

We don't need a microscope to explore the chimpanzee's mind

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Abstract

The question of whether chimpanzees, like humans, reason about unobservable mental states remains highly controversial. On one account, chimpanzees are seen as possessing a psychological system for social cognition that represents and reasons about behaviours alone. A competing account allows that the chimpanzee's social cognition system *additionally* construes the behaviours it represents in terms of mental states. Because the range of behaviours that each of the two systems can generate is not currently known, and because the latter system depends upon the former, determining the presence of this latter system in chimpanzees is a far more difficult task than has been assumed. We call for recognition of this problem, and a shift from experimental paradigms that cannot resolve this question, to ones that might allow researchers to determine when it is necessary to postulate the presence of a system which reasons about both behaviour *and* mental states.

18.1 Emergence of a gentle controversy

Are humans alone in their ability to interpret behaviour in terms of unobservable mental states—things like feelings, beliefs, desires, emotions, and intentions? Or do we share the ability to reason about mental states (at least to some degree) with other species? Premack and Woodruff (1978) coined the phrase 'theory of mind' to isolate and draw attention to the class of psychological systems that have the property of reasoning about such states: 'A system of inferences of this kind,' they noted, 'may properly be regarded as a theory because such [mental] states are not directly observable, and the system can be used to make predictions about the behaviour of others' (p. 515). It is important to note that we use the term 'theory of mind' in the broadest sense, as shorthand for the ability to represent and reason about mental states.

Whether this ability is implemented through an intentional system that is truly ‘theory-like’ in the sense implied by the ‘theory–theory’ view, or whether one of the ‘simulationist’ accounts is correct, does not affect the arguments we make in this chapter. In this chapter, we do not endorse a particular theoretical position on this topic, but we avoid the term ‘mind reading’ as shorthand for the capacity to represent and reason about mental states because of its historical association with phenomena related to parapsychology (e.g. extrasensory perception).¹

For several years in the late 1990s, there appeared to be an emerging convergence of evidence which supported the idea that humans might indeed be alone in possessing a ‘theory of mind’ system; in other words, that the capacity to conceive of mental states might have evolved hand-in-hand with the human lineage (e.g. Povinelli and Prince 1998; Tomasello and Call 1997). To be sure, there were (and continue to be) plenty of opinions to the contrary (those who believe that chimpanzees, at least, also possess a system for reasoning about mental states). Nonetheless, the vast majority of experimental tests had suggested that chimpanzees (and other non-human primates) might not possess this ability (reviews by Heyes 1998; Tomasello and Call 1997). Alas, as is so often the case in the cognitive sciences, even this limited consensus proved transitory. The question of whether chimpanzees possess a ‘theory of mind’ system excites as much controversy now as it did when Premack and Woodruff (1978) first proposed the possibility a quarter century ago.

In particular, the two research groups that have published most widely in this area—our own group, based in the United States in Lafayette, Louisiana, and a group led by Michael Tomasello in Leipzig, Germany—have now parted ways in their assessment of the evidence related to ‘theory of mind’ capacities in primates (see Povinelli and Vonk 2003; Tomasello *et al.* 2003a, b). Of importance, however, is that the conclusions reached by these laboratories are not of the same kind. The Leipzig group has asserted a factual claim, proposing that by ‘turning up the microscope’ recent data have more or less definitively established that chimpanzees possess at least ‘parts’ of a ‘theory of mind’ system: ‘... although chimpanzees almost certainly do not understand other minds in the same way that humans do (e.g. they apparently do not understand beliefs) they do understand some psychological processes (e.g. seeing)’ (Tomasello *et al.* 2003b, p. 239). In contrast, our group has reached a different interim conclusion: although we do not deny the possibility that chimpanzees possess an ability to represent and reason about mental states, we contend that *the research paradigms that have been heralded as providing evidence that*

¹ Editors’ note: In the rest of this volume, ‘mind reading’ is used in the generic sense for which Povinelli and Vonk prefer in this chapter to use ‘theory of mind.’ ‘Mind reading’ is used in the rest of the volume in order clearly to distinguish the generic capacity to understand the minds of others and attribute mental states to others from a particular account of how that is done, the theory-theory. Obviously, from use of ‘theory of mind’ in the generic sense that simply refers to mind-reading capacity, nothing can be inferred about the correctness of theory-theory as opposed to simulation theory.

they do reason about such mental states, do not, in principle, have the ability to provide evidence that uniquely supports that hypothesis. Thus, we show that so long as we rely upon the current paradigms, 'turning up the microscope' will not help. And, in a further move, we have also argued that key aspects of the data point toward the possibility that if chimpanzees do have a 'theory of mind' system, it must be radically different from our own. In what follows, we dissect this 'gentle controversy' and offer productive suggestions for how to make progress toward resolving it.

18.2 Is 'theory of mind' anthropocentric?

First, let us address a question that always evokes some worry when discussing the question of 'theory of mind' capacities in other species: 'Why devote so much energy to trying to determine if chimpanzees have a human-like 'theory of mind' system? Why not try to figure out what makes them chimpanzees, instead?'

At first glance, this would seem to be a legitimate concern. In trying to reconstruct the evolution of certain forms of cognition, we should never lose sight of the fact that the human mind is not the only psychological system in town (a point we have repeatedly stressed in recent years: e.g. Povinelli 2000; Povinelli and Bering 2002; Povinelli and Prince 1998). Surely, then, in comparing the psychologies of humans and chimpanzees, we should not overlook fascinating questions concerning the unique abilities of chimpanzees.

A moment's reflection, however, will show that this is only a single side of one of the many coins in the purse of comparative psychology. Furthermore, the flip side of this particular coin is that just as understanding the unique nature of chimpanzees can and should interest us as a project with its own intrinsic merits, so too should the task of discovering the unique abilities of humans. Indeed, a comparative psychology would surely be comparative: it would embrace the evolutionary notion of diversity, and should therefore be a science that asked equally about similarities *and* differences among species. Applied to the present problem—the question of the evolutionary history of a 'theory of mind' system—a mature comparative psychology should give equal scrutiny to each of the logical possibilities: (a) that the capacity to reason about mental states may be shared by many species; (b) that it may be unique to primates; (c) that it may be unique to some primates; (d) that it may be unique to humans alone; (e) that different aspects of the system may be present in different species (see Povinelli and Eddy 1996a, Chapter 2). In any event, it would never commit so egregious a sin as to exclude questions about what makes the human mind uniquely human in the first place. After all, this, too, is a fascinating question.

So, to our way of thinking, the criticism that it is anthropocentric to ask whether chimpanzees possess the capacity to reason about mental states is almost irrelevant. There is even a sense in which the validity of the charge of anthropocentrism will depend on the outcome of the science. If it turns out that 'theory of mind' capacities

are shared by at least some other species, then the endeavour would not have been anthropocentric at all. On the other hand, if the ability to reason about mental states turns out to be a unique specialization of the human mind, then, yes, of course, these efforts would have been profoundly anthropocentric. But the overarching point is that the motivation to ask whether other species possess these abilities is not an anthropocentric one, and, in any event, we will have learned more about both chimpanzees and humans for having done so.

18.3 Concepts about behaviour versus concepts about behaviour and mind

For the moment, let us place in abeyance the empirical question of whether chimpanzees actually possess an ability to reason about mental states, and instead address another frequently asked question, ‘How can you ever determine if a language-less organism reasons about the hidden, subjective mental states of others?’

The standard approach to answering this question goes something like this: ‘If the organism is reasoning about a particular mental state (for example, an *<intention>*) then we ought to be able to devise a behavioural test that can tap into it. So, (a) conceive of a situation in which a subject would need to respond in some fashion to the behaviour of another organism, then, (b) devise an experiment that will sort out whether the subject is reasoning about just the behaviour of another, or about both their behaviour *and* the relevant underlying mental state. Finally, make the design clear enough so that a distinct response ‘r’ can be predicted if the subject is reasoning strictly about behaviour, whereas distinct response ‘q’ can be predicted if the subject is also reasoning about the unobservable mental state.’

At first glance, this approach to constructing non-verbal ‘theory of mind’ tasks for an organism like the chimpanzee seems remarkably easy. But let us examine the logic of this general scheme and expose its fundamental flaws. First, explicit in this approach is the assumption that reasoning about the underlying mental state in such paradigms would *inevitably* lead to a fundamentally different response on the part of the subject than reasoning about surface behaviour alone. Second, there is an implicit assumption that the humans who design the experiments can use their folk psychology to successfully intuit which responses can be produced only by reasoning about the underlying mental state. In other words, it relies upon the experimenters’ use of their own folk psychology to posit a causal relationship between reasoning about mental states and subsequent behaviour. Anyone who doubts that this is in fact the current practice need only examine published scientific reports; typically, no formal demonstrations (logical or empirical) are provided to show that response ‘q’ is possible *only* if the subject is reasoning about the particular mental state under consideration. Rather, intuitions about how the human ‘theory of mind’ system works are used as a basis for designing experimental situations for other species.

A simple example from our own laboratory may help to clarify the role that our folk psychology plays in designing a non-verbal ‘theory of mind’ task, and certain logical flaws therein. Imagine that you wished to know if chimpanzees represent the psychological experience of <seeing> in others. You might reason as follows: ‘If I confronted them with two people—one of whom could see them, the other of whom could not—and created a situation where they had to use their natural begging gesture (a visually-based signal), and then if they gestured to the person who could see them, this would indicate an understanding of <seeing> on their part.’ We conducted precisely such a set of studies by confronting our chimpanzees with the simple situation depicted in Fig. 18.1a–c. First, the chimpanzees encountered only one experimenter, either on the right or left, approached her, and requested a piece of fruit by using their begging gesture. In testing, the situation was different: two experimenters were present, one facing the subject, the other facing away (Fig. 18.2a). The chimpanzees paused, but then proceeded directly to the experimenter who was facing them (the one who could see them) and gestured. Here, the purported response ‘q’ was consistently approaching the experimenter who could see them across trials, whereas purported response ‘r’ was approaching the experimenters equally often. The empirical results show that the chimpanzees, by the way, consistently produce response ‘q’ (see Povinelli and Eddy 1996a). As shall become clear, however, one of our central conclusions is that for the class of experiments in question, neatly carving up the response space into ‘q-’ and ‘r-types’ may be impossible. For instance, it will

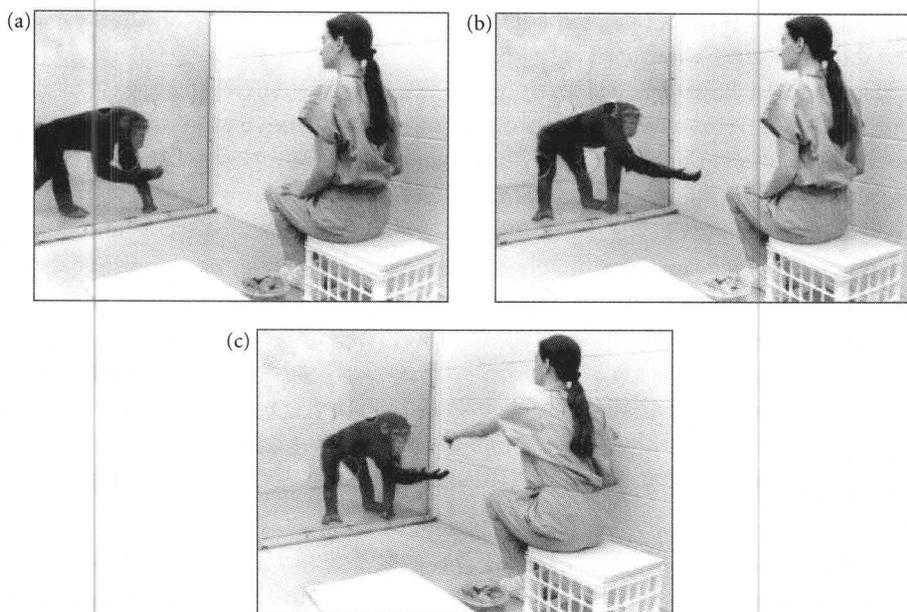


Fig. 18.1 A chimpanzee (a) approaches the experimenter, (b) requests a food reward with a species typical begging gesture, and (c) receives the food reward.

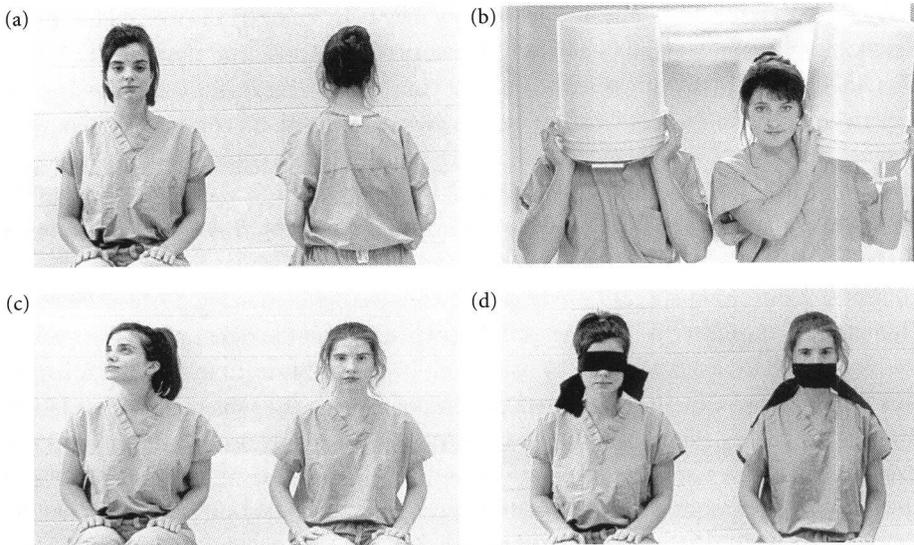


Fig. 18.2 Simple choice situations used to test chimpanzees' understanding of seeing: (a) front versus back condition, (b) buckets condition, (c) attending versus distracted condition, (d) blindfolds condition. Chimpanzees were successful in condition (a) from the first trial onward, but were at chance in conditions (b)-(d) until they received many trials with differential feedback.

become obvious in what follows that response 'q' could easily be generated by a system that does not reason about mental states.

Elsewhere, we have provided a critique of the approach described above (Povinelli and Vonk 2003). However, because our previous verbal critique was so general, it may not have been fully understood (see Tomasello *et al.* 2003b). Here we shall be slightly more formal (and hence more explicit). Let S_b stand for a psychological system dedicated to social cognition, but one which forms and uses concepts about only 'behaviours' which can, in principle, be observed. Further, let us suppose that this system is every bit as sophisticated as other cognitive systems already known to exist in humans and other animals. To be clear, we are not suggesting that a system that reasons about behaviour alone is not a 'cognitive' system. Clearly such a system would depend on the ability to represent and reason about complex intervening variables. The point is merely that the variables about which the organism is reasoning do not include representations of mental states. In particular, we conceive of S_b as having three main components:

1. a database of representations of both specific behaviours and statistical invariants which are abstracted across multiple instances of specific behaviours (representations that may be formed either by direct experience with the world, or may be epigenetically canalized);

2. a network of statistical relationships that adhere between and among the specific behaviours and invariants in the database;
3. an ability to use the statistical regularities to compute the likelihood of the specific future actions of others.

Important for this characterization of S_b is that it also interacts with the organism's representations of the physical layout of the world. (Baird and Baldwin (2001) have proposed that a system similar to this is fully operational in human infants, and Povinelli (2001) has described its operation in other species.)

This system (S_b) can now be properly contrasted with a psychological system that, in addition, reasons about the mental states of other organisms. Our use of the qualifier 'in addition' is crucial to understanding our argument, because in its traditional characterization, the human 'theory of mind' system cannot be thought of as operating in isolation from an organism's representations of behaviour; the system does not generate inferences about mental states in others at random. Rather, it uses information about ongoing, recent, or even quite temporally distant behaviours, to generate inferences about the likely mental states of others. Thus, we believe that the kinds of representations we posit to exist in the database of S_b exist in humans and interact with the 'theory of mind' system. *Indeed, it seems likely that much human social interaction is supported solely by the features of S_b that we have just described.* Indeed, Baird and Baldwin (2001) propose that human infants may initially rely precisely upon such a system.

So, to return to the case of the chimpanzee using its begging gesture to request food from the experimenter facing them as opposed to the one facing away, we can clearly see that our common-sense intuitions about what should qualify as response 'q' and what should qualify as response 'r' are highly problematic. We can see how response 'q' could have easily been generated by S_b and, furthermore, that response 'r' may not have been a reasonable prediction of the behaviour that would be generated by this system. Although the chimpanzee may or may not attribute the mental experience of <seeing> to the person facing them, they almost certainly know from previous experience that organisms that are facing them have a non-zero probability of responding to their visually based gestures, whereas those facing away do not. Indeed, in this situation we can clearly see that the notion of <seeing> is clearly secondary to the detection of the observable invariants associated with 'facing forward.'

What all of this means is that on the standard interpretation, the 'theory of mind' system can be considered to have a mutualistic relationship with S_b . Thus, because it is a system which must perform joint computations about both the behaviour and the mental states of other organisms in order to successfully predict future behaviour (and hence assist the organism in determining what actions it should take), we describe the 'theory of mind' system as S_{b+ms} . The inescapable implication of this is that making inferences about mental states does not allow an organism to skip the step of having to detect the abstract categories of behaviour and compute the regularities among them.

With these formalisms in mind, we can now ask a more difficult question: ‘Can the research paradigms that are currently in use with chimpanzees (and other animals) effectively distinguish between the operation/presence of S_{b+ms} versus S_b ?’ We submit that they cannot.

To show why, let us begin with an example. Faced with the indeterminacy of the results of the ‘front-versus-back’ test described above, suppose one wanted to further pursue the question of whether chimpanzees reason about who can and cannot <see> them. In this case, we allow chimpanzees to approach two individuals, one who is wearing blindfolds over her eyes and one who is not (Fig. 18.2d). Now (contrary to the actual empirical results), let us imagine that the chimpanzees immediately and consistently deploy their (visually-based) begging gestures in front of the person whose eyes are not covered by blindfolds. One interpretation is that the chimpanzee behaves in this manner because he or she knows that this person can <see> her, whereas the other person cannot. The logic of this interpretation is as follows:

- (a) chimp observes Suzy with eyes blindfolded (eyes not visible);
- (b) chimp observes Mary with eyes not blindfolded (eyes visible);
- (c) chimp concludes that because Mary’s eyes are uncovered, she can <see>, whereas Suzy cannot;
- (d) chimp gestures to Mary (because only people that can <see> the gestures respond appropriately to begging gestures).

But, given that (a) and (b) are both observable regularities, and (d) is a contingent outcome that stems from (c), we must critically ask which of the two aspects of Mary that the subject represents in (c) are causally related to generating the response of gesturing to her: the invariant associated with the observable feature of ‘unobstructed eyes,’ or the additional attribution of <seeing>? In short, if we substituted the perceptual invariant for the mental state in (d), would the same response occur? We submit that it would:

- (a) chimp observes Suzy with eyes blindfolded (eyes not visible);
- (b) chimp observes Mary with eyes not blindfolded (eyes visible);
- (c) chimp gestures to Mary (because only people whose eyes are visible respond appropriately).

One might object that this possibility could be easily ruled out by determining if chimpanzees would respond in the same way if the eyes were not merely visible, but were oriented in an appropriate versus inappropriate direction (e.g. Fig. 18.2c; Povinelli and Eddy 1996b). So, in this case Suzy’s eyes are open and uncovered, but are directed away from the subject, but Mary’s eyes are open and oriented in the subject’s direction. Imagine now that the chimpanzee still prefers to gesture to Mary over Suzy. Surely, then, this is not a simple case of assessing the presence or absence of the eyes, so the chimpanzee must be reasoning about who can <see> them—right?

Unfortunately, this logic does not hold. The reason is simple: the same kind of contingent dependencies between the observable features of others and the inference to a mental state still exist. The inference about *<seeing>* that would be generated by S_{b+ms} depends in the first place upon the orientation of the observable feature of eye direction. But the predicted outcome upon which the chimpanzee is basing its decision (the other person responding or not responding to their gesture) also depends on the orientation of the eyes. For instance, in the previous example, even assuming that the chimpanzee subject has an understanding of *<seeing>*, if the subject did not have a robust representation of the relevant perceptually invariant aspect of the other agent's pupil orientation, how could he or she ever compute what the other agent was *<seeing>*?

The general difficulty is that the design of these tests necessarily presupposes that the subjects notice, attend to, and/or represent, precisely those observable aspects of the other agent that are being experimentally manipulated. Once this is properly understood, however, it must be conceded that the subject's predictions about the other agent's future behaviour could be made either on the basis of a single step from knowledge about the contingent relationships between the relevant invariant features of the agent and the agent's subsequent behaviour, or on the basis of multiple steps from the invariant features, to the mental state, to the predicted behaviour. Without an analytical specification of what additional explanatory work the extra cognitive step is doing in the latter case, there is nothing to implicate the operation of S_{b+ms} over S_b alone.

Some researchers will object on the grounds of parsimony, claiming that positing the presence of S_b alone requires the existence of an intractably large number of specific rules by which the chimpanzee subject, for example, would need to behave (perhaps even drawing an analogy to the historical rejection of behaviourism on the grounds of parsimony; see Tomasello and Call, Chapter 17, this volume). As we have made clear, however, this is misleading. A hypothetical chimpanzee subject, endowed with a full-blown, human-like 'theory of mind' system, would still need the ability to detect every behavioural category that is relevant to a proper 'theory of mind' inference (regardless of whether one ascribes to theory-theory or simulation accounts of 'theory of mind' abilities). This is a key point: as originally conceived by Premack and Woodruff (1978), reasoning about mental states must entail observing and reasoning about behaviour (in all its subtleties) and, on the basis of such observed features, generating and reasoning about representations of unobserved mental states. Thus, the capacity to reason about mental states does not somehow relieve the burden of representing the massive nuances of behaviour or the statistical invariances that sort them into more and less related groups. In either event, these behavioural abstractions must be represented. With respect to parsimony, then, the question becomes a simple one: 'Is a system (S_b) that represents the invariant spatiotemporal aspects of behaviour which are the purported and observable manifestations of *<wanting>* a banana, for example, any more or

less parsimonious than a system that represents the invariant aspects of the same class of behaviours, but, in addition, generates a mental state concept to go along with it?

18.4 Parsimonious illusions?

In light of the preceding discussion, let us briefly examine how the concept of parsimony has been deployed in the current controversy. At several points, the Leipzig group has asserted that although it is possible that chimpanzees form concepts solely about behaviour, the case for this is unproven, and they seem to imply that parsimony should push us toward assuming that they do, in fact, represent mental states. For example Tomasello *et al.* (2003b) assert: 'Of course it is possible that human beings are the only species that understand any psychological processes in others, and we ourselves held this position not so very long ago. But evidence is mounting that it is simply not the case' (p. 239–240). Referring to us (i.e. Povinelli and Vonk 2003), they go on: 'We cannot dismiss this evidence by noting that simpler explanations are hypothetically possible with no supporting evidence' (p. 240).

The best interpretation of such statements is that the Leipzig group has not yet addressed the very heart of our analysis: namely, that because the current studies that are held up as evidence for 'theory of mind' capacities in chimpanzees presuppose that the subjects can form very (very!) subtle and abstract concepts of behaviour, dependent measures which then show that they know what to do when confronted with situations that are empirically linked to such behaviours cannot provide evidence in support of the hypothesis that they also form concepts about mental states purportedly linked to such behaviours. Why not? As we have shown above, the experiments do not specify the unique causal work that concepts about mental states do above and beyond the work that can be done by representations of the invariant aspects of behaviour. Indeed, if our analysis is correct, there is no sense in which a system that makes inferences about behavioural concepts alone provides a *less* parsimonious account of behaviour than a system that must make all of those same inferences *plus* generate inferences about mental states.² Although it is possible to

² Perhaps the sense of parsimony that is being invoked in such discussions pertains more to linguistic or explanatory parsimony—the idea that one can or should *describe* behaviour in the simplest *linguistic* manner possible, regardless of the underlying complexity of the behaviour itself. It is in some sense 'easier' for us to describe the chimpanzees as understanding <seeing> *then it is to explain that they understand all of the unique and specific behavioural regularities that must be computed first in order to then represent a concept of <seeing>* (see Dennett's 1987, notion of the 'intentional stance'). This sense of the term parsimony is misleading in the current discussion, however. Making use of a single term or concept such as <seeing> *to subsume all of the necessary abstractions does not change the fact that the psychological system itself cannot skip the step of representing the behavioural abstractions necessary to extract the concept of <seeing>*.

imagine situations in which responding appropriately in relatively novel situations might be facilitated by a system that reasons about mental states, we contend that a system that reasons about behavioural abstractions alone suffices to explain the data that currently exists (see also Povinelli and Vonk 2003, p. 159). Again, we are not suggesting that S_b is a simpler or lower-level system than S_{b+ms} in the sense implied by Morgan's canon (see Tomasello and Call, Chapter 17, this volume), despite the undeniable fact that the ability to reason about mental states depends upon an ability to reason about behaviour and not *vice versa*. In this context, it is unfortunate that we have previously referred to 'high-level' and 'low-level' models when testing for the presence of S_b versus S_{b+ms} , respectively (e.g. Povinelli 2000).

Povinelli and Vonk (2003) applied this analysis to several recent experiments that are frequently championed by the Leipzig group as establishing that chimpanzees reason about mental states (e.g. Hare *et al.* 2000, 2001). In responding to our analysis, Tomasello *et al.* (2003b) cry foul, asserting that we 'ignore' (p. 239) certain 'control conditions' present in their studies and that we simply assert alternative explanations with no data to support them (p. 240).

Unfortunately, these objections highlight the Leipzig group's failure to address our analysis which suggests that in the context of the class of experiments we have indicted, no control conditions can ever help to establish the presence of S_{b+ms} over S_b (see Povinelli and Vonk 2003). By way of illustration, let us examine their specific objection (Tomasello *et al.* 2003b). In one set of their published studies (Hare *et al.* 2000), a subordinate and dominant chimpanzee are placed in separate enclosures, facing each other, with an arena between them. Inside the arena are two pieces of food, one on the right, one on the left, with one of the pieces visible only to the subordinate because it is behind a small barrier (Fig. 18.3). These studies are designed to elucidate whether the subordinate can reason about which piece of food the dominant animal is able to see by measuring where the subordinate heads first when released into the arena (the dominant is released immediately thereafter). In some studies, the subordinate and dominant are allowed to look into the enclosure at the same time, raising the worry that the subordinate's future behaviour may be the result of reasoning about the

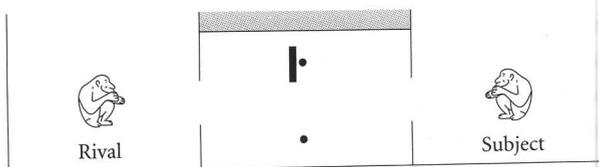


Fig. 18.3 An experimental condition used in Experiment 6 of Karin-D'Arcy and Povinelli (2002), an unsuccessful attempt to replicate key findings of Hare *et al.* (2000). One piece of food is fully occluded (visible only to the subordinate animal). The other piece of food is fully visible to both the subordinate and the rival.

observable behaviour of the dominant, avoiding the food toward which the dominant was oriented.

Apparently thinking that reasoning about behaviour must occur on-line, Tomasello *et al.* (2003b) note that a control condition was implemented in which the dominant's door was down 'and so there was no behaviour to read' (p. 239). However, even for their own logic to go through, the subordinate subject must be able to store a representation of the previous observable orientation of the dominant's body/face in relation to the food items—otherwise how could a subject with an understanding of <seeing> ever make the inference that 'the dominant was able <to see> the food over there, in the open'?

But, of course, once this point is granted, then one must also grant that the subordinate's reaction (heading away from the food that is visible to the dominant animal) can be explained either by the subject's possession of a concept about the statistical invariants that exist in head/eye/body orientation toward food, on the one hand, and future behaviour, on the other, or all of that, *plus* a representation of an unobservable mental state. To put a finer point on this issue, would the Leipzig group really wish to deny that chimpanzees have concepts about the invariant aspects of the observable past and current behaviour of their conspecifics, or, for that matter, their future behaviour? Of course not. And, if the Leipzig group concedes this, then in the absence of the production of an analytical proof demonstrating the work of S_{b+ms} over S_b , they must also concede our broader point that the outcome responses they believe to be of type 'q' can be explained in terms of the operation of S_b or S_{b+ms} .

Thus, the problem we face is not primarily an empirical one. Instead, the most pressing problem is to come to grips with the fact that the experimental results from the kinds of techniques that are currently in vogue cannot add a single bit of evidence in unique support of the conclusion that chimpanzees reason about mental states—*any* mental states.

18.5 Retooling our research paradigms

Of course, there is more to the current controversy than a debate over parsimony. For example it seems possible (even likely) that an organism possessing an S_{b+ms} wields certain predictive and explanatory abilities over and above an organism possessing only an S_b , irrespective of the issue of parsimony. As presently conceived and implemented, however, research paradigms do not analytically cope with this. There is no formal or informal demonstration associated with current research techniques which shows that S_b cannot generate the exact same behavioural predictions (without having to generate mental state inferences) as S_{b+ms} . Instead, interested scholars are simply asked to accept the folk psychological intuitions of the researchers in question.

If our current methods cannot distinguish between the presence of S_b versus S_{b+ms} in other species, then where can we turn for help? The core issue to emerge from the

above discussion is that the current generation of experiments do not offer an *a priori* way of demonstrating the additive causal impact of reasoning about mental states when it is combined with a system that already reasons about behaviour. Thus, the way out of this trap must be to either: (a) develop a general, formal, analytical equation specifying the theoretical limits on selected parameters for social complexity that are achievable by S_b , and further specifying the exact alteration of social complexity that the introduction of any particular S_{b+ms} could achieve; or (b) develop specific empirical tests which perform a logical end-run around S_b ; tests which have as at least one of their possible outcomes, q-type responses that logically cannot be generated by a system which reasons about behaviour alone (tests which deprive the subjects of the historical linkages between the relevant observable features of others and particular behavioural invariances). Tomasello *et al.* (2003a) correctly point out that this is, conceptually, what they and others (ourselves included) have been trying to do; as our analysis shows, however, they are incorrect in assuming that they (or we) have been successful.

Because we believe that the kind of general purpose analytical solution to the problem referred to in (a) will have to begin with assumptions about the computational limits of S_b and/or the unique impact of S_{b+ms} on the complexity of a given social system, and because we further believe that such assumptions will be extraordinarily difficult to empirically validate, we have focussed our attention on option (b) above. In particular, we have proposed pursuing a class of behavioural tasks which have, among their possible outcomes, behaviours that can be generated only by mapping self-experience onto the experience of others (Povinelli and Vonk 2003). Although not motivated by precisely the same theoretical concerns, Gallup (1988) proposed using such a class of tasks to assay whether other species might be capable of using their own mental states to model the experiences of others. For example he suggested allowing organisms to receive extensive first-person experience wearing sound-dampening ear muffs, and to then determine if they would alter the volume of their vocalizations when attempting to gain the attention of another individual wearing them. We believe that this class of self \rightarrow other inference tasks might achieve precisely the circumvention of S_b that is desired—as long as certain key stipulations are met.

To illustrate this approach, we recently suggested that a task offered by Heyes (1998) could be modified to provide unique evidence in favour of the idea that chimpanzees are reasoning about the mental experience of <seeing> in others. Heyes proposed a task in which chimpanzees would be given extensive experience covering their own eyes with two pairs of goggles, one of which they would be able to see through, the other of which they would not. Critically, the goggles would need to appear visually identical with the exception of an arbitrary cue, such as their colour. Also of critical importance, the subjects would not be allowed to observe others interacting with the goggles. This requirement ensures that the only relevant experience with the two pairs of goggles is the subject's first person, subjective experience of <seeing> or <not seeing> through

the goggles. Eliminating the subject's opportunity to observe the distinct behavioural invariances associated with others wearing the two goggle types, negates the possibility that correct discrimination between two such individuals at test is based upon such invariants. In other words, S_b has no information in its database on how others behave when such goggles are covering their eyes. In contrast, an organism that represents mental experiences would have the possibility of mapping its own first person experience of $\langle \text{seeing} \rangle$ or $\langle \text{not seeing} \rangle$ onto the other agents, and from that knowledge, make an inference about how to behave.

Heyes' (1998) proposed task involving these goggles was to be implemented as an extension of the guesser/knower procedure developed by Povinelli *et al.* (1990): after subjects obtained first-person experience with the sensory properties of the goggles, they would (a) observe two persons wearing the goggles who in turn are 'watching' a third person hide food in one of two locations, and then (b) receive contradictory advice from these persons (by pointing) about the location of the food. Although there is nothing in principal wrong with Heyes' proposal, this particular test requires subjects to discriminate between the *knowledge* states of others (that is, first the subjects would have to infer who can $\langle \text{see} \rangle$ the hiding process, then, from that, they have to infer who $\langle \text{knows} \rangle$ where the food is located). However, given the apparent consensus that chimpanzees do not appear to reason about epistemic states (see Tomasello *et al.* 2003a, p. 156; 2003b, p. 239), we proposed a variation of this test (again based on one of our earlier procedures) in which the subjects would be required to make only a putatively 'simpler' inference regarding the experimenters' ability to $\langle \text{see} \rangle$ (see Povinelli and Vonk 2003; for discussions of the development of 'level one' and 'level two' visual perspective taking in young children, see Flavell *et al.* 1981).

Our task is simply a variant of the one proposed by Heyes (1998) to focus it strictly on the question of $\langle \text{seeing} \rangle$. Subjects would first be exposed to the subjective experience of wearing two buckets containing visors which look identical from the outside, but one of which is see-through, the other of which is opaque (Fig. 18.4a). The buckets would be of different colours and/or shapes in order to provide the arbitrary cue to their different experiential qualities. Then, at test, subjects are given the opportunity to use their begging gesture to request food from one of two experimenters, one wearing the $\langle \text{seeing} \rangle$ bucket and the other wearing the $\langle \text{not seeing} \rangle$ bucket (Fig. 18.4b). Here, response 'q' would be the subjects' gesturing to the experimenter wearing the bucket with the see-through visor from the first trial forward. By definition, S_b has no information that would lead the subjects to generate this response. In contrast, a system that first codes the first-person mental experience, and then attributes an analogue of this experience to the other agent (in other words, S_{b+ms}) could have relevant information upon which to base a response.

Tomasello *et al.* (2003b) have responded to our proposal in a curious fashion. They have not denied the analytical problem we have outlined, nor indicted the task as an invalid measure of the attribution of $\langle \text{seeing} \rangle$. Instead, they suggested that the

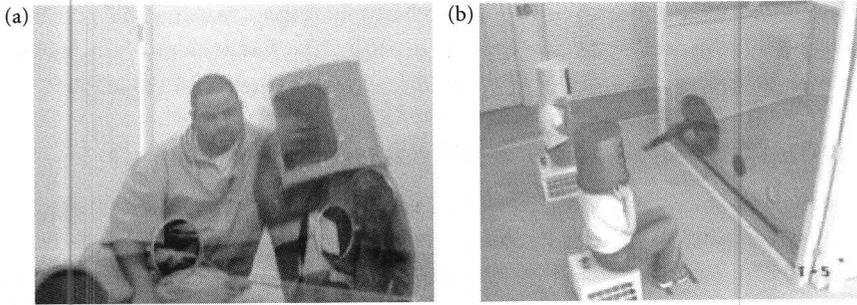


Fig. 18.4 (a) A subject being familiarized with the buckets. (b) A subject gestures to the incorrect experimenter.

proposed test has ‘very low ecological validity’ (p. 239) and, in support of this, they cite a brief commentary by Kamawar and Olson (1998) who report that the results of their unpublished pilot study using Heyes’ (1998) original test did not correlate well with other ‘theory of mind’ tasks in preschool children.

But the difficulties in their reasoning here are apparent. First, why should the standard, laboratory-based ‘theory of mind’ tasks used by Kamawar and Olson (and other developmental psychologists)—tests such as false belief, appearance-reality, etc.—be considered any more ecologically valid than Heyes’ original test? Second, the Heyes/Kamawar/Olson task is not the one we suggest. Heyes’ test was designed to assay <knowledge> attribution (something that Tomasello *et al.* 2003b, already assert that chimpanzees cannot do). Our modification is a test for their understanding of <seeing> (i.e. level-one visual perspective taking), precisely the ability that is currently under scrutiny and is the focal point of much of the current controversy with non-human primates. Finally, and most importantly, even if the results of our proposed test of understanding of <seeing> turned out not to correlate perfectly with other laboratory-based tests for this ability, what should we conclude? Should we follow the logic of Tomasello *et al.* and conclude that it is inferior to other non-verbal tests developed to assay this ability in chimpanzees? But wait: if the other tests could be ‘solved’ by either S_{b+ms} or by S_b , then why should the results of such indeterminate tasks be favoured? The analytical challenge we have offered would remain: we need tests that can, in principle, distinguish between S_b on the one hand, and S_{b+ms} , on the other. At the very least, this would be a test whose results *could* uniquely implicate the presence of S_{b+ms} even if it were not the ‘easiest’ test of its kind (as inferred by the results obtained with human children).

We are not trying to sell any task as the definitive ‘acid test’ for reasoning about mental states in general, or about <seeing> in particular; in fact, our task has its own potential pitfalls and limitations. Nor are we trying to suggest that there are no other approaches that will prove capable of distinguishing between the two systems in question. Rather, we have used this example to emphasize that if we want to address the

question of whether chimpanzees (or any other non-verbal creatures) have the ability to reason about mental states, we need to use tests that have the resolving power to discriminate between the work of S_b versus S_{b+ms} (see Povinelli and Vonk 2003).

18.6 Do we really need a more powerful microscope?

If current comparative methods are, *by their nature*, insufficient to address the question of ‘theory of mind’ capacities in chimpanzees, then we must take a harder look at Tomasello *et al.*’s (2003a) assertion that the ‘way forward in research on chimpanzee social cognition is to “turn up the microscope”’ (p. 156), a phrase repeated word for word in Tomasello *et al.* (2003b, p. 240, see also Tomasello and Call 2004). The logic of their analogy would appear to be that we can continue using our old microscopes as long as we crank up the magnification factor. And, apparently, they believe that by doing so, we will reveal previously hidden (apparently microscopic) elements of the chimpanzees’ ‘theory of mind’ system. Indeed, Tomasello *et al.* (2003a, b) allude to a group of unpublished studies from their laboratory which they argue further establish the presence of an ability to reason about certain mental states in chimpanzees.

Alas, if we are right, simply generating more studies within the current paradigm will not help. As we noted above (and see especially Povinelli and Vonk 2003), we do not need more experiments that have no ability to distinguish between the presence of S_b versus S_{b+ms} ; we need experiments of a conceptually different nature. Turning up the power on our existing microscopes, to pursue their metaphor, will merely confirm what we already know: that our minds are very good at automatically construing certain behaviours in terms of mental states (Fig. 18.5). Scaling up current methods



Fig. 18.5 Current experimental techniques reveal more about the workings of the human mind, than the chimpanzee’s mind.

can do nothing but scale up the indeterminacy of the results. (Incidentally, this will be true whether the testing frameworks are 'co-operative' or 'competitive' (e.g. Hare 2001) because that is not the axis along which the conceptual conflation of S_b and S_{b+ms} occurs; see also Item 6 in Appendix I where we address the criticisms of our studies about <seeing>.)

But then why does the 'microscope metaphor' appeal so much to the Leipzig group that they would use it three times? One possibility is that there may be a general proclivity to see 'the theory of mind system' as the 'holy grail' of comparative cognition, a view which inevitably creates a kind of narrow focus in which researchers come to believe that with just the right design, or just the right control condition, the long sought-after bejewelled cup will be found. Coupled with a conceptual framework that has difficulty acknowledging the evolution of novel cognitive innovations (see Povinelli and Vonk 2003), such an approach creates the illusion that we already know the answer, and that it's just a matter of coming up with the right test to prove it. Such dynamics appear throughout their recent opinion piece (Tomasello *et al.* 2003a). For example, experiments that would appear to confirm the presence of abilities to reason about certain mental states are hailed as 'breakthroughs' whereas our own, carefully conducted, programmatic set of over two dozen studies following a group of chimpanzees over their life-span, is dismissed as part of a general pattern of 'negative evidence' (p. 153). An objective scientific approach, however, would see progress toward providing evidence that uniquely supports either possibility as a breakthrough, and thus place the greatest emphasis on the resolving power of the methods used, not the results obtained.

In the end, there may be an even more fundamental danger in adopting the microscope metaphor of Tomasello *et al.* (2003a, b). The very idea of needing a microscope, let alone a more powerful one, to successfully characterize the nature of the chimpanzee's mind, resonates with the already widespread antievolutionary idea that the minds of other species are simply smaller, more watered-down versions of our own. We don't need a microscope to explore the chimpanzee's capacity to reason about mental states, we need experimental techniques that can distinguish between the operation of S_b and S_{b+ms} .

18.7 'All or none'?

There is an additional confusion that must be clarified if the current controversy is to be resolved. Tomasello *et al.* (2003b) assert that we believe that chimpanzees either have the entire human 'theory of mind' system or none of its components: 'Povinelli and Vonk argue that human beings have a theory of mind and chimpanzees do not. But this black and white picture is exceedingly misleading' (p. 239). Indeed, such a view *is* highly misleading; fortunately, it is not our own.

To begin, even a cursory glance at our laboratory's theoretical papers makes the falsity of this claim apparent. For example in our laboratory's early monograph,

What young chimpanzees know about seeing (Povinelli and Eddy 1996a), we devoted an entire chapter to issuing a plea that researchers consider the possibility of breaking down the ‘theory of mind’ system as it exists in adult humans into component parts, and then think about reconstructing the evolutionary timing of these separate parts:

... [U]sing the techniques outlined by Premack and Woodruff (1978), and applying them to the questions that have emerged from investigations of theory of mind in young children, it is now possible to determine whether theory of mind represents a psychological innovation unique to the human lineage or whether it is a more primitive innovation, perhaps one that evolved sometime after the divergence of the great ape-human lineage from other primates... *Additionally, it is quite possible that the psychological innovations responsible for theory of mind dispositions were not, in fact, a single innovation at all but rather evolved in a number of discrete steps... Thus... it is quite possible that transitions in theory of mind dispositions identified by developmental psychologists represent the retention of discrete ontogenetic innovations during the course of primate evolution.* (italics added, p. 14)

We concluded the chapter with the following suggestions:

It is clear from the above considerations that reconstructing the evolution of theory of mind will proceed through three distinct phases. To begin, *researchers must use the methods of comparative psychology to identify which species possess which aspects of mental state attribution and at what point in development.* The second step will be for researchers to use the methods of phylogenetic reconstruction to infer what the likely features of theory of mind were in each common ancestor... Once this reconstruction has occurred, the exact timing and order of each of the features will be known.... (italics added, p. 15–16).

Indeed, over the past several years we have clearly noted that our strategic retreat from asking about chimpanzees’ understanding of epistemic states (e.g. <knowing>) in the late 1980s and early 1990s, to perceptual states (e.g. <seeing>) in the mid to late 1990s, was motivated by our recognition that the human ‘theory of mind’ system might not be evolutionarily hegemonic (see Povinelli and Eddy 1996a; Povinelli and Prince 1998; Povinelli and Giambrone 2000). In doing so, we were merely following the lead of developmental researchers who were quite comfortable in thinking of various components of the human ‘theory of mind’ system emerging at different points in development.

Given that we most definitely do not believe that the ‘theory of mind’ system as found in adult, western cultures must be thought of as an indivisible psychological unit, why have Tomasello *et al.* (2003b) and others attributed this belief to us? The answer may be that it is easy to conflate our claim that the current evidence does not exclude the possibility that the capacity to conceive of mental states (possibly all hypothetical entities) is a unique feature of the human mind, with the very different claim that if any component of the human system is absent in chimpanzees, then the whole system must be absent.

But this cannot be the complete explanation, because Tomasello *et al.* (2003b) believe that it is unproductive to even entertain the hypothesis that the entire ‘theory of mind’ system is uniquely human: ‘... to repeat our earlier, more general point, we are certainly never going to make progress on questions concerning the evolution and

ontogeny of social cognition if we think in terms of a monolithic 'theory of mind' that species either do or do not have' (p. 239). Such an assertion could only have force if one already knew (from independent evidence) that the ability to conceive of mental states was not unique to the human species.

In the hope of diffusing this issue, let us break down the reasoning behind the Leipzig group's proscription and examine its logic. First, the point regarding human ontogeny is a red herring: no one is disputing the claim that human adults have the ability to reason about mental states. Thus, given that the system exists in some form or another in humans from all cultures, one very real possibility is that the system 'develops' in component parts (a possibility that has been urged by various researchers for many years; e.g. Leslie 1987; Wellman 1990). But this has no bearing on whether other species possess limited aspects of the system. Members of other species are not, after all, immature adult humans.

Next, with respect to the assertion about the *evolution* of the 'theory of mind' system, why is it not possible (or even likely) that a system for reasoning about mental states is indeed a uniquely derived feature of the human lineage? Why will we make 'no progress' by seriously considering this possibility? By way of analogy, could we make no progress toward understanding the evolution of echolocation in bats unless we assumed that closely related species (e.g. primates) have at least some parts of this echolocation system? Of course not.

In this sense, then, our laboratory's strong experimental emphasis on chimpanzees' understanding of <seeing> was a 'test case'—a point we have repeatedly stressed (e.g. Povinelli and Prince 1998). In humans, because our reasoning about visual perception is a context in which two worlds commingle—the world of observable things (gaze direction, head movements, eye movements, orientation of the torso, position of the eyelids, direction of movement, etc.) and the world of the private, unobserved features we infer in others—it seemed like an excellent place to examine possible evolutionary associations—and *dissociations*—between S_b and S_{b+ms} .

Indeed, we believe the results of the studies from our laboratory and elsewhere concerning non-human primates' understanding of visual perception have produced general lessons for trying to assess the ability of an organism to understand mental states. For example no one currently disputes that chimpanzees reason about the observable aspects of others that are relevant to visual perception (their face, eye direction, etc.). Indeed, numerous studies have shown that chimpanzees understand that they should direct their gestures preferentially to others who are facing them (Fig. 18.2a; Povinelli and Eddy 1996a; see also Hostetter *et al.* 2001; Tomasello *et al.* 1998). Other studies have revealed that they will follow gaze of others, even in response to simple eye movements (Povinelli and Eddy 1996b), and will even prefer to approach someone who makes eye contact with them (Povinelli and Eddy 1996c). Clearly, then, their knowledge about observable features related to the folk psychological notion of <seeing> is impressive.

18.8 Current tests can (and do) implicate the presence of S_b alone

Finally we can now turn to the second part of our claim, that, at the same time, the very same chimpanzees who provided us with the evidence for the abilities just described responded as if they knew nothing about <seeing> in such simple situations as those outlined in Fig. 18.2b–d. Why should this be so? In other words, if S_b is powerful enough to extract the kinds of information that we have suggested, then why do chimpanzees not show immediate evidence of understanding the implications of all of the regularities that exist in our tests? Furthermore, if we are right that current tests are inadequate to demonstrate the presence of S_{b+ms} , then how can we claim that the very same tests could uniquely implicate the presence of S_b alone?

First, there is no logical problem with our argument. We are simply proposing that a pattern of results of type ‘x’ could be produced by either S_{b+ms} or S_b , but that a pattern of results of type ‘y’ would be expected for S_b , but not for S_{b+ms} . In the case of their understanding of <seeing>, for example, the generalizations that our chimpanzee made across the carefully planned experiments were highly sequential and specific (first, generalizations about the front and back, then the face, then the eyes, with the initial generalizations being more important than the ones that were learned later), not an overarching generalization encapsulating the concept of <seeing> (see Povinelli and Eddy 1996a; Reaux *et al.* 1999). We have interpreted this pattern of results as showing that if S_b alone is present, although much of the organism’s behaviour will look strikingly similar to one that possesses S_{b+ms} , an experimental, microgenetic analysis will detect the tell-tale indicators of this fact (see especially Reaux *et al.* 1999, Exp. 4). Indeed, it is the complex interplay of what chimpanzees do and do not do in the same context (pattern ‘y’) that has led us to suggest that one viable hypothesis is that they have a powerful S_b , but no S_{b+ms} .

One reasonable way of thinking about this is to suppose that the chimpanzee’s S_b is wired up to spontaneously detect and exploit numerous relevant regularities that exist in the behavioural interactions of themselves and others, but that this system computes the fewest number of abstractions needed. Only if forced to make a distinction between two patterns that normally co-vary (e.g. eye orientation versus face orientation), will S_b bother to extract these distinct regularities, and then will do so only according to the contingencies to which it is exposed. We propose that the best interpretation of the combined experimental results from multiple laboratories is that S_{b+ms} and S_b are dissociable, and that chimpanzees may be living proof of how S_b will detect and store for future use only those relationships minimally necessary to uncover the predictive relationship between the current and past behaviour of others (broadly construed) and their future behaviour. To be clear, our results do not force this conclusion, they merely allow for it.

A distracting, but important, side-issue is the objection of numerous critics that our studies about <seeing> are flawed in one way or another. Because these criticisms are

numerous, varied, and even contradictory, we present them along with our responses in Appendix I. Suffice it to say here, however, that these criticisms merely highlight the fact that the most fundamental point of all of these studies has been frequently misunderstood. Our central conclusion has *not* been that chimpanzees ‘cannot do x’, but to the contrary, that under the right set of contingent experiences, S_b is powerful enough to abstract out the spatiotemporal invariances relevant to the given situation.

Again, to be absolutely clear, we possess no privileged information as to whether chimpanzees have the ability to reason about mental states. Thus, we assert no factual claim. However, we do believe that no current evidence uniquely provides evidence in favour of the idea that they do, and considerable evidence suggests that they are not making such inferences in situations where humans (children and adults) would readily do so. This is not a truth-claim: it is our assessment of how the current evidence bears upon the hypotheses at stake.

18.9 Will the real sceptic please stand up?

We end with a general message to the current generation of students who are fascinated by the question of whether other species possess the capacity to reason about mental states: ‘Do not lose your fascination with this problem, but, at the same time, do not be dissuaded from pursuing a more rational approach to investigating the question of whether other minds reason about mental states. Do not be blown into one camp or the other by jeers that you are a ‘sceptic’ or that you have produced ‘negative’ findings. Realize that the scholar who doubts that chimpanzees have a ‘theory of mind’ system, and the scholar who doubts that it is a uniquely human trait, are both sceptics, and that without scepticism there can be no such thing as science. Yes, *be* sceptical. Pursue multiple working hypotheses simultaneously, and be ruthless in your tests of the hypothesis that, in your heart, you know you truly favour (see Chamberlin 1897). In a single mind, embody *both* sceptical natures. Recognize that the hardest path is pursued by those who constantly challenge their beliefs, but also recognize that this is the most intellectually rewarding path. And most of all, do not be afraid of differences if that is where the evidence leads you. Differences among species are real. They’re what evolution is all about.’

Appendix 18.1 Criticisms of Povinelli and colleagues’ seeing-not seeing studies with rebuttals

1. The fact that the chimpanzees were responding to human experimenters (as opposed to conspecifics) invalidates the findings.

In order to be consistent, those who raise this objection to our experiments (the results of which consistently provided evidence inconsistent with the view that the chimpanzees were reasoning about <seeing>) would need to object just as strongly to the experiments

from their own (or other) laboratories that also make use of human experimenters, even those which seem to provide evidence confirming the presence of 'theory of mind' skills. For instance, results from experiments that require chimpanzees to distinguish between the intentions (accidental versus intentional), 'line-of-sight', deceptive actions, and attentional status of human experimenters have all recently been marshalled as strong support for the idea that chimpanzees reason about psychological states. The fact that the objection to using human experimenters only appears when the results *disconfirm* the presence of 'theory of mind' abilities, reveals a powerful underlying confirmatory bias in which experiments that seem to produce one class of evidence are not held to the same scrutiny as experiments producing a different class. If chimpanzees are expected to reveal evidence for inferring the mental states of humans in some situations, why should we ignore data from different situations in which they appear not to do so?

Indeed, if we disregarded the data from all studies in which chimpanzees were asked to infer the mental states of human experimenters we would be left with only observational studies (inferences from which are severely problematic; see Povinelli and Vonk 2003), and a very small set of experiments, the data from which has not uniquely confirmed the presence of one psychological system over the other (e.g. Call *et al.* 2000; Hare *et al.* 2000, 2001; Karin-D'Arcy and Povinelli 2002).

At a more conceptual level, the existing data robustly supports the view that chimpanzees respond appropriately to virtually the entire range of social signals from humans that are the visible manifestations of <seeing>. Indeed, time and time again, in our own studies we have shown that our chimpanzees attend and respond to these social signals (for a recent example see Povinelli *et al.* 2003). In fact, some researchers have taken their propensity to follow human gaze, their attraction to humans making eye contact with them, their tendency to use different forms of communication with humans instantiating differing attentional states, etc., as indicative of an ability to read the mental states of humans, as well as their behaviours. Thus, the existing data shows that chimpanzees respond to human social signals of eye/head direction and movement in the same manner (at least at our current level of resolution) that they respond to those of their fellow chimpanzees.

Finally, why should a 'theory of mind' system be so narrowly functional in chimpanzees that they can infer only the mental states of members of their own species? Although, we are not closed-minded to this possibility, (indeed we were among the first to suggest it), if it were the case, this narrow ability would clearly be radically different from that which humans invoke (see discussion of this issue in Povinelli 1996). Humans, for instance, attribute mental states not only to other species, but even to inanimate objects. Given the evidence that chimpanzees are avid psychological consumers of at least the behaviour of others, even when the behaviour is being performed by humans, it seems highly suspect to propose that the system for inferring mental states, which depends upon the system for reading behaviours, would be activated only when the object of perception has the exact same physical features of a chimpanzee.

2. The pretraining procedures in the Povinelli studies, in effect, trained their chimpanzees not to attend to the faces and eyes of the experimenters.

The fact that this objection is so often made, and yet so obviously false, makes it hard to know how to respond to it. For the archival record, our chimpanzees robustly demonstrated an inherent predisposition to attend to the most subtle cues regarding the faces and eyes of the experimenters, such as a slight deflection of the pupils, and required no training from us to do so (see for example Povinelli and Eddy 1996a, b, c, 1997; Povinelli *et al.* 1997; Theall and Povinelli 1999; Povinelli *et al.* 1999; Povinelli *et al.* 2002; Povinelli *et al.* 2003). Importantly, our chimpanzees exhibited these sensitivities on the very same trials in which they made no discrimination in the choice of gesturing to an experimenter who was visually attending to them, and one who was not (see especially, Povinelli and Eddy 1996a, Exp. 12). Thus, the claim that something about our procedures trained our subjects not to attend to the relevant social cues exhibits a severe lack of familiarity with our studies and their results.

3. The Povinelli studies reveal only that chimpanzees do not understand the eyes in particular as the portals of visual attention.

Again, this claim is empirically false. First, the data suggest that our chimpanzees minimally did not appreciate the specific relevance of the entire face to visual attention. In other words, they initially did not discriminate between conditions in which the experimenter's face was or was not visible (e.g. looking-over-the-shoulder, buckets, screens; see Povinelli and Eddy 1996a). Furthermore, Povinelli and Eddy (1996a, pp. 137–138) went to great lengths to outline a possible system in which chimpanzees might have concepts about an 'amodal' psychological state of <attention> without understanding the unique relevance of the eyes, or the face, in determining such attentional experience. However, as they pointed out, it is difficult to imagine how the hypothesis that chimpanzees have some notion of <seeing> that is linked only to the general frontal features of another organism, could ever be rigorously separated from the idea that they understand the importance of directing visually-based gestures to the fronts of others (see Povinelli 2001, for an extended discussion of this problem).

Tomasello and Call (this volume, Chapter 17) cite a paper by Kaminski *et al.* (2004) whose results they claim 'supersede' the 'negative' findings of Povinelli and Eddy (1996a). However, while Kaminski *et al.* apparently show that some apes are sensitive to the orientation of an experimenter's face when the experimenter's body is facing forward, notably the apes did not respond differently when the experimenter's body was turned away from the subjects, even though the experimenter's facial orientation was equally relevant in both cases. Thus, it is not clear that their apes have a general understanding of the significance of facial orientation; they may simply be responding to the salience of the cues, body orientation first and presence of the face second, exactly as suggested by Povinelli and Eddy (1996a). Furthermore, Kaminski *et al.* measured the behavioural response in terms of frequency of communicative signals.

It is not clear that such measures are comparable to those which require an ape to make a discriminative choice between two experimenters—one of whom can see them and the other of whom can not.

4. The fact that the experimenters in the Povinelli studies did not look the chimpanzees directly in the eyes invalidates the conclusions.

The core misconception of this objection seems to be that the scrupulous choreography used in *one* set of our experiments in which we did not allow the experimenters to make direct eye contact with the chimpanzees, was an oversight or flaw when the experiments were designed. In fact, this was a deliberate and crucial aspect of the procedure (for an extensive discussion of this issue, see Povinelli and Eddy 1996a, pp. 34–36). In those studies, eye contact was deliberately neutralized so as to preclude a different interpretation of the chimpanzees' possible correct choices; namely, that they may simply be attracted to the salience of direct eye contact in the correct conditions without necessarily drawing inferences about the experimenters' underlying mental experience of <seeing>. Significantly, analogous experiments with 2.5-year-old children, which provided even less salient direct eye contact cues, resulted in the children performing correctly from trial one forward (Povinelli and Eddy 1996a, Exp. 15).

Furthermore, additional studies specifically explored the role of direct eye contact, among other cues, in the exact same setting (see Povinelli and Eddy 1996b). And, indeed, in those studies, our chimpanzees *did* preferentially choose to respond to the experimenter who made eye contact with them. Of course, they also preferred to gesture to someone whose eyes were closed, but who made subtle head bobbing movements resembling chimpanzee behaviour, as opposed to someone whose eyes were open! Regardless of how one wishes to interpret these findings, they certainly vindicate our conceptual concern that eye contact (among other signals) may simply be a 'hot' social cue that has a high valence, quite independent of any understanding of <seeing>.

5. The reason for the difficulty that Povinelli's chimpanzees encountered was that they may simply not have the mental capacity to track and compute the cues related to the psychological states of two experimenters simultaneously.

This criticism would seem to begin with the assumption that chimpanzees have a concept of <seeing>, deploy and use it in their everyday natural social lives, but that they cannot keep track of who can and cannot see them. On the face of it, this seems implausible. In the relevant studies, the chimpanzees in actuality had only to interpret the attentional status of a single experimenter in order to succeed in either task. For example, they could enter the test unit and determine that the first experimenter they looked at did not <see> them and then approach the other. Or they could ascertain that the first experimenter they looked towards did <see> them and thus proceed to approach him or her. At the very least, this critique seems to embrace the idea that chimpanzees have a 'weaker' or more 'watered-down' version of the human system

for reasoning about mental states, an idea we find strongly suspect (see Povinelli 1996; Povinelli and Vonk 2003). Furthermore, we submit that data from analogous single-experimenter studies (see Theall and Povinelli 1999; Hostetter *et al.* 2001; Kaminski *et al.* 2004) simply confirm the findings of the two-experimenter studies.

6. The Povinelli studies were tests that occurred in a co-operative context, and co-operative settings are not appropriate for assaying the chimpanzees' ability to reason about mental states.

This criticism can be thought of as the 'ecological validity' complaint. Hare (2001) and colleagues have levelled this criticism, championing the use of competitive paradigms instead. They believe that competitive tests may be more ecologically valid and may thus provide a more conducive and natural context for the chimpanzees to engage in visual perspective taking, for example. Although this line of reasoning is possible, from a number of perspectives it seems questionable (see Povinelli 1996).

First and foremost, as we have already pointed out, the results from competitive tests can provide no better evidence for 'theory of mind' capacities than co-operative ones so long as the exact causal power of S_{b+ms} and of S_b continues to be unknown. Worse yet, because of the critical role of competition in the survival of an organism, competitive situations are precisely those for which evolution might have prepared an organism's cognitive system to respond in an intelligent, fast manner in the context of what may be highly costly situations. A system such as a 'theory of mind' system that is presumably 'designed' to allow for highly abstract interpretations of behaviour may not facilitate rapid responding, and may be least adaptive in such circumstances in which behavioural contingencies are relatively invariant. On the other hand, cognitive systems which exploit abstract representations of classes of behaviours, such as 'approach', 'facial expression type x', and 'aggression', may allow for less error, and may have been shaped in the evolutionary history of the species. The use of such representations may well appear to be evoked spontaneously (i.e. without evidence of 'learning'). For example socially isolated infant macaques that have never seen adults of their own species react appropriately to threat versus neutral facial expressions in photographs of adult monkeys (Sackett 1966). Thus, situations of 'high ecological relevance' may be among the worst contexts in which to seek responses generated by S_b that differ from those generated by S_{b+ms} (and hence, the worst circumstances to distinguish between the two systems).

Second, and perhaps more to the point, the argument that co-operative situations are unnatural for chimpanzees is unfounded. Every highly social species will have specifically evolved mechanisms that balance co-operative and competitive tendencies (see de Waal 1986). Hence, 'co-operation' is every bit as important to chimpanzee social ecology as is 'competition,' even though in the final analysis both have evolved in a manner that serve individual fitness. In fact, from the perspective of this criticism, it is somewhat ironic that observations of chimpanzees co-operatively hunting in the

wild have routinely been cited as suggestive of their mental perspective taking abilities (Boesch and Boesch-Acherman 2000). More specifically, the chimpanzees' begging gesture that is the focal point of this criticism was not specifically trained—it emerges in the course of normal social development. Because chimpanzees use this gesture to obtain food—both from their conspecifics and their human counterparts (both in captivity and in the wild)—it can hardly be construed as an unnatural behaviour. As it pertains to our own group of captive chimpanzees, they have an extensive history of using this gesture to obtain foods and other desirables from humans. Thus, utilizing scenarios in which they beg for food from humans in an experimental setting hardly seems ecologically invalid.

7. Povinelli's apes suffer from an impoverished rearing history and living environment that cripple their ability to respond in species-specific ways to tests about their social cognitive skills.

Finally we hook a red herring, an argument that is often used to deflect attention away from the real issues at hand. First, our chimpanzees have: (a) undergone extensive behavioural enrichment in the form of highly diverse cognitive tests in which they have participated two to three times a day for the past decade and a half; (b) been housed in social groups, replete with toys and other enriching objects, for their entire lives; and (c) have had extensive contact with humans (an experience often speculated to facilitate the acquisition of the very abilities we probed in our tests; i.e. Tomasello and Call 1996).

Second, our chimpanzees have led the way in demonstrating the complexities of their species' behaviour including the ability to follow human gaze, engage in mirror-guided self exploratory behaviours, joint attention, tool use, use social cues to determine object choice, etc. (for a recent example see Povinelli *et al.* 2003). In no way has their performance on our tasks, or their spontaneous behaviour, indicated that they deviate from the patterns of behaviour observed in wild chimpanzees or in other captive settings. In addition, preliminary data from our lab indicate that home-reared chimpanzees are no more likely than our own lab-reared chimpanzees to gesture preferentially to experimenters who can see them versus those who can not.

Third, if the concern about our chimpanzees' environment was valid, it would apply to any laboratory raised chimps, including those that other researchers claim show evidence of an understanding of mental states. Curiously, however, this complaint seems reserved for those cases in which the overall pattern of results is 'unpopular'. Would the same criticism have been launched at our chimpanzees had we been satisfied to accept indeterminate data as evidence of the ability to reason about mental states? We doubt it.

Thus, the surface behaviour of our chimpanzees is identical to that of chimpanzees in other labs, and indeed in the wild. But, what our studies have achieved has been to probe beneath the surface of their behaviour to examine in detail what social cues the chimpanzees were and were not using in deciding which experimenter to gesture

towards. In doing so, we have been led to conclude that there is no compelling evidence that they understand <seeing> in others.

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