this target article. These have had exactly the effect I hoped for: generating new debate, even some new data, and encouraging revision of my model in various ways. Before I respond in detail to each commentary individually (alphabetically below), let me make a few general remarks:

1. The model of the Mindreading System is intended to be a first sketch only, open to refutation, revision, and extension. It was proposed in order to fill a gap in this field. Some commentators question the precise input specifications for some of the components in the Mindreading System. Such questions may be premature in a field where the relevant studies are as yet still few and far between. My hope is that the sketch I have put forward might stimulate some of the relevant experiments to be done.

2. Some commentators devote considerable space to discussing if each component mechanism in the Mindreading System "really is a module". Clearly, some mechanisms fit all the strong criteria of being a 'Fodor-module', whilst others only fit *some* of these criteria for

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modularity. How many criteria does a mechanism have to possess before we will call it a module? Seven or more out of nine? These definitions are arbitrary. Those who insist on there being only one kind of modularity (Fodorian, with all 9 criteria being met) are I think being somewhat orthodox on this issue. They may suggest I should simply use the term 'neurocognitive mechanism' whenever I discuss a system that fails to meet *all* 9 criteria. I used the term 'module' in the sense used by those who recognize *varieties of modularity* (e.g., Segal, this issue). 'Neurocognitive mechanism' would suffice too.

3. Some commentators suggest that there should be additional mechanisms in the Mindreading System: e.g., a mechanism for recognizing facial expressions of emotion, or a mechanism for computing central coherence. I readily acknowledge that such components could be added to the Mindreading System, though they were not included originally because they are not unique to this system. Thus, a mechanism for recognizing facial expressions of emotion has been outlined in the face-processing system (Bruce & Young, 1986). And a mechanism for computing central coherence presumably operates over all of cognition, language, and perception (Frith, 1989; Sperber & Wilson, 1986). Such additional mechanisms clearly interact with the components of the Mindreading System.

4. Some commentaries fall into different categories, and it is worth making these explicit: those in support of the notion of a specialized "social brain"; those opposed to this; those in support of identifying further innate mechanisms in social perception; and those opposed to this. Recognizing these inherent differences in view may help the reader clarify some of the key dimensions within these debates.

5. I cannot possibly respond to all of the interesting points made in the commentaries, but below I pick up a good number of them. I have rephrased these points as questions (in italics, below), to which I try to respond.

REPLIES TO INDIVIDUAL COMMENTARIES

Baldwin and Moses

1. *Is there sufficient evidence that all 4 mechanisms are biological modules?* Clearly, we need far more evidence to test the claim. Baldwin and Moses are right that the evidence is thinnest for ID, though the new studies from Premack's and Gergely's groups are filling this gap.

2. Does SAM have a normatively based characteristic ontogeny? Baldwin and Moses are concerned that little evidence exists for the age of onset of SAM. It may therefore be of interest to note that we have recently completed a screening of 16,000 toddlers (all aged about 18 months old) in the southeast of England, to assess age of onset of joint attention and pretence (Baron-Cohen, Cox, et al., 1994). We found that more than 90% of toddlers at this age show both behaviours. In the target article here, I suggested the timewindow for the onset of SAM is 9-14 months (not 6 months, as Baldwin and Moses state). This timewindow is entirely consistent with the Corkum and Moore (in press) data.

3. If ToMM shows variability in age of onset, doesn't this imply it can't be a biological system? I think this argument contains a flaw. Many biological systems show variability (e.g., the development of language, walking, and teeth). Variability in size or timing of development does not imply that the underlying system is non-biological. Indeed, natural selection only operates because there is variability within biological structures and processes.

4. If EDD's first function (detecting eyes) is present in neonates, whilst EDD's second function (detecting gaze direction) is not present until later in infancy (3 month olds: see Muir et al., below), doesn't this undercut the idea of a single mechanism underlying both functions? This could actually be taken as evidence for very rapid development of a single system! Positing a single mechanism does not imply that there is no development within that system. (Language is a good example.) However, it may be that EDD's two functions will ultimately fractionate into two separate systems. For the time being, I make the assumption that they are rather two functions of the same system.

5. Are single cell studies evidence for biological modularity? Clearly, by themselves they are not sufficient evidence, but they are certainly relevant evidence. However, to demonstrate biological modularity one needs evidence from lesion studies, single cell studies, neuroimaging studies, etc. We still lack many of these, though I suggest the case is currently strongest for EDD.

6. Do we not need evidence from double dissociations? This type of data would be the strongest sort, but given that the postulated components in the Mindreading System are developmental, it may not be possible for a person to have an intact ToMM but an impaired SAM. Indeed, it may not even be appropriate to ask for such evidence when mechanisms stand in a developmental relationship with one another.

Finding patients with an intact EDD but an impaired ID should however be possible.

7. Couldn't the components of the Mindreading- System be constructed through general learning processes? In principle, this is of course an option. However, a general learning account fails to explain why most children with autism do not learn to mindread, or share attention, etc., when many of them clearly learn many other skills (such as riding bicycles, naming the stars and planets, or knowing the times of trains of British inter-city trains). I do not doubt that experience and learning play a role in shaping mindreading skills; I simply wish to bring out the biological basis of these skills, tragically revealed to us in autism.

An analogy with language ability may be helpful. It is clear that many environmental factors play a role in shaping the rate of language development, but this in no way rules out that language is an "instinct", as Pinker (1994) calls it. Children with Specific Language Impairment (SLI) – a genetic disorder (Bishop et al., in press) – show the biological basis of language as clearly as children with autism show the biological basis of mindreading.

8. Is the model of the Mindreading System falsifiable? Many commentators mention that the clear claims in the model render it very testable and falsifiable. "ToMM requires SAM" is one such falsifiable claim. Baldwin and Moses worry that the weak (i.e., non-Fodorian) version of modularity I use may not be falsifiable, but I think this misses the point. As mentioned earlier, the definition of a module is arbitrary (9 criteria, 6 criteria, etc.). It is important to test any given neurocognitive mechanism against the set of criteria, and thereby establish the degree or type of modularity. Some modules will then meet all 9 criteria, others will meet only 6, etc. This may give us a principled way of distinguishing one class of module from another.

Brothers

1. Might ID be part of the cognitive architecture of many primates, not just humans? I think this is a fair assumption. When we have good tests of ID in humans, we will hopefully be able to apply them to a range of other animals.

2. Might pretence play a special role in human development, in allowing the child to assume different roles within a given culture? The idea that pretence allows one not only to play with one's attitudes towards objects but also to play with one's attitudes towards proposi-

tions and social roles, reminds us of the power of this system in development. I would agree with Brothers that, as currently construed, the Mindreading System implies that ToMM and pretence/imagination are one undifferentiated component. A task for the future must be to specify their separate functions.

Campbell

1. Should we give up the criterion of informational encapsulation for modules – even non-Fodorian modules – so quickly? I am persuaded that mechanisms should indeed be tested for this property. (See also my reply to Heyes and German, Segal, and others, below.)

2. Why should triadic representations be so hard for children with autism to build? Is this a primary deficit, or a manifestation of their deficits in central coherence (Frith, 1989)? And what in normal development allows a child to be able to bolt or fuse two dyadic representations together in such a way as to build a triadic representation? Campbell's questions are crucial for understanding both the deficit in autism, and normal development. I look forward to seeing experiments that attempt to answer them.

Charman

1. Doesn't Piaget's 3 Mountains Task involve triadic representation? Charman is right that this task *could* involve triadic representation, but the point is that the task can also be passed using the simpler dyadic representation [Doll-sees-the church]. This means that a child with an impaired SAM, but who had EDD intact, could still pass. This appears to mirror what is happening in autism. They can use EDD to pass not only visual perspective taking tasks, but also the explicit task "Which sweet does Charlie see?". Unlike joint attention, these tasks can be passed using purely dyadic representation, if this is all you have available.

2. Is SAM independent of ToMM, or simply an earlier manifestation of it? This is an important question. I do not rule out the latter possibility. We need to develop ways of testing between these two alternatives.

3. Is affect a crucial part of joint attention? I have no doubt that it is. The problem here is that (a) affect is part of many systems (attachment, play, aggression, etc.), so it is not clear what specific and necessary role it plays in joint attention; (b) many children with autism clearly experience basic affective states even though they don't appear to experience

joint attention. We therefore need a theory of the specific role of emotion in the Mindreading System.

4. Doesn't a purely cognitive characterization of mindreading predict that children might ascribe mental states to non-human agents such as animals, or even to apparent agents, such as machines? This is correct, but for me this is a strength of the design of the Mindreading System. It needs to have a flexible feature like this in order to account for our apparently ubiquitous use of mindreading. It may be largely used in our interactions with humans, but it is not restricted to such cases. Dennett (1978) gives clear examples of how we readily ascribe mental states to cats and mice, computers and thermostats, as well as to each other.

5. What is the evidence for the two subgroups of autism (A = impaired in SAM and ToMM, B = intact SAM but impaired ToMM)? Clearly, most cases of autism are likely to be Category A type. We need more evidence relevant to Category B: apparent cases of 'late onset autism' (at 18 months of age) may be just such instances.

de Gelder and van Baaren

1. Since children with autism can answer test questions, isn't this evidence that they have ToMM? Taking a conservative approach, it needs not be (see Baron-Cohen, 1988). A standard autobank machine can ask questions ("Do you want to withdraw money? Y or N?") and answer them (e.g., "You are overdrawn by £206"), but it clearly has no awareness of the mind of the person with whom it is interacting.

2. If children with autism have an impairment in gaze detection (i.e., in EDD), might this not confound whether they can read gaze in volitional/mentalistic terms (i.e., whether they can use SAM? This certainly would risk confounding the deficit. However, in our study (Baron-Cohen, Campbell, et al., in press), subjects were not impaired in EDD, but only in SAM. The lack of impairment in EDD is consistent with other studies (Hobson, 1984; Baron-Cohen, 1989a; Tan & Harris, 1989). That Gepner et al. (submitted) found a deficit in EDD in their sample of children with autism is interesting – in some groups there may be deficits at this primitive level – but it is clearly not found in most samples with autism.

3. Is SAM impaired in autism? de Gelder and van Baaren appear to have misunderstood the section on SAM and autism: they interpret it as saying that SAM is not impaired, despite the large number of studies suggesting it is.

4. When EDD represents [Mummy-sees-me], should the representation really be [I-know-Mummy-sees-me]? The claim that all this is entailed when EDD fires without ToMM seems to me to be an unnecessarily rich interpretation.

5. In what sense is the term 'intentionality' being used? de Gelder and van Baaren are right that different senses of the term are used in relation to the different proposed mechanisms. Thus, the first 3 mechanisms code intentional terms that are 'transparent', whilst ToMM codes "fully intentional" terms that are 'opaque'.

6. Can ID and EDD represent misprepresentation? As currently defined, without ToMM they cannot. This leads to a strong claim: Children who lack ToMM should be unable to understand mistaken beliefs. The results from many false belief experiments are consistent with this claim.

7. Isn't there a better alternative to the Mindreading System? If there is, de Gelder and van Baaren have not yet provided it. Suggesting we could instead talk about "the ability to entertain concepts" or "the ability to provide rational explanations of behaviour" does not appear to have much explanatory power.

8. Isn't mindreading just social skill? Mindreading is undoubtedly an important subset of social skill. However, mindreading and social skill are clearly not the same thing, since some social skill involves no mindreading ability. Gender recognition and relationship perception are two such examples, both of which are relatively intact in autism (Baron-Cohen, 1991c). This suggests that mindreading is distinct, and can be *selectively* impaired.

Gergely and Csibra

1. Why include stimuli with direction as potential input to ID? Surely a static tree-branch (which has direction) is not interpreted as goal-directed? I concede this is unlikely, though a tree-branch moving in the wind might be so misconstrued. These would seem to be purely empirical questions: Is directionality sufficient for an intentional interpretation, or does ID require directionality *plus* apparent self-propelled motion to fire? We await relevant data.

2. Does ID "flood the central learning system with a continuous flow of false positive instances of volitionally interpreted stimulus events"? This is not yet known. For all we know, ID might overextend in this way. Most of us as children have been scared by what turned out to be a shadow, not a creature lurking by the bedroom door. Certainly, this

overextension would not be incompatible with ID having evolved via natural selection, since overresponsivity (interpreting an object as intentional when it is not) might be more adaptive than underresponsivity (not interpreting an object as intentional when it is). The cost of treating something as non-agentive is that you might miss a potential predator! We certainly see EDD clearly overresponding – for example, treating eye-spots or real eyes as alike, in some circumstances – presumably again, for reasons of evolutionary adaptation.

3. Would mother's hand stroking the infant up and down be interpreted by ID as [Mum-wants-my head] and [Mum-wants-my bottom]? This amusing suggestion would only be correct if ID could not solve the 'frame-problem' – a problem that is general to all domains of cognition. (The frame-problem arises in explaining how an intelligent system identifies the scope of a problem to be solved: does it identify each local feature as the problem, or some larger set?) I think the frame-problem requires a separate explanation – for example, does ID interact with a 'relevance-calculator'? However this is achieved, this would result in interpreting mother's hand stroking the infant as [Mum-wants-to stroke me].

4. Do Gergely et al.'s (in press) data refute the notion of an ID? I think not. Their important experiment shows that 9-12 month olds discriminate stimuli with self-propulsion and directionality according to a "rationality" feature, when this is present. But this does not imply that ID requires a rationality feature. Self-propulsion plus directionality may be sufficient for ID to compute simple volition.

5. Should dyadic and triadic representations have a propositional structure in the object slot? I readily concede this, and suggest dyadic representations be modified as [Agent/Self-Relation-"Proposition"]. And in the case of triadic representations, as [Agent/Self-Relation-(Agent/Self-Relation-"Proposition")]. (Segal asks the same question – see my reply to him, below). This modification does not, as I see it, have any bearing on whether dyadic representations are unique to ID; they clearly appear in both ID and EDD. However, non-intentional dyadic representations may well be represented independently to intentional dyadic representations, and cases of neuropsychological dissociation between these would be important evidence for this.

6. Would "a person wandering pensively around a wood" be interpreted by ID as "the person 'wants the tree"-that at any given moment happens to be aligned with the direction of his/her movement"? Again, this amusing view of what ID computes might correspond to how ID would make sense of this situation if it was operating entirely in isola-

tion from the rest of cognition. But then, what kind of cognitive system would have ID operating purely on its own? Maybe a very young infant's? Suffice it to say, Gergely and Csibra's query appears to be really about the frame-problem again. In the mature mindreader, ID's connection to ToMM would presumably produce the mature interpretation [Agent-wants-to get out of the wood]. This is because ToMM would be computing the Agent's wider plans and thoughts, and not just the immediate goal of each action. And again, maybe the Mindreading System interacts with a relevance-calculator, independent of the Mindreading System. Gergely and Csibra's extreme scenario is a useful prompt to question how this is achieved.

7. Does an Agency Detector (akin to ToBy [Leslie, 1994]) operate prior to ID? At this stage, I see no empirical grounds (either from ontogenetic or neuropsychological evidence) for separating perception of agency from interpretation of volitional action. If this is found, then by all means we should fractionate ID further.

Gómez

1. When the guy on the tube (whose newspaper you have been surreptitiously reading over his shoulder) spots you, are triadic representations sufficient to describe your detection of this? Gómez suggests we need a more complex representation, such as "I see that he sees me seeing that he is looking at the newspaper". My own view is that this is unnecessarily embedded. Indeed, why should Gómez stop at this number of embeddings? Why not just keep adding further levels of embedding, by adding an additional prefix of "I see..."? Clearly, this would rapidly run into an impossibly complex information-processing task, and runs the risk of an infinite regress. Moreover, I think it is not required for *adaptive* processing of social situations. My solution is to make dyadic and triadic representations encode *unidirectional* or *bidirectional* relation terms. That seems enough to mark the distinction of whether the guy on the tube simply spots you reading his paper, or spots you seeing you have been spotted. The same applies to your distinction of these two situations.

2. Is bidirectionality automatically computed when [Self-sees-Agent]? The relation term is bidirectional only when the other Agent is at the time also perceiving (or standing in some other relation to) the Self. Gómez is right that when the relation term is unidirectional, this identifies the situation as involving an Agent currently as an object, rather than as an Intentional Agent. Gómez suggests all dyadic representations

involving two Agents are potentially bidirectional. This is precisely what I intended to convey in allowing the relation slot to vary in its directionality. Thus, if at time 1, the representation shows

[Self—sees—> Agent],

and at time 2 the representation shows

[Self <—sees—> Agent],

the minor addition of another arrow head has specified that the Agent has now spotted s/he was being looked at.

3. Is there a need for a change in the notation of the sentence-like format of dyadic and triadic representations? Gómez suggests the inclusion of the arrow heads, or a new order of elements in such representations. I do not see that these modify the theory or its predictions.

4. Do non-human primates have a SAM? Gómez' new data suggests they may, but see Povinelli and Eddy (below).

Happé

1. Is there any joint attention before 9-14 months? That is, can SAM receive input from EDD and/or ID before 9-14 months? Clearly, there are going to be individual differences in this. Just as some children start to walk at 10 months, and others not until 18 months, so SAM may be expected to show some variability in terms of its earliest age of functioning. In my article I suggest the broad time-window for this is 9-14 months, though we should expect there may be individuals who are even more precocious in this respect.

2. What do the arrows in the model represent? These are intended to represent both information flow *and* developmental contingencies. They are not however meant to imply that ToMM can only receive input from SAM, since it is likely that it has a wide variety of inputs.

3. Does SAM import volitional terms from ID only during a finite, critical period in development? This is unlikely. It appears to happen from toddlerhood onward, throughout the lifespan. However, there may be a critical or sensitive period for SAM to activate ToMM. This needs to be investigated.

4. Once activated, does ToMM continue to require SAM? This is a very good question. ToMM probably does not continue to require SAM, but the empirical test of this would be cases of acquired damage to SAM – do such patients retain ToMM, or inevitably lose ToMM too?

5. Does ID fire for accidental actions? Stimulus events can only be interpreted by ID in very simple terms: desire, and goal. ID has nothing within it to mark the accidental-deliberate distinction. To make this

latter distinction would require the concept of intention, not processed by ID but by ToMM. Recent evidence by Phillips (1993) suggests this distinction is not well understood until at least 4 years of age, in the normal case, and that children with autism (with a mental age in excess of 4) have severe difficulty in understanding it.

6. Can ID discriminate different desires, e.g., the desire to reach versus the desire to push? I would have thought that these stimulus events are perceptually discriminable, in that reaching implies no contact has yet been made between the hand and the object (and would be represented as [Agent-has goal-reach object]), whilst pushing implies contact has been made (and would be represented as [Agent-has goal-push object]). However, could ID discriminate these prior to contact between the Agent and the object being made? Premack (1993) suggests that ID may be able to distinguish actions by their "power": for example, a caress and a shove have markedly different psychophysical properties. This would seem to be an empirical question.

7. Can eye-movements trigger ID? This is another excellent question. My claim is that in infancy eye-movements are only coded by EDD. This is because, as defined, EDD is a system which 'sits there waiting' for eye-like stimuli to process. Any such stimuli will be 'grabbed' by EDD. For eye-movements to trigger ID to fire, the eyes would need to be clearly moving from the head in which they are housed. Since such events don't happen — eyes, head, and body move together towards an object —, ID would process the head and body movement in terms of an Agent's goal.

8. How does EDD code the Self? See my response (below) to Moore on this.

9. Is eye-contact treated as an ostensive act? In my model it is, but only by SAM, not by EDD. Since most children with autism are impaired in SAM, they often miss the ostensive significance of eye-contact.

10. How does EDD resolve the frame-problem? That is, how does EDD compute which of 4 objects in the other Agent's line of regard is the object being looked at? As noted earlier (see my reply to Gergely and Csibra, above), the frame-problem rears its head in every domain of cognition, and a relevance-calculator, or a coherence device, would clearly be useful and necessary. This is not part of EDD, as Happé notes, but must be part of central cognitive processes.

11. Can SAM represent [Mum-touches-(I-touch-the cup)]? I have suggested just this (see page 532, this issue, on joint tactile attention; or Baron-Cohen & Ring, 1994, p. 191). *Out of what can SAM build such*

triadic representations? In my model, this could be built from ID's representation [Mum-has goal-touch the cup]. (But see my reply to Perrett and Emery, below.)

12. Might there be aspects of EDD that are impaired in autism, and aspects of SAM that are unimpaired in autism? These are good, testable questions.

13. How does SAM compute reference? Here, I take reference to be a special instance of goal (to refer), and therefore computable by ID and SAM. However, I do not consider reference to be an intention, in the complex sense. If it were, it would require ToMM.

14. In the Phillips et al. (1992) task, when the child makes eye-contact with the adult immediately following the adult's ambiguous action, what representation is being built? Happé suggests that under these conditions, the child makes eye-contact because this is a way of checking "mutual knowledge" and "intention to communicate". The latter is made manifest through eye-contact as an ostensive act. I think Happé overstates this event, however. There is no evidence that infants of 9-14 months of age can represent their own or another Agent's *knowledge state*, let alone mutual knowledge.

However, checking eye-contact as a signal of the adult's goal is exactly what I argue SAM can do. Moreover, an attempt to explain the child's search for eye-contact solely as a check for ostensive acts fails to account for why the child should search the adult's *eyes*, as opposed to the adult's hands, since hands can also convey ostensive signals. The search for eye-contact, I contend, allows the child to distinguish if s/he is the goal to which the adult is attending, or if the adult is attending to a different goal at the time the action was performed.

15. Isn't facial expression required to distinguish a 'tease' from a 'give'? I agree with this. Clearly, information about facial expression (from the face-processing system [Bruce & Young, 1986]) must interact with the Mindreading System to aid discrimination of such goals.

Heyes and German

1. Surely intermodular interaction does not preclude informational encapsulation? I accept this point. (See my responses to Campbell, and Segal.)

2. Don't triadic representations such as [Self-sees-(Agent-sees-the bus)] involve implicit inferences such as [Self-sees-the bus]? I had hoped that such inferences were expressed in the sentence-like format of triadic representations: namely, that the Self sees everything in the embedded

(bracketed) clause.

3. Can we justifiably ascribe a concept of 'seeing' to an animal (like a snake) who shows sensitivity to the eye-direction of another animal (like an experimenter)? I agree with Heyes and German that this is too rich an interpretation. That is why I suggested that EDD has two functions: (i) to identify the presence of eye-like stimuli (especially those aligned with the Self); and (ii) to code eyes as "seeing". Evidence for this second function is hard to obtain from non-verbal animals, and this is probably true for the ascription of "looking" or "seeing". My reason for including this second function of EDD was to resolve the paradox that most children with autism can identify what someone is looking at (when they are explicitly instructed to do this), and yet show little spontaneous joint attention. In Leekam et al.'s (1993) terms, they can compute the geometry of gaze but seem to fail to share attention with others.

4. Shouldn't we be cautious about EDD's second function? Heyes and German identify an important problem: in a non-verbal organism, it may be impossible to prove that in reading another's eyes, the animal ever ascribes 'seeing' to the other individual. And yet as soon as human infants have the words to tell us what they know, they tell us that when Mum covers her eyes, *she can't see*! Heyes and German may remain sceptical until such linguistic evidence is available, but this may attach undue importance to language.

5. Could associative learning explain joint attention? I think not, since this would hardly explain why children with autism fail to show joint attention when they are perfectly capable of associative learning. Moore and Corkum's (in press) data can also be interpreted as showing that spontaneous joint attention is radically different to instrumental conditioning.

6. What about the empirical issue of head versus eye-direction? Let me emphasize that these are not mutually exclusive. When another individual's head turns, ID processes self-propulsion and directionality cues, and computes that individual's goal. ID therefore processes head direction and postural information. When that individual's eyes move, EDD processes a change in that Agent's visual attentional state. Perrett and his colleagues have shown that, as one would expect, neural systems are using both head and eye-direction, and where these contradict each other, eye-direction takes precedence over head direction. Presumably, this is because eye-direction is more important – hence the need for EDD. But in most situations, EDD and ID work together, not in any disconnected fashion.

Jacob

1. Would GD (Goal Detector) be a better name for ID? In some ways it would, since this would avoid the confusion that 'complex' intentions are processed by ID when in fact they are processed by ToMM. I used the term "intentionality" in ID to be a broad term indicating aboutness, and then defined ID's function more specifically to represent goal, and simple desire. Defined in this way, I think the term ID is appropriate, and as good as any.

2. Can ID really be amodal? More specifically, why would an illusion of intentionality be like in a non-visual modality? When I say that ID is amodal, I mean it can operate in the visual modality or in other modalities. This idea was intended to include Premack's (1990) specific case of the visual illusion of goal directedness from moving dots, and other kinds of (non-visual) input. Presumably a tactile equivalent of illusory goal detection could be created by touching a person using a series of touches. If the series of touches had apparent direction and self-propulsion (like someone 'walking' their fingers up your leg), ID would code this as an Agent with a goal. The illusion would depend on just this type of sensation being produced by a non-Agent (e.g., a robot). This would make it equivalent to the moving dots on the screen. Similarly, an auditory equivalent of the illusion of goal detection could be created by producing a series of computer-generated 'calls', with apparent direction and self-propulsion. Clearly, the relevant experiments need to be done.

3. How do ID and EDD represent the Self? See my reply to Moore (below).

4. Is SAM necessary to integrate ID and EDD? One might expect that the 'Charlie and the 4 sweets' task could be solved by ID and EDD together ("Charlie sees x" and "Charlie wants x"). A simple arrow between these two boxes in the model would allow for this. However, I have deliberately made SAM the first point of integration for ID and EDD, for reasons of parsimony: most children with autism not only fail to establish shared attention, but also fail the 'Charlie and the 4 Sweets' task. It seems economical to assume that an impairment in a single mechanism (SAM) may be responsible for both. In this case, the model is constrained by the neuropsychological evidence, rather than by an a priori analysis.

Jacques and Zelazo

1. Doesn't modularity lead to a "multiplication of theoretical entities"? This is indeed a feature of modular theories. However, in most biological sciences this is seen to be quite ordinary (the body has dozens of modules, if one sees organs in this way). Psychology should not simply assume that a general purpose learning mechanism is going to be all that there is in the brain/mind.

2. Might ToMM depend on domain general psychological functions, such as the use of higher-order rules or the ability to disengage from attractive stimuli? ToMM may well involve the use of such domain general functions, but so do many other tasks which many children with autism pass, even whilst they fail tasks tapping ToMM. The clearest example is their good performance on false photograph tasks (Leslie & Thaiss, 1992). More recently, Scott (1995) has reported that children with autism are unimpaired on a range of logical reasoning tasks (including analogical, counterfactual, syllogistic, and transitive inferential reasoning). These are likely to involve higher-order rules. Such findings are consistent with their ToMM deficit being domain-specific.

3. Is theory of mind impaired only in autism, and not in other clinical groups? Jacques and Zelazo cite one study of ToMM deficits in children with mental handicap (Benson et al., 1993), which stands in stark contrast to the dozens of independent studies reported over the last 10 years showing that children with mental handicap tend to pass ToMM tasks. Why one sample performed worse could be due to a large number of factors, but the positive evidence from all of the other studies suggests the deficit in ToMM is specific to autism, relative to mentally handicapped controls. The other study they cite is an unpublished manuscript (Zelazo et al., 1994), so I cannot comment on that yet. Suffice it to say that the bulk of the evidence suggests that given an adequate mental age (of about 4 years or above), most children without autism can pass tests of ToMM.

4. Since orbito-frontal cortex (OFC) plays a role in executive function, doesn't this imply that it can't be uniquely dedicated to ToMM? This is quite correct. Note though that I do not claim that OFC is the neural basis of ToMM, since such a claim would be premature. Rather, on the basis of one neuroimaging study (Baron-Cohen, Ring, et al., 1994), OFC appears to be *implicated* in ToMM, and we suggest this may be just one part of a neural *circuit* for ToMM (Baron-Cohen & Ring, 1994). OFC may also be involved in executive function, though the latter is usually associated with dorsolateral prefrontal cortex (DFC,

see Baron-Cohen & Ring, 1994, for a review).

5. If children with autism have a deficit in ToMM, doesn't this imply that they should have no deficits in unrelated domains? This does not follow. For example, if autism is due to multiple (independent) deficits, they may have a deficit in ToMM and in other domains (e.g., executive function).

6. Could deficits in autism in ToMM be a bi-product of deficits in executive function? This is in principle possible, but there are several problems with this view: (i) Many clinical populations (ADHD, OCD, schizophrenia, mental handicap, Tourette's Syndrome, anxiety disorders, treated PKU, etc.) have deficits in executive function without showing apparent deficits in ToMM, which implies that executive function cannot be necessary for ToMM (see Baron-Cohen & Moriarty, in press, for a review of this evidence); (ii) The studies in autism show a *correlation* of executive function and ToMM deficits, but this does not imply that one is therefore causal of the other; and (iii) the finding of executive function deficits in *so many* widely differing clinical populations raises questions about the value of the construct, and what it is measuring. Is it simply a general indicator of any kind of pathology? (Planning skills after all could be interfered with by anything which causes distraction).

7. If autism were a case of mindblindness, surely children with autism should not have any symptoms unrelated to mindblindness? Nowhere have I or my colleagues argued that *all* the symptoms in autism can be explained by mindblindess – only those in the domains of social interaction, communication, and pretence. But there are many other symptoms outside of this "triad", such as oversensitivity to sounds, perseveration, self-stimulation, etc., which the mindblindness account does not attempt to explain. These are assumed to arise as a result of other abnormalities. Many of these are also not specific to autism but are seen in other handicapping conditions.

Johnson

1. Since EDD is not active until 3-4 months of age, does this not imply that it is not innate? This argument does not follow. Plenty of characteristics are not present at birth, and yet are innate. (One example is human secondary sexual characteristics.) The extensive evidence that EDD is active in many different species suggests a strong innate basis to EDD in humans.

2. Surely characteristic ontogenesis is not "diagnostic of modularity" by itself? I agree with this, but then nobody suggested that it was. Combined with many *other* sources of evidence, it may well be one important indicator of modularity.

3. Since the cells in the macaque STS are responsive for head-direction, faces, hands, and eye-direction, does this not imply that EDD is not modular? I don't think so. First, the STS is a large anatomical area, which is likely to contain functional specialization. Secondly, evidence of any neurological dissociation would be strong evidence for EDD's modularity. Johnson claims that patients *never* lose EDD without also losing other aspects of visual recognition. Note though that *just one case* of a subject who selectively loses EDD and no other aspect of visual recognition would refute Johnson's strong claim. However, my own claim was that EDD does not employ a unique class of representations – dyadic representations are shared with ID – so EDD may be a module in a weaker sense than, say, SAM.

4. Since EDD is subject to context effects, doesn't this imply that EDD is not modular? This argument only holds if a criterion of strong informational encapsulation is used. Remove this criterion (e.g., if you grant what Heyes and German, and Segal, call "intermodular interaction"), and the argument becomes irrelevant. Note that Perrett et al. (1992) have presented data similar to Vecera and Gildes (1994), showing that head-angle influences perception of eye-direction. When these are concordant, head-angle is used to infer attention. (In my model, this would be processed by ID.) When these are *divergent*, eye-direction takes precedence over head-angle. (This would be an example of EDD taking over from ID.) This makes good evolutionary sense, since eye-direction is a more reliable indicator of attentional state than is head-angle. (See comments on Heyes and German, above, too.) This shows how brain systems interact under some conditions, and can work independently under others – EDD can inhibit head-direction detector cells.

Vecera and Gildes (1994) demonstrate that under cartoon conditions, one can produce an illusion that eye-direction changed when it didn't (see Figure 1 in Johnson's commentary, above). However, with real faces, my prediction is that we do not judge that a face is no longer looking at us just because the nose moved. On the contrary, just think of a person looking at you, whilst she shakes her head disapprovingly or in disagreement, from left to right and back again. You don't suffer a strange illusion that each time her head moves, her eyes switch away from you! Vecera and Gildes cartoon method may therefore be a misleading analog of real faces.

5. Isn't it possible that ID and EDD are not functionally distinct, but are both simply products of a learning system which picks up a variety of cues relevant to perceiving conspecifics? I am persuaded that at a broad level of description there is a system which directs attention to conspecifics. But to which aspects? And why to their eyes preferentially? Without EDD, Johnson and Morton's CONLERN learning device might end up producing a baby that prefers attending to feet, or hands, or knees! Yet curiously, eye-contact seems to be a universal human bias in face-processing. Moreover, how is a CONLERN mechanism to account for differential physiological arousal to eye-contact? Finally, if ID and EDD are not functionally distinct, how can patient AB (Campbell et al., 1990) lose EDD but still be sensitive to goal directedness (as she presumably is)?

6. Can't deficits in SAM and ToMM in autism be explained by higher-order deficits in a "general invariance extraction learning mechanism", such as CONLERN? A prediction from this model is that performance on tasks of equivalent complexity *that require learning* should also be impaired in autism. To my mind, the remarkably good performance on understanding the representational nature of photographs (Leekam & Perner, 1991; Leslie & Thaiss, 1992) and drawings (Charman & Baron-Cohen, 1992) shows impressive general invariance extraction from learning mechanisms in autism, and within the domain of conspecific behaviour there is evidence of other similar intact knowledge (e.g., the animate-inanimate distinction; and relationship perception [Baron-Cohen, 1991c]). A theory of impairment in a general learning invariance mechanism therefore just cannot account for the autism data.

Leekam

1. Can't social relationships explain development, without reference to neurocognitive mechanisms or their representations? Leekam asserts that "the prospect of building a baby that reads minds but works like a machine is out of the question". Yet for cognitive neuroscientists, the goal is precisely to explain how it is that a baby can do things like read minds and yet work like a machine. This is because cognitive neuroscience is committed to the goal of characterizing the brain as a biological machine (like any other: a kidney, a heart, an eye).

2. What about a Vygotskian theory? Social constructivist theorists like Vygotsky are fine if your focus is narrowly on social relationships, but this ignores evolutionary biology and neuropsychology. Inevitably,

the social constructivist view will run into difficulties when faced with cases of neurodevelopmental disorder such as autism in which, despite immersing the child in the relevant social relationships (caring parents, teachers, and sibs), the child fails to show universal behaviours such as joint-attention or theory of mind. This strongly suggests that aspects of the *machinery* are impaired, and that an appeal to social constructivism as an explanation is inappropriate. Furthermore, the fact that, in the normal case, joint attention and theory of mind are universal (as they appear to be), despite the huge variation in parenting styles between and within cultures, suggests that such behaviours are the expression of species-typical mechanisms.

Mayes

1. Does the development of mindreading require social input with parents and other caring adults? Without a doubt. Just as a "language instinct" (or Chomsky's LAD) would presumably not function without the necessary linguistic input, so the Mindreading System would presumably not function without the necessary social input. Cases of extreme deprivation in early childhood may be tragic natural experiments to illustrate this, though these have not been well studied in regard to mindreading.

2. Is it possible to have the Mindreading System in place and yet not use it in all situations? This raises a competence-performance distinction, which is useful. Mayes suggests the term "mindedness" to refer to the competence capacity, and "mindreading" to refer to the performance capacity. I prefer the old fashioned terms "competence" and "performance". I am glad she raises this distinction, however. Presumably we will find individuals who are within the normal range but who are just less efficient at mindreading (they have a performance deficit), but who nevertheless clearly have the competence. Studies of individual differences in mindreading have scarcely begun.

3. Do we need an affect recognition component in mindreading? See my comments to Charman, above.

Mitchell and Saltmarsh

1. If EDD were crucial in the development of ToMM, wouldn't congenitally blind children be forever impaired in ToMM? Yes, they would. This is one key reason why my model includes an alternative (amodal) route for developing ToMM – namely, through ID and SAM. EDD has

an important, but not a necessary role, in the normal development of SAM and ToMM. Mitchell and Saltmarsh suggest I have over-emphasized the visual modality, and overlooked the case of the blind, but they clearly misread me on this. Three out of the 4 mechanisms in the Mindreading System are amodal.

2. Don't triadic representations of the form [I-see-(Mum-sees-the bus)] miss out that [I-see-the bus] and [Mum-sees-the bus] at the same time? Not at all. This is precisely what triadic representations are able to capture, since what I see is *everything* inside the embedded brackets: the bus, mum, and mum seeing the bus.

3. Doesn't embeddedness in triadic representations fail to capture the child's new ability to reflect on a cognitive state, such as seeing? I had hoped that embeddedness would capture this reflective stance. Is there a better way of accounting for a reflexive stance, without using embedding?

4. Don't triadic representations fail to capture the "aboutness" of seeing? Again, I fear Mitchell and Saltmarsh have misread the article. Dyadic and triadic representations contain a relation slot filled by a volitional or perceptual term, connecting an Agent to a state of affairs. This is 'aboutness'.

5. Isn't it a limitation of the model that SAM centres exclusively on sensory information about gaze? Clearly it would be if the model did this! But as I stress throughout the article, SAM is *amodal*. It is therefore able to extract shared attention from verbal reference (or touch) too.

6. How is the model to account for understanding one's own mental states, not just those of other people? This is an important point, which I recognize my account is relatively silent about. Here are some possibilities however:

a) In infancy, ID can interpret the child's own actions (e.g., reaching, looking), as goal-directed.

b) In infancy, EDD can store information about seeing, and what the Self is looking at.

c) In toddlerhood, SAM can code not only when shared attention is established, but when it is not, thus gaining experience of the Self's own attitude being different to someone else's.

d) ToMM can attribute mental states to Self or another Agent precisely because such states can be ascribed indiscriminately. You can, for example, attribute thoughts to your mother, your computer, your dog, or yourself.

e) However, it is possible that any complete model of the Mindread-

ing System will need a component that can monitor one's own actual thoughts and desires on line. This issue needs further consideration.

f) Mitchell and Saltmarsh suggest experience of one's own violated expectations must count as relevant input in developing a concept of false belief. Note though that such experiences cannot in and of themselves be sufficient for the development of such a concept, since presumably children with autism experience their own expectations being violated – their toy is not where they searched for it – and yet they do not appear to be able to reflect on their own beliefs.

7. Doesn't the model require a mechanism for gradually overriding a reality bias? I think this is a good suggestion. Leslie and Roth (1993) suggest a 'selection processor' could do this work, and Russell (1994) suggests a central executive (of the Shallice/Baddeley type) could be required. These may be important refinements to the internal architecture of ToMM. Mitchell and Saltmarsh suggest language may function this way, and I accept that this is an important class of behaviour from which we gather information about another Agent's mental states. Other people literally *tell* us their thoughts. Not surprisingly, language can be considered a 'print-out' of mental states – but only if ToMM is in place to analyse it in this way. Children with autism process language, but not apparently as an indicator of the speaker's mental states. (See also my comments on Tager-Flusberg, below).

8. Are two subgroups of autism too few? I did not wish to assert that two are all there are. Indeed, the examples Mitchell and Saltmarsh suggest of other subgroups (e.g., children with autism who can pass first-order but not second-order false belief tasks) are drawn from my own studies. Furthermore, the notion that a child might be delayed *at any point* in the development of the Mindreading System is one that I have advanced in several places (e.g., Baron-Cohen, 1989c, 1991b, 1991d).

Moore

1. Is directionality sufficient for ID to fire? See my response to Gergely and Csibra's same question, above.

2. Does spontaneous gaze-monitoring necessarily involve SAM? See my response to Povinelli and Eddy's same question, below.

3. Is spontaneous gaze-monitoring initially easier to elicit by the combination of head and eye-direction? Certainly, this combination would serve to amplify the other person's direction of attention. Moreover, using head direction as an index of attention state is reasonably reliable, since head and eye-direction tend to correlate. In my model,

this would be achieved by ID and EDD working together. However, to conclude that the 'eyes do not have it' whilst acknowledging the preference infants show for the eyes seems contradictory.

4. Where does the infant's representation of Self come from? More specifically, where does its representations of the Self's eyes come from, given that this is not visual and therefore cannot be built by EDD? And how can double-touch be used to recognize equivalence of Self and another Agent, since double-touch is only ever experienced by the Self? Moore is right to raise these concerns. The biology of self-recognition is not well studied. Nor is it discussed in my target article, as I assume that information relating to self-recognition is not processed by the Mindreading System, but is processed elsewhere in the cognitive system. Explaining where and how this occurs is an important task. For my account, it is sufficient to note (a) that infants can distinguish Self from other Agents (infants tend to suck their own thumbs and not their mother's or father's); and (b) that infants are likely to be able to draw analogies between the Self and the other Agent. Moore's criticism that my model suffers from internal inconsistency is thus misplaced, since explaining the origins of the self-concept is not directly relevant to the Mindreading System.

5. What are the implications of Moore and Corkum's new data that infants do not follow gaze alone until 16-18m, and before this only follow head direction? There are two possibilities: (i) The results could be a function of the methodology employed, in which case other procedures may be more successful in finding infants younger than 16m following gaze alone. (ii) Or the finding could be evidence that head-direction detection (processed by ID) is used more in early development, since the cues are more salient, and that eye-direction (processed by EDD) is only used on its own when head and eye-direction are discrepant. In such cases, EDD would have to inhibit ID. Such inhibition may only develop slowly.

6. Isn't modularity only justified when there is a learnability problem? Since there is no learnability problem in joint attention, doesn't this imply SAM is just part of a general purpose learning mechanism? This argument has a problem in accounting for why children with autism do not learn joint attention, when they certainly learn many other things. It also has a problem in accounting for why the normal subjects in Moore's own studies do not show joint attention despite being rewarded for it. Learnability factors may be one reason to suspect modularity, but specificity of neurological impairment and characteristic ontogenetic timetable are at least two others.

Muir, Hains, and Symons

1. Is the still-face effect the result of ID receiving anomalous input? This interesting idea suggests new ways of testing ID experimentally.

2. Since 3 and 6 month old infants show more positive affect to a person than to an object, following a still-face procedure, doesn't this mean that "they do not read mental states into non-human stimuli"? I think this conclusion may be overhasty, in that at best all this shows is that infants at these ages show an affective preference for interacting with a person over an object. Whether ID fires given inanimate but agent-like stimuli or not remains to be systematically tested, but the preliminary results from Premack's and Gergely's laboratories suggest that it does. Muir et al. however make a good case for "contingency interaction" information being one class of stimuli that may cause ID to fire.

3. Is EDD active in 3 month olds? Hains et al. (1994) data are very interesting, demonstrating a clear sensitivity in 3-5 month olds (as seen in their smiling response) to eye-direction changes.

4. Does EDD show a 'pop-out' effect? Symons and Muir's (1994) data are relevant to my prediction that detection of eyes-looking-at-me shows a 'pop-out' effect. Muir et al.'s point is well made that this is not as rapid (800 ms) as, say, the pop-out detection of line-orientation (400 ms), but this is nevertheless rapid processing, implicating a special mechanism like EDD.

Perrett and Emery

1. Is EDD too specific? Should we instead postulate a Direction of Attention Detector (DAD)? As I understand Perrett and Emery's suggestion, DAD would code attention from head and nose direction, hand direction, posture, etc. In my model, ID processes such information as goal-directed. I am attracted by the idea of adding DAD to the Mind-reading System, as another input system to SAM (whilst still retaining ID and EDD). However, I would first have to be persuaded that proposing DAD as independent of ID was justified on grounds of its dissociability.

2. Might representing "looking at me"/"looking away" precede "looking at an external object"? This developmental hypothesis seems correct, and the evidence from Stern (1985) seems to be consistent with it. The idea that these might also show neurological dissociation is very interesting, and merits further testing. Fractionating EDD may well

reveal the existence of a Mutual Attention Mechanism (MAM), as Perrett and Emery suggest.

3. Are SAM's representations really triadic, or just a series of dyadic representations? I acknowledge that triadic representations are built from a series of dyadic representations. It may be that SAM's function is to bolt or fuse these together in such a way as to specify shared attention on an external object.

4. Does SAM arise from a coupling of MAM and DAD? This picture is, I think, consistent with my formulation that SAM integrates EDD and ID (see points 1 and 2, above).

5. Does ToMM involve orbito-frontal cortex (OFC)? I was interested to learn of the OFC sensitivity to the sight of threats (Thorpe et al., 1983). This may be convergent evidence with our SPECT study result (Baron-Cohen, Ring, et al., 1994). I look forward to more single cell studies of this area, using the kinds of methods Perrett and Emery suggest.

Povinelli and Eddy

1. Do Povinelli an Eddy's new data raise doubt about whether spontaneous gaze-monitoring (SGM) and spontaneous protodeclarative point-ing (SPP) are both expressions of SAM, or whether only the latter is? It appears that in chimpanzees, SGM may be present in the absence of any appreciation that eye-direction signifies attention. Chimps also appear to lack SPP. This looks like they possess aspects of EDD, and an orienting response to follow eye-direction, without possessing SAM. Povinelli and Eddy suggest such a primitive gaze-following orienting response might be very adaptive in social animals, since it would reorient the observer to a potentially important event – a predator – without any necessary awareness that the other animal *sees* the predator.

However, we must be careful in drawing conclusions from chimpanzees to humans. In the human case, we rarely (if ever) see such a dissociation between SGM and SPP. Both appear at roughly the same time in normal development (9-14m), and both appear to be impaired in autism. This strongly suggests that they are both being driven by the same mechanism (something like SAM). Povinelli and Eddy are therefore right to warn us that SGM may not be a *unique* diagnostic indicator of SAM in all species: a similar looking behaviour could arise as a result of a simpler mechanism (such as EDD). Rather, the *cluster* of behaviours (SGM, SPP, and showing gestures) which we see in human toddlers, may be a more reliable indicator of SAM's functioning.

Segal

1. Why not opt for intermodular interaction, instead of abandoning informational encapsulation altogether? I think this suggestion is a good one.

2. Why not let dyadic and triadic representations take propositions in their object slot? Again, this is a welcome modification to the model. (See also comments on Gergely and Csibra, above).

3. Aren't the logical properties of triadic representation more clearly represented in [Mum-sees-the cup] and [I-see-the cup], instead of [Mummy-sees-(I-see-the cup)]? Segal may be correct, though I worry whether this alternative formulation specifies the *shared* aspect of shared attention: that at least one of the two agents is aware that they are both seeing the object or event at the same time. Nevertheless, Segal's formulation brings out an essential property of triadic representations, namely, that they are derived from integrating two dyadic representations.

Sheinkopf and Mundy

1. Doesn't SAM need to do two things: (1) respond to the attentional focus of another Agent, and (2) redirect it to converge on its own attentional focus? I certainly see SAM as doing both, and suggest that both involve triadic representation. Povinelli and Eddy also raise the question as to whether response to another's gaze-switch necessarily involves triadic representation (see above), and I agree that the case is much clearer in the case of directing another's attention towards one's own. This is because redirection is aimed at achieving representations of the type [Agent-sees- (Self-sees-"proposition")]. The evidence of a dissociation in some subjects (e.g., chimps) between intact responding to gaze shifts, and an inability to redirect an Agent's gaze shift, suggests that some limited form of gaze-following (of a non-triadic variety) may be a property of EDD.

2. Doesn't SAM have a primarily affective role? Charman (above) makes the same point. However, to see the force of this, we need to disentangle (a) if the SAM deficit in autism primarily involves appreciating that others have attentional states which may or may not converge with one's own; or (b) if children with autism understand that shared attention is possible, but do not appreciate that it is a vehicle for sharing affect. My own suspicion is that (a) is correct, and that they not only fail to share attention about their affective state, but about anything.

Tager-Flusberg

1. Should ID, EDD, and SAM be considered as aspects of ToMM, rather than independent of ToMM? The test for whether these earlier mechanisms are independent of ToMM presumably would come from cases of dissociation. Clearly, congenital blindness shows us that EDD can dissociate from SAM and ToMM. So do cases of prosopagnosia such as patient AB (Campbell et al., 1990). Autism shows us that ID and EDD can both dissociate from SAM, and that in some cases, SAM may dissociate from ToMM. Whether other such dissociations exist remains to be fully investigated. But certainly, I see all 4 mechanisms as aspects of the Mindreading System.

2. Can the modularity of language teach us about the modularity of mindreading? A comparison of this sort may be very worthwhile, though this remains to be carried out in detail.

3. Could ToMM depend on a mechanism for processing syntactic structures of a particular complexity? At first glance, syntactic complexity does not seem sufficient for explaining mindreading, in that children with autism appear to understand the syntax of reported speech ("John *said* the chocolate was in the cupboard") whilst still having difficulty understanding mental state sentences with a similar syntactic complexity ("John *thought* the chocolate was in the cupboard"). However, this is an empirical question, and one which merits further investigation.

4. Wouldn't a close relationship between ToMM and language be expected if both evolved simultaneously? This may be correct, but I would argue that aspects of mindreading must have developed *before* language, for several reasons: (i) many non-human primates (without language) appear to have aspects of mindreading (ID, and primitive functions of EDD); (ii) language would be of little value without SAM (it would be impossible to establish shared reference, for example); and (iii) language without ToMM would be very limited – in fact, as limited as the language produced by most children with autism. Indeed, one view is that the speech of many children with autism shows us what a pure "syntax organ" might look like, devoid of the normal overlay of pragmatics. Pragmatics thus becomes defined as interaction of language and mindreading.

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