

Author's response

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I have read the replies to my target article with great interest. Each addresses different issues of relevance to our current and future knowledge of primates' theory of mind (ToM). Thus, **Høgh-Olesen, Anderson, Caldwell, and Krøjgaard** raise concern about the methodological aspects of the ToM paradigms; **Engelsted, Schultz, and Rochat** present philosophical points relating to the ToM debate and to that of being a human being; and **Call, Anderson, Krøjgaard, Engelsted, Köppe, and Baron-Cohen** discuss human and non-human enculturation in general and the enculturation process in particular.

Below, I will respond to most of the points in the replies.

The problem with anecdotes

Høgh-Olesen draws attention to two paradoxes in the ToM research. First, why do anecdotes regarding non-human primate ToM abound when the evidence is, at best, non-conclusive in the event of non-human primate ToM being subdued to controlled experimental procedures? Second, why do non-human primates succeed in gaze-following experiments more consistently than in object-choice tasks when the latter, among other things, involve precisely the ability to take cues from a gaze?

Starting with the paradox concerning anecdotes, the reader should keep in mind the insightful and often cited words of Bernstein (1988, p.247): the plural of "anecdote" is not "data"! In the debate, the general impression is that if we were able to collect a sufficient amount of anecdotes of what appears to be evidence of ToM in non-human primates, we would somehow reach a magical threshold in which learning will no longer apply as a rival explanation. However, the sheer amount of anecdotes, no matter how extensive, does not hold that power in itself. This is due to at least two different kinds of validity problems.

First, I want to congratulate and thank **Høgh-Olesen** for pointing out correctly and importantly how humans simply cannot help attributing "sense and meaning into the meaningless...". Being *Homo sapiens sapiens*, this entails not only having the capacity of ToM yourself, but also the capacity of comprehending the world in terms of everybody (and everything!) having that capacity as well! Observe any person with their dog, guppy, PC, or two-week-old child, and it becomes clear that it really does not matter that much to a human being whether the response he or she receives from the surrounding world could not possibly be related to ToM. Humans are always quite certain that some kind of ToM is at hand! The geometry example cited by **Høgh-Olesen**, besides from being a personal favorite of mine as to the social responsiveness of humans, is a brilliant case in point. As **Høgh-Olesen** states, if we readily create meaningful stories out of the movements of

geometrical figures, which most certainly do not look anything like our conspecifics, what would stop us from spontaneously and inevitably interpreting the behavior of our closest animal relatives in terms of ToM? Not much, I am afraid. Even if ten people repeatedly observed one or several chimpanzee individuals behaving in a certain way that *might* be indicative of ToM, if more parsimonious explanations of the chimpanzee behavior existed, the *number* of observations or observers would not in itself strengthen the validity of the ToM explanation. If, on the other hand, it is more or less impossible, or at least highly implausible, to conceive of a given behavioral scenario without attributing ToM to the actor, the observations would be indefinitely more valid. However, as numerous authors with Heyes (e.g., 1988; 1993) and Povinelli (e.g., Povinelli & Vonk, 2003) in front have advocated repeatedly, this is not the case. Whenever a ToM anecdote is presented as evidence of ToM, if examined in details, often the anecdote, if not always, is equally well explained by learning (see Whiten & Byrne, 1988, with commentaries for a prime example of such analytical tug-of-war).

It is important for me to stress that it is *not* my mission here to argue that learning is the more correct explanation of the anecdotes reported. Neither do I oppose Byrne & Whiten's (1991) objection that sometimes the learning explanations are so laborious and convoluted that a ToM account is a much more likely explanation! Nevertheless, like Heyes, Povinelli, and others, I find it very rarely hard to re-interpret ToM anecdotes in a learning perspective. For instance, **Anderson** presents an anecdote featuring an orangutan, that, on several nights, escaped his cage by flicking the lock with a piece of wire, which he subsequently hid in his mouth to keep the caretakers from taking it away from him. I do not agree with **Anderson** when he states that "it is difficult not to attribute advanced planning abilities to this individual along with intentionality-understanding...". **Anderson's** ToM analysis is that the orangutan understands "that the caretakers were looking for the tool and would surely have deprived him of it earlier if they had known where to look". I find no problems in leaving out the ToM aspect of this analysis and simply contend that all but the most mentally challenged captive great ape individual (and probably most other captive mammals) knows that caretakers, as a rule, take everything out of the ordinary away from you if they get their hands on it. The learning rule, then, would simply be: "If you find something fun, put it in your pocket!", learned the hard way by experiencing repeatedly that: "Fun, but illegal, things that are sticking out of your hand or mouth or foot (!) will get stolen from you by caretakers so fast it makes your head spin!". Actually, it is my experience that great ape individuals are so used to having conspecifics stealing things from them that only the least quick-witted animal on the farm do not quickly learn to hide things.

Methodological problems with the ToM paradigms

Continuing to the paradox of why great apes are perfectly capable of gaze-following, but do not succeed in object-choice tasks, **Høgh-Olesen** suggests that gaze-following is a more ecologically relevant activity to a non-human primate than the object-choice paradigm. I absolutely agree. As I have discussed at length elsewhere (Byrmit, 2005, Chapter 5), the concealment of food items by screen and objects (cups, containers and so forth) in the object-choice task may in itself increase the complexity of the task to the extent that it distracts non-human primates from cue reading (Vick & Anderson, 2003). Call, Hare & Tomasello (1998), intriguingly, found that chimpanzees who previously had shown poor performance on a standard object-choice task were able to use an experimenter's gaze cue to direct them toward food hidden in a tube. The tube could be peered into from both the subject's and the experimenter's side but had a barrier inside of it that allowed only the experimenter to see the food. The authors suggest that from their previous experience with tubes and tube-like items the chimpanzees understood that the experimenter could see the food although they themselves could not. This would make the experimenter's cue actually refer to something in a way that looking at an opaque container does not. Likewise, Vick and Anderson (2003) found differences in baboon performance in three versions of the object-choice task. In one version of the task, an experimenter placed food items out in the open on both sides of the center line of a wooden tray, directed a cue toward one of the food items and pushed the tray toward the baboon subject in order for it to reach for one of the food items. In the second version, the food items were placed visibly on the tray, but were then covered by cups that the subject had to remove to get to the food item underneath. In the last version, a screen was introduced once the cups were in place and held for five seconds before the subject could respond. The data analysis revealed a near-significant effect of condition with a considerable decrease in performance with the introduction of the objects and the objects plus screen.

Krøjgaard is concerned that I do not consider the different standards by which we measure the performance of non-human primates and children. Specifically, he presents the point of view that the criterium to pass a cognitive test is more lenient when non-human primates are the subjects than when the subjects are human children. Thus, non-human primates are sometimes considered successful on a given paradigm even if they need several hundred trials to master the task while human children are given very few trials to succeed.

Krøjgaard is correct in that, historically, many cognitive tasks have used such different standards across species, and I certainly agree that it seems quite generous to attribute genuine ToM to a non-human primate subject based on its performance on the umpteenth trial after learning would have taken place most certainly, and that it is not any less biased to put children at a disadvantage than non-human primates. However, I do not share **Krøjgaard's** concern that such differ-

ent standards really represent a big problem these days. First and foremost, by now several authors (e.g., Heyes, 1993; Povinelli, 2000; Premack, 1988) have subjected older ToM data to rather meticulous scrutiny in an attempt to disentangle genuine understanding from more basic learning histories. This has been done by placing a premium on subjects' spontaneous performance, i.e. their performance on the first few trials, like one would do on the performances of human children. Also, several newer studies (e.g., Byrmit, 2005, Chapter 4) make much less sweeping claims about performances that occur only after numerous trials than used to be the case. Second, I still believe that non-human primates deserve a little extra patience, so to speak, in terms of the number of acceptable trials, simply because even non-verbal tasks, unquestionably, place extra demands on them compared to human children. Many of these extra demands I have already noted in the target article and elsewhere (Byrmit, 2005, Chapter 5). They arise simply because we, as humans, construct paradigms in a way that makes sense to ... humans, but not necessarily to any other creature on Earth. Despite the fact that Meltzoff's (1995) imitation study, as noted by **Krøjgaard**, only allowed human infant subjects very few trials to get acquainted with task demands, it is thus also certainly true that the task is inherently geared to even young humans. Humans *do* have a strong propensity to reproduce the actions of others; a propensity non-human primates, contrary to popular beliefs, do not share (see below). Furthermore, 18-month-old human infants, as opposed to non-human primates, have been subjected to numerous informal imitation "learning trials" in their everyday lives with their parents and other adults. This makes it reasonable to expect that they will understand task demands even if given only very few formal warm-up trials. Finally, human infants, again as opposed to non-human primates, are used to being around cultural artefacts and observing other people manipulating such artefacts, making the infants primed for the task at hand in a way that only enculturated non-human primates would be.

Other approaches to ToM

I thank **Anderson** for pointing out that not all non-human primate observations are of an anecdotal kind, but may be quite systematic, and I thoroughly appreciate that **Anderson** broadens the ToM discussion to include studies on behavior that, although not rigidly concerned with ToM, may potentially be related to or tap into ToM. The vital strength of some of these approaches, such as the reconciliation studies by de Waal and colleagues (e.g., Preuschoft, Wang, Aureli, and de Waal, 2002), is the ecological validity, which is regrettably lacking in the traditional ToM experiments. However, I am not sure whether all the examples suggested by **Anderson** really are illustrative in relation to ToM. While important differences definitely exist between the performances of monkeys and great apes in cognitive tasks such as mirror self-recognition and imitation, I am not at all confident that the skills and abilities implicit in tasks such as these are related to ToM.

To start with the example that non-human primates' victims of aggression often retaliate against the aggressor's kin, what does this tell us about ToM? It certainly tells us something about non-human primates' impressive understanding of complex social relationships, but I do not believe that it is necessary for an individual to attribute mental states to others to become upset about being attacked and then go on to attack any of its enemies' friends or relatives. To postulate that ToM is at stake here, we would have to hypothesize that the victim of aggression *appreciates* that the aggressor, indirectly, will get hurt by having a family member attacked, because he or she *understands* that the attack was really directed at him or her. I believe that the above example is much more parsimoniously explained by the fact that the victim, simply, associates family members with each other as these are always around the group and help each other, and, therefore, will get his or her tension released by re-directing anger toward a less dangerous family member than the original aggressor.

As to the differences found between monkeys and great apes in mirror self-recognition (MSR), controversy exists as to what this signifies. It is certainly true that Gallup (e.g., 1970) and the "early" Povinelli (e.g., 1994) advocated that MSR may imply at least a rudimentary self-concept, but since then, such interpretations have been severely criticized. Thus, I have to concur with Heyes (1998) and Tomasello & Call (1997) that MSR may be more about perception of the body and being able to distinguish sensory inputs resulting from one's own body from sensory inputs originating elsewhere (Heyes, 1998, p.105) than, necessarily, about loftier concepts such as self-awareness. In regard to the ToM debate, it is noteworthy that autistic children, who are supposedly incapable of mental state attribution, start to use mirrors to inspect their bodies at around the same age as normal children (Ungerer, 1989, cited in Heyes, 1998). Also, what are we to make of the fact that gorillas, a great ape species, do not appear to show MSR (see Tomasello & Call, 1997, for an overview)?

My caveats regarding ToM interpretations of mirror self-recognition also apply partly to imitation studies. First, although traditional views have long held that the great apes are great imitators (see e.g., for wild chimpanzees: Boesch & Boesch, 1990; and home-raised great apes: Gardner & Gardner, 1969; Miles, Mitchell, & Harper, 1996; Patterson & Linden, 1981; Russon & Galdikas, 1993), controversy has arisen, however, about whether or not previously reported instances are indicative of true imitation (see Tomasello & Call, 1997). This controversy is partly due to the occasionally fierce debate about what cognitive capacities underlie imitation in the first place. Nowadays, most researchers agree that observational learning may take different forms that may require different cognitive capacities. Thus, a distinction has been made in the literature between "true imitation" and other imitative processes (see Tomasello & Call, 1997, for an overview). True imitational learning requires for the observer to reproduce the behavior of a demonstrator faithfully upon observing this model's behavior. Thus, learning which occurs on the part of the observer simply because the demonstrator's presence increases the observer's behavioral frequency (so-called *social facilitation*) or attracts the observer to specific locations or objects with which the demonstrator interacts (so-called *stimu-*

lus enhancement) does not qualify as true imitation. Neither do instances in which the observer attempts to reproduce the same change of state that the demonstrator did, but does this in his or her own way instead of reproducing the actual behavior of the demonstrator (so-called *emulation learning*). Recently, a number of studies have addressed whether apes are capable of true imitation in light of the above distinction, and the conclusion seems to be that, again, relatively consistent differences in the performances of enculturated and nonenculturated great apes appear to exist (see Byrnit, 2005, Chapter 7C, for an overview). Whereas nonenculturated great ape individuals may readily engage in emulation, enculturated great apes will imitate the particular behavioral sequence of a demonstrator like humans would.

Second, although, in humans, imitation has often been linked to theory of mind (e.g., Call & Tomasello, 1996; Tomasello, Savage-Rumbaugh, and Kruger, 1993), just one overruling explanation for how individuals come to imitate may not exist. Maybe, for instance, it is the case that non-human individuals perform imitative behavior on the basis of a representation of the behavior of demonstrators (Heyes, 1998), or, as **Anderson** notes himself, on sensitivity to physical and temporal contingencies between the actions of oneself and those of the imitators. Humans, on the other hand, being the insistently mind-reading creatures they are (Baron-Cohen, 1995), imitate on the basis of what they perceive to be the demonstrator's intentions or goals. Such differences in attributions could both result in imitative behavior, and these would not necessarily be distinguishably different on the surface. In this regard, the superior imitative skills of enculturated individuals need not be explained in terms of advanced socio-cognitive development, but may rather show that such individuals have learned that it is well worth their effort to pay close attention to human behavior; that humans know how to solve object-related problems (Bering, 2004).

Caldwell, importantly, questions how to determine precisely what kind of experimental design might represent a natural situation to a given species or individual. She, hereby, implicitly addresses two vital points. First, it is a mistake simply to treat non-human primates as one homogenous group which shares the same *modus operandi*. Second, it is extremely difficult with any kind of certainty to establish which particular features of an experimental design, such as the new competitive ones, that tap into a given species' natural propensities.

The importance of eye contact

As dealt with in more details several other places (see e.g., Byrnit, 2005, Chapter 5), even in the most controlled experiment, several kinds of interspecies miscommunication may arise between the human researcher and the non-human subject. Let me here deal with one of these issues just briefly. **Caldwell** refers to a study in which marmosets (like it has been found with baboons previously) are quicker to learn to reach for a food item that is not being stared at than to reach for one that is being stared at and places these results within a

cooperative-competitive framework. Likewise, **Anderson** cites a study in which rhesus monkeys are more likely to approach and snatch food from a nearby human who is not looking their way than from one who is looking toward them and concludes that in such competitive situations, these rhesus monkeys subjects appear to show more sensitivity to being looked at than the chimpanzees who begged for food from humans in the now classic study by Povinelli & Eddy (1996). If I have understood the conclusions of **Caldwell** and **Anderson** correctly, I think they need to take into account the role being played by eye contact in non-human primates. While I profess to never having worked with marmoset, I have spent quite a bit of time around both rhesus monkeys and baboons and am impressed by their sensitivity to eye contact and by the vigilance with which they react to other individuals staring at them. This is very different from both chimpanzees' and orangutans' relative tolerance for eye contact, but in my experience more similar to that of gorillas as I have discussed elsewhere (Byrnit, 2005, Chapter 4). I find it quite plausible that the marmoset, baboon and rhesus monkey subjects mentioned above not so much react to the difference between a cooperative or competitive type of paradigm, but rather stay clear of the conditions in which people are staring directly at them. It would be interesting to see how they would perform if given a cooperative task with the researcher averting his or her eyes.

I am very excited that **Caldwell** brings marmosets and tamarins into the discussion and immediately ventures forth to offer the prophesy that callitrichid monkeys, despite their cooperative ways of life, will do no better than more competitive species on the standard version of the object-choice task! This I base on what were the most surprising results to me that Hare and colleagues (Hare & Tomasello, 1999) found dogs, *but not wolves* (Agnetta, Hare, & Tomasello, 2000), to succeed in the standard object-choice task. Given the social organization of dogs and wolves, which mirrors that of the callitrichids, I was certain that dogs' good performance on the object-choice task easily could be due to the cooperative demands of the paradigm, and, hence, that the reason for non-human primates' typically poor performance was explicable in terms of validity problems inherent in the methodology. Imagine my surprise when wolves turned out to perform as poorly as non-human primates on the paradigm! Apparently, the reason for dogs' good performance, then, has little to do with special cognitive skills associated with their cooperative social system, but must be related to some kind of aspect of the domestication process as the authors suggest themselves. If I, for the sake of discussion, for a moment disregard my ethical qualms about subjecting wild animals to the follies of humans, I would find it immensely interesting to see how a wolf raised in a human home would do on the object-choice task because it is still not clear to me whether domestic dogs perform well on the task because they are selectively bred to be of a kind that easily respond to human communication and culture, or if what we see is entirely an enculturation effect of being raised in a human home. I was jubilant when Hare and colleagues started to subject dogs and wolves to the standard object-choice task, and I still believe that object-choice studies on non-primate animals in time will help us crack the nut why

some species and not others are capable of employing human-given referential cues. Unfortunately, at the present time, the data seems rather confusing. **Caldwell's** suggestion to subject gibbons to tasks such as the object-choice paradigm, I strongly support. Gibbons, being lesser apes, have a pivotal role to play in cognitive studies as the evolutionary intermediary of monkeys and great apes, but surprisingly few cognitive studies have examined the *Hylobatidae* species.

Enculturating the human and non-human primate

The above discussion brings me to the complicated and fascinating subject of non-human primate enculturation. **Krøjgaard** points out that the term "enculturation" may be inadequately operationalized and needs a much firmer definition to function as a proper variable, and I agree. Enculturation is used rather off-handedly to mean anything from having spent your entire ape-life in a human home-like environment to having been trained for some years to perform in a circus. In addition, not many details on the subjects' rearing histories are known or presented, especially in older studies.

Krøjgaard is correct that the successful performances of the nonenculturated chimpanzee subjects in the competitive study by Hare, Call & Tomasello (2001) runs counter to my suggestion that enculturation is the critical catalyst in many ToM paradigms. Other kinds of data have also been accumulated since 2003 when I finished an early draft of the present target article. As described by **Call** himself, after years of advocating quite strongly, the belief that nonenculturated great apes do not understand intentions (for a comprehensive discussion, see Tomasello & Call, 1997), new data from Tomasello and Call's laboratory in Leipzig recently led the authors to revise this view.

Apart from the results of the competitive paradigms discussed in the target article, Call, Hare, Carpenter, & Tomasello in 2004 found that nonenculturated chimpanzees seem to understand more about others' intentions than was previously thought. In Call et al.'s study, a human experimenter was seated in front of the subject separated by a Plexiglas panel. At the bottom of the Plexiglas were three small holes through which food could be given to the subject. In motivational trials, subjects would enter the testing room and go to the Plexiglas window to receive a piece of fruit. In contrast, subjects always failed to receive any food in test trials, but the conditions impeding this were of two very different kinds: unwilling and unable. Thus, in unable trials, the experimenter was unable to transfer the food through the hole due to various circumstances in the situation, represented by six different conditions. For instance, in some unable trials the hole in the glass was too small to get food through; in others, the experimenter was distracted before he could deliver the food. In unwilling trials, represented by three different conditions, the experimenter would simply refuse to transfer the food, for instance, by teasing the subject, and holding the food near the hole, only to remove it whenever the subject tried to

take it through the hole with his or her lips or fingers. The dependent measures were the chimpanzees' behavioral rate and their latency to leave the testing room. Results showed that, overall, subjects had a higher behavioral rate in unwilling than in unable conditions, displaying angry behavior such as slapping or pushing the Plexiglas. Furthermore, the chimpanzees left the testing place at an earlier point in unwilling than in unable conditions, suggesting that they were more patient with the experimenter who tried to give them food, but somehow failed, than with the experimenter who had no such food transferring intentions. Also, these differences in behavior occurred spontaneously, i.e. before differential learning could have taken place, making the authors conclude that subjects were using the actions of the experimenter, not just as simple discriminative cues, but as a means to determine his or her goal, indicating that chimpanzees can understand at least some mental states in others.

Based on the above-mentioned results, Tomasello & Call (2004) have changed their view on nonenculturated great apes' understanding of intentionality and, hence, of the enculturation process. The data have also forced *me* to abandon what **Call**, rightly, terms my somewhat monolithic stance that nonenculturated great apes do not possess *any* kind of ToM abilities. As described by **Call**, "there is enough evidence today that nonenculturated apes, not just enculturated ones, possess some aspects of theory of mind". If nonenculturated great apes understand intentionality, enculturation must work differently than was originally suggested. In a preliminary revised version of the enculturation hypothesis, the authors suggest a "broadened version of Bering's" (Tomasello & Call, 2004, p.214), which I currently subscribe to. To summarize this new stance, great apes without the need of enculturation may understand some basic aspects of theory of mind such as intention, but enculturated individuals acquire a different perspective on and different social skills for interacting with humans. Thus, during ontogeny, great ape individuals raised by humans encounter difficulties with cultural objects and experience that human caretakers intervene in their unsuccessful behavioral strategies to solve the problem. This way, they come to perceive humans as problem-solving experts whose actions it is well worth paying attention to (Bering, 2004; Bjorklund, Younger, Bering, & Ragan, 2002). As **Call** suggests, this may mean that either enculturated great apes perform better on ToM tasks because they pay much closer attention to the human experimenters, or that they are more amenable to the type of testing used in the laboratory (see also Byrmit, 2005, Chapter 5). I regret, consequently, having to reply to the enthusiastic response by **Engelsted** with my revised point of view that enculturated great apes may not develop more complex levels of cognition or, even, truly participate in joint attention. Rather, what may develop is more vigilance and attention towards humans, which makes enculturated individuals do better on human type tasks. The data on non-human primates' use of joint attention was never convincing to begin with. However, please note that I still keep a very open mind to the enculturation issue as data is quite rapidly being accumulated, bringing new aspects to light continuously.

I am very excited that **Köppe** brings neoteny into the debate, and I owe him many thanks for stressing this issue in previous personal discussions which has made me contemplate human sociality and culture in a whole new way. Like **Köppe**, it surprises me that the discussion of too early birth in humans is relatively lacking in the ToM debate. If the nature-nurture distinction was always a somewhat artificial distinction, as pointed out by **Baron-Cohen**, neoteny brilliantly offers an explanation as to why in a human context, it makes next to no sense to remove the nurture aspect from the equation. A chimpanzee, dog, or dolphin raised in an impoverished or atypical environment, needless to say, will behave more or less atypical for its species, but a human without its socio-cultural environment is simply *not* a human at all! Neoteny may be the reason, as **Köppe** correctly points out, why the substantial difference between humans and other apes is not reducible to the genome. I was fascinated by the suggestion that given the right circumstances (longer pregnancies), humans would develop backwards, so to speak, and become much more like the other anthropoids (experimental subjects are needed!). This appears to be in contrast to **Engelsted's** stance that apes could potentially develop into being the mind-reading creatures that humans are, but the suggestion seems to me much more logical: that humans, having developed further away from our common ancestor that we share with the chimpanzees, would regress to an earlier stage instead of chimpanzees, magically, jumping "ahead". It is important for me to stress that I do *not* conceive of **Baron-Cohen's** modularity approach as a nativistic "caricature" that does not take environmental influences into account. What I questioned in the target article is the extent to which basic, innate modules are open to the kind of drastic cognitive changes that are postulated in the enculturation hypothesis.

Köppe says that because humans are born too early, a wholly new premium is placed on the "social determination of the socialization", which makes us much more sensitive to changes in society than other animals. I believe neoteny, as acknowledged by **Rochat**, has to be taken into account as **Rochat** claims that new levels of ToM evolved in modern humans as by-products of particular ways of sharing resources; that ToM evolved by "co-existential necessity". Actually, the sharing of resources itself places a special emphasis on human sociality, reciprocity and *trust* that may not have been possible without neoteny and human infants' long dependency on caregivers during which period they have to "adapt to such reciprocity in order to receive the care they need to survive".

Selective advantages of a mind-reading individual

In relation to the above, **Schultz** made me ponder if ToM really would lead to selective advantages. First, I applaud him for pointing out that ToM is too poorly based in a philosophical reference. This philosophical confusion seems to be at the root of many of the methodological problems inherent in ToM

paradigms. Second, I thank **Schultz** for an inspiring and thoughtful overview of phenomena and levels implicit in the debate on ToM. It immediately rushed me "back to the armchair", as suggested by **Høgh-Olesen**, to re-think which evolutionary challenges may have necessitated ToM. In the target article, I espoused the view that "mind-reading individuals would be able to anticipate others' behavior long before any moves had actually been made on their part, greatly enhancing their abilities to develop counter-strategies..." Implicit in this claim was the assumption that a chain of event only is foreseeable if you are capable of ToM. After reading **Schultz'** reply, however, I must contend that in most situations being able to tell functionality and intentionality apart is not necessary, but perceiving *functionality* in others, *is* necessary in order to anticipate their behavior. I was trying very hard to think of situations in which an individual capable of ToM truly would be at an advantage and in which other individuals only capable of perceiving functionality would be at a disadvantage. All the types of situations that came to mind had to do with deception. Although deception, as discussed in the target article, does occur in some animal species as a hard-wired response demanding no real insight about others' minds, I really do not see how you may deceive others intentionally without understanding something about their *beliefs*, not just perceiving the functionality of their behavior. But while deception might be *the* candidate for the evolution of ToM, it somehow struck me as lacking in ecological validity, as human life seems to have evolved around cooperation and sharing, not primarily around deception of group members.

Rochat provided me with the missing link in my account: *trust*. In an ideal world, no one would even think of deceiving you, and trust would thus be superfluous as its existence served no purpose. This makes trust and deception intimately linked. **Rochat** draws attention to the fact that although all social animals share resources, "humans have evolved unique ways of sharing based on reciprocity, agreement, contracts, or handshakes". I believe his account of the evolution of ToM could be correct in that these unique human ways of sharing possibly cannot exist without trust, which entails that deception is a possibility, and deception, to me, necessitates ToM. However, if I understand **Rochat** correctly, I do not agree with his statement that "what evolved in human evolution is the human niche, not the individual". To me, this is too close for comfort to claims about some sort of group evolution, which I do not espouse. Even if humans are inconceivable without culture, and consequently are socially determined, something must be "inside the individual" that, historically, have provided him or her with life-sustaining and reproductive advantages in order for evolution to work. Evolution does not work at an abstract level as relations between individuals. They would only "transact and share resources among themselves" in the words of **Rochat** if it was implied that the *individual* human being is capable of participating in these uniquely human relations.

The final issue I want to address is the relationship between ToM and language. **Köppe** emphasizes that the unique human brain may have been developed on account of the use of language, and **Engelsted**, in the same vein, stresses that it may be the case that language not only provides human chil-

dren, as opposed to animal subjects, with a means to display ToM, but is actually at the root of ToM. Citing **Engelsted**, ToM may be an accidental byproduct of language; language existed in some form or another before mind-reading. As discussed in the target article, I fully acknowledge that language may play a pivotal role in the development of a full-fledged ToM. I am indecisive, though, as to how to conceive the evolutionary co-development of language and ToM. Barring the most primitive of grunts related to emotional states, which all vocal animals are capable of and which would not qualify as language in a human sense, as soon as you have signals as stand-ins for objects, we are faced with the Wittgensteinian problem of how knowledge is culturally transferred about how certain words are connected with certain referents (see the target article for elaboration). This transfer seems to presuppose an understanding of others' intentions, which therefore must exist prior to or simultaneously with the evolution of language. To reiterate my statements from the target article, what language and language alone would make possible is a dramatic increase in the complexity of mind-reading in a species that already has an understanding of others' intentionality.

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