EXPLORING PRIMATE SOCIAL COGNITION:
SOME CRITICAL REMARKS\textsuperscript{1})

by

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Introduction

The Scientific Committee has kindly asked the first author to comment on the recent move of primate ethologists into the domain of social cognition. Specifically, they have raised the question whether this movement has been "truly innovative". As the speaker is a largely postproductive male he decided that his lifetime scientific fitness could not be overly affected by accepting this interesting but thorny assignment.

The social cognition move seems to have originated in observations of the 1950ies that a primate can use another as a social tool. In hamadryas baboons, for example, subordinate females were seen to induce a dominant male to attack an opponent superior to themselves by the routine of "protected threat" (KUMMER, 1957). Over the years, CHANCE & MEAD (1953), Allison JOLLY (1966) and eventually HUMPHREY (1976) suggested that primates are so intelligent because their complex social life selected for cognitive abilities. The social cognition move gained momentum from CHEYNEY and SEYFARTH's field experiments on vervet monkeys. They suggested, for example, that a vervet knows which juveniles belong to which mothers of his group (CHENEY & SEYFARTH, 1980). In our lab, BACHMANN & KUMMER (1980) showed experimentally that baboon males responded to whether another male ranked high or low in the preference of a female, and Verena DASSER's (1988) experiments demonstrated by means of slides that long-tailed macaques have a generalized concept of the mother-child dyad.

\textsuperscript{1}) Text of a plenary lecture presented by the first author at IEC 21, Utrecht, The Netherlands.
In 1981, Donald Griffin (1982) convened a seminal Dahlem Conference on the question of Animal Minds, which profited from the critical and constructive contributions of philosophers. The conference achieved significant progress in analysis and interdisciplinary understanding.

The cognitive movement also brought forth a non-experimental offshoot. It culminated in 1982 with Frans de Waal’s book “Chimpanzee politics” and more recently with Whiten and Byrne’s collection of field anecdotes on Tactical Deception (1986) and the ensuing peer commentaries in Behavioral and Brain Sciences (1988), which is a short and instructive source about the state of the descriptive approach. It is this approach with which we take issue in particular.

This talk owes much to others. David Premack has answered my questions and taught me patiently for many hours over the last years. Two co-authors have widened the perspective of this talk: Verena Dasser, who has devoted her research to primate social cognition since 1981 and the philosopher Paul Hoyningen-Huene. Carola Stooob contributed valuable comments. The flaws and the unavoidable, even gross simplifications are, however, mine.

Why should field workers want to investigate cognition?

For the 30 years since the foundations of cognitive science were laid, primate field workers left cognition and intelligence to the laboratory psychologists. Why should we change that and try to master a rather difficult discipline that is alien to our concepts and methods? One reason is that animal psychologists were not too interested and successful in exploring social cognition. The sign languages taught to chimps, for example, contain no sign for kin, enemy, male, mother, or appeasement, and the work on sea lions and parrots keep neatly in this wake. Mainly, however, the new interest stems from the assumption that cognitive abilities were evolved and are ontogenetically modified in “natural environments”. There might be an ecology of intelligence. This expectation is encouraged by what we already know about adaptive Constraints on Learning, hard-wired learning programs that were evolved under selection of a specific context in the life of the animal in nature. Imprinting, Garcia Conditioning, and Song Learning in birds are examples. We may expect such “Adaptive Specializations” (Rozin, 1976) in primate cognition as well. The specific contexts in which they were formed could be identified in the field. This is an ethologist’s task which
I have outlined elsewhere (Kummer & Goodall, 1985). For example, there is evidence that some social cognitive abilities may have evolved particularly in subordinates.

But we shall want to go further than spotting interesting natural breeding grounds of primate cognition. And here we have our first obstacle to overcome. For some primatologists, field work seems to have an almost mythical quality. Some of my colleagues have maintained in personal discussions or correspondence that when evolved abilities are concerned, the field is the only reliable and productive source of insight. This is as if we said that quartz is formed only in rock crevices and we must therefore only investigate it in rock crevices because laboratory equipment might distort its reactions. We hear that the lab and the experiment are artificial and therefore unreliable. But where would virology, immunology, or neurobiology be if they had not taken this risk? The worry about distortion by experimental manipulation is justified, but it can be resolved. The optimal solution is to set up a continuous chain of conditions from the field to the experimental chamber that carefully monitors and minimizes the distortions which we have to accept in return for solid answers. This procedure has been used to pursue the gradual deformation of behavior in domestic animals across conditions in the interest of their welfare (Stolba, 1981).

We respect non-intervention as an ethical principle, but it should not be confused with arguments on methods. Scientifically, field observation is a method, and it requires complementation by other methods.

Another obstacle is that field workers are in general unprepared for cognitive research. There is an irony in field primatology turning cognitive, and it is two-fold. One is that cognition research is primarily concerned with proximate processes, with how an animal represents its environment. However, since the rise of the overriding interest in ultimate causes of animal behavior, many of us are losing the expertise in proximate research. Second, convincing interpretation of behavior in terms of cognition is impossible without sophisticated experimentation, but by and large, field primatologists never had this expertise and motivation.

The fashionable term of cognition is now sometimes misused for any perceptive process, for example for kin recognition. A simple definition seems therefore in place. Cognitive research is about how a subject represents his Umwelt in his brain. What does a primate know about his environment? How is this knowledge organized? And how is it processed? Cognition is organized knowledge that can be used in many
motivations and contexts: A cognitive topographical map, for example, can be used in foraging as well as in escape.

In the following we shall address two kinds of social cognition: Knowledge on the social structure of the animal's own group, and knowledge about mental states of other group members.

Social cognition as knowledge on group structure

Dasser's (1988) work is an example of research on the first topic. She asked: Do long-tailed macaques classify their knowledge about social structure into classes of relationships as we do? Do they, for example, have a generalized concept of the mother-child relationship? First, she determined whether the subjects recognized the individuals of their colony on color slides (Dasser, 1987). In one test, the subjects had to match slides of non-overlapping body parts, for example the lower half with the upper half of A's body when the lower half of B was the alternative. The subjects solved this problem reliably on the members of the Zurich colony, but they still could have done this without recognizing their group members on the slides, for example by recognizing similar skin texture or hair color in the body parts of the same animal. Under this hypothesis they should also have been able to match body parts of conspecifics they had never seen in real life, and in these tests they failed. Familiarity with the real individuals was conditional for slide recognition.

The concept "mother-child relationship" was examined by simultaneous discrimination and match-to-sample tasks. In one example, the subject was trained to consistently select slides of the same two females, which happened to be mother and daughter. The task was learned, but this the subject could have done in two ways: By simply associating the two individuals without reference to their relationship, or by learning that the solution was a mother-offspring dyad. In transfer tests she was now presented with slides of 14 other mother-offspring pairs of the colony and slides of dyads with other relationships; in all 14 tests she selected the mother-offspring dyads rather than the controls as positive, even in cases where the offspring was adult and older than the subject herself. Dasser concluded that the subjects had some concept of "mother and child" which they spontaneously applied to the positive training dyads.

Could this result not be obtained by observation in the field? Could one not find that a subject behaves toward all mother-infant dyads in a specific way, thus showing that he puts them in separate class, as if one wanted to know what characters defined the class of males for
sticklebacks by a specific behavior directed at males? In principle, yes. But the approach would be weaker: First, it is unlikely that primates respond to a type of relationship among others by a specific behavior. Second, the members of the dyad would always have to be together in these real life tests, and their specific interaction at the moment would affect the subject. Third, there would be no easy controls for finding those still unknown aspects of the mother-child dyad that are used as criteria for the concept by the macaque subject. That is why Tinbergen used dummies when he examined what was a male for a stickleback.

Social cognition as knowledge of mental states in group members

The above example concerns one aspect of the organization of primate knowledge about social structure. In the social cognition move, the prevailing interest is in whether primates manipulate their conspecifics, whether they deceive and conceal, have "politics" (de Waal, 1982) and use cunning "Machiavellian intelligence" (Byrne & Whiten, 1988). The fascination is about whether monkeys and apes have a representation of another's mind and attempt to influence it.

As an example, we choose a field anecdote on Tactical Deception from the stimulating collection by Whitén & Byrne (1986). Tactical deception is defined by the authors as "Acts from the normal repertoire of the Agent, deployed such that another individual is likely to misinterpret what the acts signify, to the advantage of the Agent". In one anecdote, the agent, a subdominant male baboon, while chased by another male, suddenly stopped and stared into a bush. The chaser immediately ceased his pursuit and stared as well. When no predator or intruder emerged, the chase was not resumed. Let us assume that the observer was correct in his judgement that there was not the slightest external stimulus in the bush that could have caught the agent’s attention. In a simplified way, one can then distinguish the following levels of knowledge in the agent:

There is a zero level, at which the actor has no knowledge of social effects of his staring. We unexpectedly encountered this level in 1967 during enclosure experiments on hamadryas baboons (Kummer et al., 1974). A male was admitted to a pair consisting of another male and his female after observing them for 15 min. The late-comers, called the rivals, did not fight for the female, but they scratched a lot, looked at the sky, fiddled with stones, and occasionally stared into the distance as if they saw something exciting. The same males never did this when they were placed in the role of the pair male. The data suggested that the star-
ing was attention redirected away from the pair, at which the rivals avoided looking. The staring never caused the pair to search for the imaginary leopard or eagle; they were not deceived. Observational support for this zero-interpretation would be contextual evidence of a conflict. Evidence that the staring occurred independently of any benefit for the agent would require experimental controls. The above experiment had at least the advantage of producing the conflict with a regularity that allowed a qualitative judgement. (To exemplify the complexities of interpretation, a much more demanding interpretation of the target’s non-response may be mentioned: The rival could “do something else” because he believes that the owner believes that the rival is interested in the female, and in order to disprove the owner’s belief. If the owner interpreted this as an honest message he would not stare into the bush with the rival but would do nothing, as in our experiments.)

Next, we could hypothesize, on level 1, that the baboon knows by trial and error learning that his staring can stop an opponent. This is not yet cognition; the baboon only knows that his conspecific will respond with a specific behavior to a specific action by himself.

But if he now generalized to staring at a bush when a rival groomed an oestrous female, or to stopping an attacker by a simulated find of hidden food, we would attribute him with organized knowledge independent of context, that is with cognition, level 2. He would know: “If I do anything that causes B to orient he will stop doing whatever he was doing”. His knowledge is organized. To accept this cognitive interpretation we would want to see the baboon generalize his knowledge to new contexts and to cross-connect many undesirable behaviors of his targets to many interrupting behaviors of his own. To be sure that he had not learned each combination by trial and error, we would have to observe the complete history of such stopping behavior in a sample of individuals, or, more realistically, to control the ontogenetic inputs in an experiment. The procedure is clear at least in principle and not essentially different from ethological methods in ontogeny.

The social cognition move, however, is interested in yet another level of interpretation, here labelled level 3: Not only that the subject has knowledge but that he attributes knowledge to others. The deceiving baboon might know that his conspecific has mental states such as intentions to attack and beliefs about predators; states which he, the deceiver can influence by his behavior. Thus, the deceiver would intend to induce the target to believe that a predator is in the bush and thus induce a change in the target’s intention to attack.
How could one operationalize such an interpretation, for example that a baboon attributes intentions to his conspecifics? As in the Dasser study, one could experimentally expose the subject primate to behavior sequences, preferably video scenes, and then see whether it forms a class of scenes that coextends with our category of intention. The weakness of this approach is that we cannot conceive and therefore cannot test for another kind of intention than our own. The difficulty of operationalizing mental states attributed by other species is staggering, but the anecdote may be hopeless. Whiten & Byrne (1988) are quite aware of all these and other levels of interpretations, but they still hope for a major role of anecdote. We believe that the necessary controls are principally unobtainable in the field.

**Demanding interpretation and intentional language**

Research into social cognition requires the thinking in alternative interpretations, and experiments are essential final steps. These are largely accepted, almost trivial rules, yet they are being disregarded. Why?

Three recent historical developments in ethology come to mind:

1. Unwarranted terminology in Primatology. With “Agonistic Buffering” and later “Reconciliation” it became acceptable to name a behavior by its function even when there was no evidence that something was buffered or that someone was reconciliated. The old ethological rule was to label a behavior by a descriptive name. We now see the same inflation of terms in cognition: “Politics” and “Tactical Deception”, for example, are suggestive of high-level intentional capacities in chimpanzees and baboons before the evidence of the claim is available. Sociobiology has introduced terms like Cheating and Altruism. One may ever so often assure that no intentions are implied, yet the words are intentional and have probably helped us to get away with intentional terminology in Cognition as well. Words are not just labels, they seep into the content of the bottle.

2. Donald Griffin (1976), in his influential “Question of Animal Awareness”, defended the legitimacy of bold hypotheses on animal mental processes. In his chapter “A possible window on the minds of animals” he sketched some methods, but his emphasis was not on methodology. His message was that animals most probably have minds and that we should work harder at developing methods of access to them. It is not productive to jump into the newly permissible niche, but with little concern for the second part of Griffin’s message.
3. The recent influence of philosophers and their use of "mentalese" language on ethology (Dennett, 1988) may have led us to use terms such as Belief, Desire or Want, even Cognition, in an everyday sense, before we understood what they mean.

Nevertheless, all these high claims have had good effects as well. If I find a bottle labelled "Château Lafite-Rothschild" I am more motivated to ascertain whether that is really true than when the label says: "Fermented fruit juice of undetermined quality". The claims have fired imagination and joined forces with our emotional hope that our animals are really clever.

Even so, it seems that we must argue once more for principles that seemed accepted.

Are experiments unavoidable?

Is the experiment really unavoidable for mental interpretations? Can the field observer contribute nothing but heuristically interesting anecdotal leads?

Lucky circumstances could convince us by observation alone that non-human primates attribute mental states to their conspecifics. Premack (1988) gives the lovely example of the fishing pond. The owner, in front of a boy standing ready at a distance with his rod, might stir up the water to show the absence of fish, and he might fish himself for hours without catching anything. If we as observers are certain that the owner knows that no fish are there, we can infer that the owner tries to install (and therefore attributes) a mental state in the boy, namely the belief that no fish are in the pond. We will do this if nothing supports the notion that the owner tried to affect only the behavior of the boy with the rod, such as threatening him.

Unfortunately, non-human primates have not been observed to perform behavior that could only be interpreted as a demonstration to another's mind. Even for the more modest goal of mapping an animals's knowledge about his society, there is a prerequisite that is not easily met without experimentation. The researcher must have as much knowledge as the animal subject about the relevant external object or event. I cannot judge the knowledge which a rat displays by running a maze unless I know that maze myself. I can infer a baboon's knowledge only to the extent that my knowledge of our common environment somehow overlaps with his. Ideally I should know more about his world than he does. Our technical knowledge overlaps sufficiently with that of a chimp.
to infer his knowledge about tool use. But with respect to the social maze he lives in, we are handicapped by ignorance. We do not sufficiently know what happened among our subjects last night and last year, and we lack much of their hard-wired social percepts and their acquired socialization. Our respective ignorance is, of course, the flaw of the anecdote as evidence.

Generally, only an experiment can reduce our handicap. That is why psychologists have built mazes and von Frisch put out artificial food for his bees. Some field workers (Strum, 1988; Whiten & Byrne, 1988) now see an alternative in observing all the relevant experiences of one primate in the wild by day-in, day-out focal observation over a long period. This enormous effort, applied to notoriously rare events, might be worthwhile in showing gradual build-ups of social skill in periods of rapid change, but we cannot believe in its conclusiveness because the relevant inputs will be swamped in a mass of potentially relevant experiences and developmental processes.

One source of preliminary descriptive evidence that has not been tapped might be a quantitative comparative collection by the same observer of defined classes of anecdotes of different species within the same environment — if that exists.

Parsimonious interpretations?

Parsimony of interpretation, though frequently advocated, is not a basic principle of science. If data cannot distinguish between alternative explanations the conclusion must be that both are possible, without a bias for the more parsimonious one. The reason why parsimony is nevertheless commendable in animal cognition is this: Our first naive interpretation tends to be the one we would apply to the behavior of another human, and most often it will attribute to the subject a mental state: "He has mislead her into believing that...". Thus the spontaneous, anthropomorphic interpretation is usually the most demanding we can think of: We (or our primate subject) cannot attribute intention to someone without also attributing to him causal knowledge that his chosen course of action will lead him to his goal. Since in principle we cannot think of any more cognitively demanding hypotheses than those we apply to social interactions spontaneously, all alternatives which we then must search are automatically more parsimonious. The call for parsimony is merely a necessary consequence of our anthropomorphic first choice. There are no scientific reasons for favoring the most parsimonious inter-
pretation of an anecdote; listing the alternatives is all that is needed. To put it differently: We learn nothing by jumping straight at the politics level of interpretation because in doing so we simply project our everyday percepts for human behavior. It is no more than applying a prejudice. It is not wrong to favor a demanding hypothesis, it is just not productive. By forestalling detailed research it deprives us of the opportunity to discover the fascinating social Umwelten and Innenwelten in which other species live.

In our last section, we want to address

**The Social Intelligence hypothesis**

The idea that primate intelligence was selected primarily by the demands of a complex social life is widely quoted in publications on primate social cognition (review in Byrne & Whiten, 1988). As far as we know it has never been operationalized, a phenomenon not atypical of the social cognition move. In spite of its protean vagueness, the idea is too important to be dismissed. It can be developed in several directions.

1. For example, we can ask: Can the social environment select for a different kind of intelligence than the extraspecific environment? Can we identify such specifically social cognitive abilities in primates but not in other mammals? And can we think of evidence that these social abilities were generalized to how primates deal with the extrasocial environment, with predators, prey, tools, and plant maturation schedules?

   What could the defining quality of social cognition be? Probably not ordinary causality. Michotte (1963) and recently Leslie & Keeble (1987) have shown that the percept of causality in human infants involves close contingency of two events in space and time. A ball hitting another, which then immediately departs, is an example. These immediate contingencies are rarely realized in social interactions. Here, the reaction often follows the action at a distance and with a delay, and few interactions in the group can be safely excluded as possible causes of an event.

   According to Dasser, Ulbaek & Premack (1989), the percept that bridges these gaps and orders events is that of intention. In their view, ascribing intentions plays the same role in our cognition of the social universe as ascribing cause does in our cognition of the physical universe. (Compare von Wright (1971) for a recent defense of the same view concerning the difference between the natural and the social sciences.) They present experimental evidence that young children have such a percept of intention. The children attribute it to video tapes of two moving balls.
For example, a large and a small ball appear on the screen. Small falls down a cliff and moves frantically. Large descends, carries up Small, and both depart. In another scene, Large stops. Small approaches and touches it. Large pushes Small away. Small jumps upon Large and is again pushed away. Large hits Small, returns to its original location, and Small leaves rapidly.

The Habituation/Dishabituation method was used to see how children grouped scenes. The children looked longer at the above scenes than at controls where the same movements were desynchronized. Dishabituation was stronger from scene to control or vice versa than within either class. This and some verbal comments by the children showed that they differentiated between what we class as intentional and the control scenes. Reversal of roles caused more recovery of looking time in the intentional than in the control scenes. No other interpretation appears conceivable to us adults than that the children had a percept of intention.

In order to test the social intelligence hypothesis in this form one might try to show that primates deal with nonsocial problems much as they deal with social situations, for example that they attribute intentions to objects, that they have animistic tendencies somewhat like human children, and, ideally, that their concept of intention often permits them predictions about objects that are better than predictions derived from a causation concept. There is at present no evidence on this, because to our knowledge no tests have been made to show that non-human primates have a concept of cause, and no primates other than chimps (Premack & Woodruff, 1978; Premack, 1988) have been shown to attribute intentions, or other mental states, to others.

2. Intention is a human social concept. A second approach begins with the assumption that non-human primates have hon-human social percepts. The primary but already complex percepts might be plausibly based on a refined ability to predict another’s behavior from the accelerations and directions of his whole body with regard to the subject’s own accelerations and directions, as in the relative movements of the two balls. Even without any reference to the other’s signals, the subject might predict whether the other body will harm him or simply follow him around. How accurately entire sequences of such trajectories are interpreted by at least a human observer is illustrated by an unpublished film by Heider & Simmel. This is an Esperanto of movement without signalling, a general medium of communication even between species. When an ethologist first learns the function of a primate’s signals as aggressive or appeasing, he relies on this Esperanto. Social cognition of this kind
could consist of several diverse percepts, each for the prediction of a particular set of another's behavior. There might be a percept for chasing, for gentle proximity seeking or for sneaking up from a hide. These percepts would neither be causal nor intentional in the human sense but unknown precursors with the function to classify movements of social bodies, one of which is the subject itself. The study of primate social cognition would thus begin in the domain of visual perception of movements of several bodies and would examine whether the performance of primates is superior in that respect to an equally complex task of perception in another domain — if such can be found.

This second approach could follow Paul Rozin's (1976) suggestion that the roots of intelligence are found in highly sophisticated assessment systems of the brain that were evolved for quite specific contexts. Later in an evolutionary process, such an "Adaptive Specialization" could become accessible to other brain systems functioning in other contexts. Insects have highly sophisticated skills — think of bee communication and the navigation of desert ants — but they have not generalized these computational abilities. The intelligence of mammals, in Rozin's view, would not excel by its local sophistication but by its wide accessibility across contexts.

This version of the social intelligence idea would have us search among lower primates for specializations evolved for specific social contexts. One possibility of this kind is the evolution of primate hunting skills. Primate social life requires spatial skills: In order to intervene or to use a protected threat one must predict fast movements of conspecific bodies if the right partners shall be spatially available or unavailable at the right time. This specialization, once evolved, could have become a preadaptation for the complex cooperative hunting in chimpanzees (e.g. Boesch & Boesch, 1989).

Another example is an experiment by Stammbach (1988) on long-tailed macaques. In each of 8 groups, he trained one subordinate group member to produce food for the whole group from an apparatus. As a result, group members began to groom the producers significantly more often between tests, particularly those who benefited most from him. This response mechanism might be as simple as "Groom the animal near whom you were rewarded". A more demanding version might require that the food appears only after a specific act of the producer. It might even have generalized to grooming a conspecific that "causes" an array of events to happen, or to a wider reciprocating mechanism with several inputs and outputs. This is a testable case where Rozin's ideas might
apply. Our own generalized sense of social reciprocation might have its phylogenetic origin in some quite local type of exchange rather than in the broad emergence of a theory of mind. The interest of the Rozin approach is that it could determine at which level of social cognition primates might have evolved superior abilities, and at which level they might have become transferred to non-social skills. It is this form of the social intelligence idea where the ethologist’s knowledge of the social life of his animals is useful, but unfortunately, at one time, he would have to return from the field to pursue it in earnest.

We believe that the path of small experimental steps is far more elucidating than the flight of fancy at the exciting, anthropomorphic interpretation of the moment, because in following this path we must form a mental landscape of all possible hypotheses and their operationalisation. The deeper excitement requires patience. We appreciate the heuristic and publicistic value of the anecdote, but we fear that primate politics and Machiavellian stratagems might share the fate of the social intelligence ‘‘hypothesis’’: That they will rest at peace at a stage of infancy.

Summary
The paper expresses the authors’ views on the growing interest in primate social cognition, particularly among descriptive primate ethologists. Its characteristics are the hope to extract cognitive interpretations from field anecdotes, the free use of intentional language, and the untested and so far untestable idea that primate intelligence was selected in social contexts. We believe that

1) To understand how the animal itself represents the structure of its group or its habitat is perhaps the most ethological ethology there is and well worth pursuing. The study of social cognition, in particular, has long been neglected.

2) However, it requires of ethologists that they learn from established cognitive science and integrate its categories with their own. This is an interdisciplinary enterprise.

3) A traditional inductive study begins with anecdotes, which then are translated into hypotheses, which in turn are subjected to empirical tests including experiments. Sociobiology began to publish hypotheses without tests; the social cognition move now goes on to publish anecdotes without hypotheses, with a strong penchant for anthropomorphic interpretations in terms of social manipulation. This is little more than applying human prejudice. Phylogenetic and cognitive insights will come from testing alternative levels of organization in an animal’s social knowledge about the same behavioral interaction. The experiment is the largely unavoidable method. Examples are given.

4) The speculation of the social origin of primate intelligence is tentatively interpreted in two possible directions. A version based on Rozin’s (1976) view that generalized mammal intelligence evolved from context-specific ‘‘Adaptive Specializations’’ seems the more accessible to ethological thinking and method.
References


Zusammenfassung

Der Vortrag enthält die Ansichten der Autoren über das wachsende Interesse beschreibend arbeitender Primatologen am Thema der sozialen Kognition. Man hofft, Feld-Anekdoten kognitiv deuten zu können, bedient sich vorwiegend intentionaler Sprache und zitiert häufig die alte, aber noch nie getestete und gegenwärtig auch nicht testbare Idee, die genetische Basis der Intelligenz der Primaten sei in sozialen Kontexten selektioniert worden. Unsere Meinung ist:

1) Zu verstehen, wie ein Tier die Struktur seiner Gruppe oder seines Wohngebietes in seinem Gehirn repräsentiert, ist vielleicht die ethologischste aller Ethologien und von außerordentlichem Interesse auch für evolutionäre Erkenntnistheorien. Die Erforschung sozialer Kognition von Tieren ist bisher weitgehend vernachlässigt worden.

2) Ihr Studium verlangt aber vom Ethologen, dass er von den etablierten kognitiven Wissenschaften lernt und ihre Kategorien mit den seinen integriert. Interdisziplinäre Zusammenarbeit scheint unerlässlich.
