Peter Krøjgaard
Department of Psychology, University of Aarhus

Comparative psychology, developmental psychology, and methodology

(Commentary to Jill Byrnit: Primate theory of mind: A state-of-the-art review)

Over the years there has been quite a bit of interest in the phylogenetic development of human cognition in Denmark (e.g., Engelsted, 1984; 1989; Katzenelson, 1988; 1989). However, until quite recently (e.g., Høgh-Olesen, 2004) the phylogenetic development of human beings has almost exclusively been approached from a theoretical perspective. Such an approach definitely has it own merits. However, I sincerely welcome the empirically based comparative psychological approach to primate cognition that Jill Byrnit advocates here – a strategy that at least complements the purely theoretical approach to the phylogenetic development of human beings. In brief: Among the closest relatives to human beings, the great apes are actually still alive and kicking, so why not study them directly?

In this vain, Jill Byrnit has written an engaged, well-written and focused review on primate theory of mind with special emphasis on non-human primates. Byrnit raises several interesting issues, but since I am no expert on non-human primates I will concentrate on the issues most relevant to my own area of research. My own main field of research is infant cognition, but I also have a genuine interest in the development of theory of mind in human beings. Thus, I will first comment on two issues framed as questions: (1) Is the 'enculturation thesis' currently the best bet, and how should it be studied? (2) Do the standards by which we measure the performance of non-human primates and children unequivocally favour the children? Next I have a few comments regarding the development of intentional understanding in human beings.

Is the 'enculturation thesis' currently the best bet, and how should it be studied?

Byrnit argues that the enculturation of the great apes might very well be responsible for the positive results obtained with great apes when confronted with joint attention tasks (i.e., object-choice tasks). The argument goes something like this: Results obtained with non-human primates on object-choice tasks indicate that whereas great apes are usually rather successful with regard to these tasks, monkeys consistently fail. These results have often been interpreted as evidence of the fact that great apes are simply genetically endowed with more intelligence than monkeys are. However, successful great apes, Byrnit argues, not only differ from monkeys by belonging to a different species of non-human primates, usually they also differ by being *enculturated*. Thus, genetic differences might have been confounded with differences regarding enculturation. This alternative interpretation gains indirect support from evidence found in Byrnit's own studies (2004), showing that non-enculturated great apes actually have at least some problems with object choice tasks.

Byrnit definitely has a point in proposing this alternative interpretation of the existing evidence. However, one might question whether the term 'enculturation' is adequately operationalized. To me, at least, it is not crystal clear when a given non-human primate is 'enculturated' or not. In the article enculturation is defined either as "[...] individuals who [...] have been raised by and lived with humans from an early age." (p. *8) or as "[...] individuals who have been raised in intimate and extensive contact with humans and human culture [...]." (p. *31). This definitely gives me an idea about enculturation, but I can also think of individuals that are *not* be easily categorized as either enculturated or not enculturated. However, in order to really solve the dispute we will need a firm criteria (e.g., from what age?, to what extent?, for how long?, etc.).

How could we test the thesis then? Well, in order to investigate the strength of the enculturation thesis we really ought to test at least four different groups of non-human primates on the same object-choice task: One group of non-enculturated monkeys, a second group of enculturated monkeys, a third group of non-enculturated great apes, and a fourth group of enculturated great apes. Furthermore, within each of the two species (great apes and monkeys) the individuals should be of equivalent genetic heritage before being allocated to either enculturation or 'savage' upbringing. This might in itself induce ethical concerns too, but that is another issue.

Byrnit presents a considerable amount of empirical evidence supporting the possibility of the enculturation thesis. However, it may be argued that some of the cited results actually run counter to the thesis (Tomasello & Call, 2004). For instance, many of the existing experiments within the object-choice paradigms have been criticized for their lack of ecological validity due to their *collaborative* nature (p. *28), so it is easy to understand why Byrnit (pp. *20-23) emphasizes the very clever *competitive* paradigm devised by Hare, Call and Tomasello (2001). The results from Hare et al.'s study (2001) show that, when tested in the presumably more ecologically valid competitive paradigm, the subdominant

chimpanzees actually seemed to know what the dominant chimpanzees knew (or at least could see). Now, it is, of course, debatable whether these chimpanzees should be granted with an understanding of the dominant chimpanzee's belief, or whether their understanding should be minimized to an understanding of the other individual's perspective excluding the philosophical term *belief* as such. Nevertheless, I think it seems fair to say that these chimpanzees understood the intention of the dominant chimpanzee at least to some extent –at least to the same extent as successful apes understand the intention of the assistant in the object-choice tasks. However, the results obtained in the study by Hare et al. (2001) actually seem to contradict the enculturation hypothesis because all but two of the 15 chimpanzees participating in the experiment were raised by their mother and the chimpanzee group (Hare et al., 2001, p. 141), and hence they should probably not be called enculturated. If enculturation was the critical catalyzer, these non-enculturated great apes should have been chanceless in view of the task. However, the results show the opposite.

Byrnit argues that enculturation, not genetic endowment, might be *the* catalyzing factor separating successful great apes from less successful non-human primates of lesser order. Perhaps this is not a question of either/or, perhaps both factors are important. However, this possibility is not really discussed.

To summarize, I think the term enculturation will need a much more firm definition in order to function as an operationalization of the term. Furthermore, I am less convinced than Byrnit that the superiority of the great apes' social cognitive abilities could be explained by some kind of enculturation *per se*.

Do the standards by which we measure the performance of non-human primates and children unequivocally favour the children?

Conducting comparative studies is no doubt a difficult enterprise. By considering some of the methodological problems within this field of research, Byrnit provides an insightful and elaborated discussion of some of the reasons why non-human primates seem to be less capable when tested in laboratory settings relative to the anecdotal evidence derived from field observations. Thus, Byrnit emphasizes specific areas that might disadvantage non-human primates in the tests relative to children (e.g., tests with non-human primates are usually carried out across species, often tests with non-human primates do not reflect the competitive nature of their lives in the wild).

While the abovementioned points are important and well addressed, Byrnit does not consider the often *different standards* by which we measure the *performance* of non-human primates and children, respectively. However, if these standards differ considerably and consistently, and yet remain unnoticed, these differences will obviously bias our evaluation of how to compare non-human primates and children on the basis of different tasks. If we take a closer look at these standards, it will become obvious that such differences will not favour children at all. Actually, the criteria for passing a test are, in my opinion, often much more forgiving when studying apes.

For example, consider the two studies (one with apes and one with children) that Byrnit cites in the same paragraph and take a closer look at how the criteria of success differ in these studies. Both studies are attempts to investigate whether the participants are able to use their knowledge of different helpers' diverging knowledge in order to decide which one of the helpers to rely on when trying to get food (apes) or toys (children). In the cleverly devised 'guesser/knower' chimpanzee study by Povinelli, Nelson, and Boysen (1990) referred to by Byrnit (p. *18)**,** the chimpanzees were each tested on approximately 240(!) trials before a conclusion could be established – or to be more specific: 10 trials each day for a period of three weeks (phase 2) [actually four weeks for one of the chimpanzees], plus the 10 trials for a period of three days (phase 3) (Povinelli et al., 1990, pp. 205-206). Byrnit (p. 19) refers to an analogous study with 3- and 4-year-old children conducted by Pratt and Bryant (1990). However, in this study the children were tested across 9-11 trials. The number of participants was considerably lower in the animal study (4 chimpanzees) by Povinelli et al. (1990) than in the study of (32) children by Pratt and Bryant (1990), and consequently the statistical power decreases when the studies are compared trial by trial. The difference in amount of generated data is, however, still very large and clearly favours the apes.

Sometimes non-human primates and children are tested and compared in exactly the same experiment (e.g., Call & Tomasello, 1998). In that case the standards are, of course, the same. My argument being that there seems to be a tendency that when studying non-human primates, researchers – or at least some of them – are quite generous to the amount of learning trials they will accept while still imputing genuine understanding to the participating non-human primates. However, I know of no experiments in which such standards would be accepted when it comes to studying children's understanding of mental states.

It is worth noting, when investigating human beings relative to non-human primates, that the use of relatively few trials is not only prevalent when the tasks involved for human beings are verbally based. For example, in Meltzoff's (1995) study (also cited by Byrnit) on 18-month-old infants' abilities to reproduce the goal of a specific action sequence, even though they were only presented with the intention of the act, the infants were only presented to three introduction trials and already tested on the fourth. This number of trials distinctly differs from the number typically involved when studying non-human primates.

To summarize, while Byrnit convincingly argues that some aspects of the studies seem to disadvantage non-human primates relative to children - especially those concerning task instructions (e.g., tests with non-human primates are often carried out across species) and the kinds of interaction

involved in the tests (e.g., tests with non-human primates do often not reflect the competitive nature of their lives in the wild) - I believe that when we consider the standards by which we measure their performance in tests, non-human primates are often *not* disadvantaged relative to children. Actually, I believe the opposite is the case. Thus, in order to conduct comparative psychology across species, we really ought to take *both* aspects into account.

Considerations regarding the developmental sequence of intentions and beliefs in children

My final comment is not a critical remark to Byrnit's article as such, but rather an attempt to think along the lines of the developing understanding of intentions and beliefs in children presented by Byrnit. Byrnit briefly outlines what is more or less the established understanding of how intentional understanding and understanding of beliefs develop in children. According to the traditional view, children seem to understand that people are intentional beings quite a bit earlier than they understand beliefs. For instance, as argued by Byrnit, they have a dawning understanding of intentions in their second year of life, at least to some extent, whereas a daunting understanding of beliefs does not come into play until around the third year of life (e.g., Wellman, 1993). And the more sophisticated understanding - that people's acts are actually constrained by their beliefs (even when their beliefs are wrong) – is typically not present until around their fourth birthday as evidenced by the results from the large number of false belief tests that has been carried out since Wimmer and Perner's (1983) seminal work (for an extensive review, see Wellman, Cross & Watson, 2001).

As outlined by Byrnit, there is a considerable amount of evidence supporting the view that children *begin* to understand intentions in their second year of life, that is, before beliefs, and I have myself come to the same conclusion in an earlier paper on the subject (Krøjgaard, 2002). However, neither Byrnit's present article nor my own deal with the issue regarding whether the development of children's understanding of intentions is also *consolidated* before they will actually have achieved a firm understanding of beliefs. On the face of it one might get that impression. However, the results obtained in a very clever and quite recent experiment by Schult (2002) indicate that children's elaborated understanding of intentions may not be in place prior to children's understanding of beliefs.

Schult (2002) argues convincingly, I believe, that much of the previous research on intentional understanding in children has actually to a large extent deals with desires and not intentions proper. Desires and intentions are usually discriminated as follows (e.g., Astington, 1993; Schult, 2002): Whereas a given desire can be satisfied in a number of ways, an intention is only fulfilled by actually carrying out the intended action. For instance, let us assume that I desire an ice cream and have the intention of buying one. Now, if someone subsequently were to offer me an ice cream for free, my desire will have been satisfied whereas my intention will not have been fulfilled.

Thus, in order to investigate whether children actually understand intentions, and not just desires, we need to include conditions in which desires and intentions are not *both* satisfied. According to Schult (2002), most of the previous studies on intentions have been designed in such a way that there has been a *match* between the desire *and* the outcome of the intended act. Whereas most of the previous studies have carefully included the possibility of unfulfilled intentions (e.g., Meltzoff, 1995), they have typically neglected the important condition in which the desired outcome is actually satisfied even though the intention is not fulfilled. To elaborate, in Meltzoff's (1995) otherwise very clever design we thus miss a condition in which the desire was satisfied (the end-goal of the act was obtained) whereas the intention to do so was not fulfilled – for instance a situation in which the dumb bell was separated by some coincidence. If children understand intention-in-action properly, they should be able to discriminate between situations in which the goal is obtained (a) by intention, or (b) by incident. Referring to Searle (1983), Schult (2002) calls the latter a *deviant causal chain*; that is, a desired outcome occurs because of an action that did not intend the outcome.

Schult (2002) seems to succeed in incorporating such a deviant causal chain in an elegant experiment in which 3-, 4-, and 5-year-old children participated in a target hitting game. In order to win the game the children had to get prizes. The children were asked to throw small beanbags into one of three buckets of different colours. Before each throw the child had to specify which bucket he or she was trying to hit (the blue, the white, or the green one). Some of the buckets contained prizes to be won. Sometimes the children would get a prize when they hit their target bucket, and sometimes (although it may seem like an odd game) they got a prize even though they hit a *different* bucket than the one they had specified prior to throwing the beanbag! Thus, the task involved situations in which the desired outcome was at times obtained even though the intention was not fulfilled. However, this is how an adult would evaluate the situation. In order to investigate the *children's* conception of such situations, they were always immediately after throwing a beanbag that perhaps brought them a prize, asked (among several control questions): "Which one were you trying to hit?" The results reveal that 4- and 5-year-old children had no problems "admitting" when they had attempted to hit a different bucket than the one that subsequently gave them a prize. On the contrary, the 3-year-olds often subsequently changed their minds about which bucket they had intended to hit! When getting a prize for hitting a different bucket than the one they had explicitly aimed for, they would typically maintain that the prize giving bucket was actually the one they had originally aimed for.

The results obtained by Schult (2002) seem to indicate that children do not have a full-fledged understanding of intention-in-action before the age of at least four, that is, at about the same time as children will typically pass the false belief tests. I think it would be very interesting to know how the children in Schult's (2002) experiment would have

responded, not only to their own intentions as studied by Schult, but to the intentions of the *other* children participating in the target hitting game. Although not very likely, it might be that the divergence in results between the 3-year-olds on the one side and the 4- and 5-year-olds on the other, as evidenced in the study by Schult (2002), refers to differences regarding how hard a time children have at different ages admitting that they actually went for another bucket. Even adults postrationalize at times! Results obtained from the additional experiment proposed here might provide evidence in order to decide between these two interpretations.

Thus, my point is not to reject the traditional outline of the developmental sequence of intentional understanding relative to beliefs, only to add that certain aspects of intentional understanding are actually quite complicated and may not be fully understood any earlier in the child's ontogenesis than beliefs.

Although I have raised some critical points to Byrnit's paper, I would like to stress that I endorse the empirical way of approaching the evolution of theory of mind in human beings, and I sincerely hope that Byrnit will follow this interesting and promising path in the years to come.

References

Astington, J.W. (1993).*The child's discovery of mind*. London: Fontana Press.

Byrnit, J.T. (2004). Nonenculturated orangutans' (*Pongo Pygmaeus*) use of experimenter-given manual and facial cues in an object-choice task. *Journal of Comparative Psychology, 118*, 309-315.

Call, J., & Tomasello, M. (1998). Distinguishing intentional from accidental actions in orangutans (*Pongo pygmaeus*), chimpanzees (*Pan troglodytes*), and human children (*Homo sapiens*). *Journal of Comparative Psychology, 112*, 192-206.

Engelsted, N. (1984). *Springet fra dyr til menneske* [The jump from animal to man]. København: Dansk Psykologisk Forlag.

Engelsted, N. (1989). What is the psyche and how did it get into the world? In N. Engelsted, L. Hem, & J. Mammen (Eds.), *Essays in general psychology* (pp. 13-48). Aarhus: Aarhus University Press.

Hare, B., Call, J, Tomasello, M. (2001). Do chimpanzees know what conspecifics know and do not know? *Animal Behaviour, 61*, 139-151.

Høgh-Olesen, H. (2004). Socialitetens grundformer [The basic forms of sociality]. *Bulletin fra Forum for Antropologisk Psykologi, 14*, 6-36.

Katzenelson, B. (1988). Evolution, antropologisk psykologi og kulturspsykologi. [Evolution, anthropological psychology and cultural psychology.]. *Psyke & Logos, 9*, 9-22.

Katzenelson, B. (1989). The phylogenesis and sociogenesis of the psyche and the law of correspondence. In N. Engelsted, L. Hem, & J. Mammen (Eds.), *Essays in general psychology* (pp. 49-70). Aarhus: Aarhus University Press.

Krøjgaard, P. (2002). "Jeg ved, hvad du vil!" Om udviklingen af barnets forståelse af sig selv og andre mennesker som intentionelle væsener. I M. Hermansen & A. Poulsen (Eds.), *Samfundets børn* (pp. 129-161). Århus: Forlaget Klim. ['I know, what you want!' On the development of the child's understanding of self and other as intentional agents.]

Meltzoff, A. (1995). Understanding the intentions of others: Re-enactment of intended acts by 18-month-old children. *Developmental Psychology, 31*, 838-850.

Povinelli, D.J., Nelson, K.E., & Boysen, S.T. (1990). Inferences about guessing and knowing by chimpanzees (pan trglydytes). *Journal of Comparative Psychology, 104*, 203-210.

Pratt, C., & Bryant, P. (1990). Young children understand that looking leads to knowing (so long as they are looking into a single barrel). *Child Development, 61*, 973-982.

Schult, C.A. (2002). Children's understanding of the distinction between intentions and desires. *Child Development, 73*, 1727-1747.

Searle, J.R. (1983). *Intentionality: An essay in the philosophy of mind*. New York: Cambridge University Press.

Tomasello, M., & Call, J. (2004). The role of humans in the cognitive development of apes revisited. *Animal Cognition, 7*, 213-215.

Wellman, H.M. (1993). Early understanding of mind: The normal case. In S. Baron-Cohen, H. Tager-Flusberg, & D.J. Cohen (Eds.), *Understanding other minds: Perspectives from autism* (pp. 10-39). New York: Oxford University Press.

Wellman, H.M., Cross, D., & Watson, J. (2001). Meta-analysis of theory-of-mind development: The truth about false beliefs. *Child Development, 72*, 655-684.

Wimmer, H., & Perner, J. (1983). Beliefs about beliefs: Representation and constraining function of wrong beliefs in young children's understanding of deception. *Cognition, 13*, 103-128.