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Précis of *How monkeys see the world*

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Abstract: Our book examines the mechanisms that underlie social behavior and communication in East African vervet monkeys. Our goal is to describe the sophistication of primate intelligence and to probe its limits. We suggest that vervets and other primates make good primatologists. They observe social interactions, recognize the relations that exist among others, and classify relationships into types. Monkeys also use sounds to represent features of their environment and compare different vocalizations according to their meaning. Monkeys may use abstract concepts and have motives, beliefs, and desires, however, their mental states are apparently not accessible: They do not know what they know. In addition, monkeys seem unable to attribute mental states to others: They lack a "theory of mind." Their inability to examine their own mental states or to attribute mental states to others severely constrains their ability to transmit information or to deceive one another. It also limits the extent to which their vocalizations can be called semantic. Finally, the skills that monkeys exhibit in social behavior are apparently domain specific. For reasons that are at present unclear, vervets exhibit adaptive specializations in social interactions that are not extended to their interactions with other species (although they should be).

Keywords: cognition; communication; consciousness; deception; intelligence; language; mental states; monkeys; social behavior; social judgment

How monkeys see the world reviews 11 years' research on social behavior and communication in East African vervet monkeys (*Cercopithecus aethiops*). Although our work falls squarely within the ethological tradition – a long-term study of a single species in its natural habitat – our broader objectives are explicitly interdisciplinary. We believe that ethologists can profit from an exposure to theories and research in cognitive science and that cognitive scientists have much to learn from research in evolution, animal behavior, and behavioral ecology. Questions central to all of these fields include:

What is the relation between communication, cognition, and social behavior? We typically assume that humans have more complex social relationships than other species, that this social complexity is linked to our cognitive abilities, and that language plays a crucial role in structuring social interactions. Comparative research on animals, however, indicates that the relation between cognitive and social complexity is far from clear: The sophisticated societies of insects, for example, often result from comparatively simple perceptual and behavioral mechanisms. Language, moreover, is not a necessary component of social complexity: Vervet monkeys live in highly complex societies, yet individuals apparently lack language, self-awareness, and an awareness of the mental states of others. What, then, are the elements of social behavior most crucially affected by language and cognition? Or, putting the question in evolutionary terms, how did language and cognition evolve? How would a particular cognitive skill give an individual bird, monkey or ape a selective advantage over others?

Chapter 1: What is it like to be a monkey?

We begin by outlining the book's contents and placing our methods and assumptions in historical perspective. We borrow from the philosopher Quine (1960) the example of an imaginary linguist who has entered an unknown land where none of the sounds people make are familiar. The linguist's goal is to construct a dictionary of the local language, to learn what each word means, and thus to begin to understand how the people think. But if a native shouts, "Gavagai!" when a rabbit appears, can the linguist conclude that *gavagai* means the same as *rabbit* in English? Can we ever really understand what the words, sounds, or communicative signs of an alien culture actually mean? Quine (and many other philosophers, often for different reasons) believes that precise understanding of word meaning is impossible even in our own species; attempts to understand another species are therefore certainly doomed to fail. We argue instead that even if Quine is ultimately proved right there is much to be learned from the attempt to understand how animals

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perceive their world and how these perceptions structure their social interactions.

Our research, like that of many ethologists, adopts an evolutionary, ecological perspective, first documenting what animals do in their natural habitat and then asking what sorts of underlying mental operations might account for this behavior. What are the problems vervet monkeys face in their daily lives? What do they *need* to know, and how might one method of obtaining, storing, and communicating knowledge give certain individuals a reproductive advantage over others? Unlike traditional laboratory tests of primate intelligence, research on group-living primates inevitably leads to a focus on social stimuli. We ask not what monkeys know about blocks, tones, blue triangles, or computer lexigrams but what they know about each other and about the vocalizations they hear during encounters with predators, other groups, or familiar companions. This focus on social behavior allows us to examine primate intelligence in the environment where it presumably evolved; it also allows us to compare the monkeys' performance when dealing with one another with their performance outside the social domain.

There is at present a tension in ethology, psychology, and cognitive science between traditional empiricists who disavow the use of mental terms like "concept" or "representation" and those who, in varying degrees, believe that such terms are useful, either as heuristic devices or because behavior simply cannot be explained without them (for reviews see Gardner 1987; Kamil 1987; Roitblat 1987). Regardless of the exact position they adopt, all participants in the debate agree that assumptions about the mental processes of others are controversial enough when we discuss our own species; when the minds of animals are at issue the problem becomes even more intractable.

Given these difficulties, our solution has been adopted as much from necessity as from conviction: We borrow the methods of the empiricists but place them tentatively within the framework of a more mentalistic approach. Because we cannot interview our subjects and ask them for introspective reports of their current or recent thoughts, we must, in conducting our observations and experiments, adopt a nonmentalistic position and study communication and behavior operationally, in terms of the responses they evoke in others. This method carries with it all of the limitations pointed out by Quine. Can we ever really know, from a study of responses alone, what "Gavagai!" means?

On the other hand, though armed with behaviorist methods, we are less agnostic about the mind as a causal agent than behaviorists might like us to be, and we cautiously adopt such words as *attribution, representation, consciousness,* and *strategy.* In using these terms, our approach is mentalist rather than behaviorist, as we assume that mental states are characterized not just by external environmental influences but also by reference to other mental states (Churchland 1984).

Our openness to the use of mental terms also means that, in spite of our efforts to avoid it, the word *cognition* occasionally appears and therefore requires definition. For present purposes we adopt Markl's (1985) definition of cognition as the ability to relate different unconnected pieces of information in new ways and to apply the results in an adaptive manner. This definition is useful to those who study animals because it examines cognition in terms of what individuals do without specifying or being limited to any particular mental mechanism that might underlie behavior.

Our approach to animal intelligence is not original; it borrows much from the writings of Kummer (1982), Premack (1983), Dennett (1983), Mason (1986), Kamil (1987), Gallistel (1989), and others. It is, however, a hybrid, combining the methods of behaviorists (who don't believe in mentality) with many interpretations of mentalists (who feel that the behaviorist approach is inadequate). This uneasy alliance of two historically different viewpoints recalls the comment of Saki's character Reginald (in the short-story compendium, "The Complete Works of Saki") who, when asked to name his religion, said, "The fashion just now is a Roman Catholic frame of mind with an agnostic conscience: You get the medieval picturesqueness of the one with the modern conveniences of the other."

Chapter 2: Social behavior

Chapter 2 reviews what we and our colleagues learned during 11 years' continuous research on the behavior and ecology of vervet monkeys in Amboseli National Park, Kenya. Although the primary focus is on vervets, we also draw on other research, emphasizing in particular studies of baboons and macaques.

In Amboseli, vervets live in groups of 10 to 30 individuals. Each group occupies an area roughly four hectares in size that it aggressively defends against incursion by other groups. Within groups there are anywhere from one to eight adult males, two to eight adult females, and their offspring.

As in many other Old World monkey species like baboons (*Papio cynocephalus*), rhesus macaques (*Macaca mulatta*), or Japanese macaques (*M. fuscata*), vervet females generally remain throughout their lives in the group where they were born. Males, in contrast, transfer to neighboring groups when they become sexually mature, at around four years of age. Adult females can be ranked in a linear dominance hierarchy, and offspring acquire ranks immediately below those of their mothers. As a result, vervet monkey groups actually consist of a hierarchy of families, with all the members of family A outranking all the members of family B, all the members of family B outranking all the members of family C, and so on. Even a male assumes his mother's rank until he transfers to another group. After that, a male's rank depends on more subtle factors, including fighting ability, age, and his acceptance by the adult females in his adopted group.

Vervet society is characterized by close bonds among matrilineal kin, who groom one another at high rates, often forage and sleep together, and come to one another's aid in alliances (alliances occur whenever two individuals are involved in an aggressive interaction and a third, previously uninvolved, animal intervenes to aid one of them in attack or defense). Bonds among maternal kin apparently have their genesis in the close relationship between a mother and her offspring (Kummer 1971).

By contrast, there is little evidence that vervets recognize their paternal kin. Adult males interact only rarely with infants and typically show no special preference for those infants that are likely to be their offspring (but see Hauser 1986). The monkeys behave as if they have no way of distinguishing who their fathers are and hence no way of distinguishing kinship through the paternal line. When we speak of kin, therefore, we are referring only to individuals related through the maternal line.

In addition to establishing close bonds among maternal kin, vervet monkeys attempt to form bonds with higher-ranking individuals – a strategy apparently designed to win some of the benefits associated with high rank even if an animal's actual status remains low. Evidence for such "status striving" (de Waal 1987) comes from data on alliance formation and grooming.

When an aggressive interaction occurs, vervet monkeys are given an opportunity to "choose" whether to intervene on behalf of one of the animals involved. Data on alliance formation indicate that, in addition to forming alliances at high rates with their kin, adult females and juveniles form alliances with individuals that are higher-ranking than themselves at higher rates per opportunity than they do with individuals that are lower-ranking. Similarly, vervets compete for the opportunity to groom particular partners and the most attractive partners are high-ranking individuals.

Why are females attracted to high-ranking individuals? We have argued that individual monkeys attempt to maximize the benefits they derive from such social interactions as grooming, much as they attempt to maximize the benefits derived from foraging or mate choice. High-ranking individuals are preferred social partners because bonds with these individuals can result in the greatest benefit to the individuals involved. Though such bonds may not actually increase an individual's rank, they may nevertheless allow lower-ranking animals to acquire some of the benefits enjoyed by higher-ranking group members. This argument rests on three points.

First, in many species, high-ranking females win the majority of the disputes they enter and have preferential access to scarce resources. Second, even among unrelated animals there is often a strong positive correlation between alliances and other cooperative behavior such as grooming or tolerance at feeding sites (e.g., Colvin 1983; Packer 1977; Seyfarth 1977). Animals that form alliances at high rates are also those that groom, feed, or play together most often (Cheney 1983; Lee 1983; Seyfarth 1980; see also below, and Walters & Seyfarth, 1987, who review data on other species). Third, we argue that these correlations reflect *causal* relations: If a low-ranking animal forms an alliance with a higher-ranking one, the higher-ranking animal will subsequently be more likely to support her partner in an alliance or allow her partner access to a resource she could not otherwise obtain. Alternatively, the higher-ranking animal may simply be less aggressive in the future toward her partner or her partner's offspring (Silk 1982). Experiments with vervets have shown that when one individual grooms an unrelated animal, grooming increases the recipient's willingness to respond to the groomer's subsequent solicitations for support (Seyfarth & Cheney 1984).

Status-striving is not limited to adult females, nor is it manifested only in grooming and the formation of alliances. Frans de Waal (1982; 1987), for example, describes a variety of interactions among male and female chimpanzees that cannot be explained except by assuming that the animals were selectively forming bonds (by means of grooming, alliances, tolerance, reconciliation, and reassurance) with those from whom they could potentially derive the most benefit (see also Nishida 1983).

Focusing on the stable core of vervet monkey groups – the adult females – we therefore conclude that three motives underlie much of female behavior: attraction to kin, deference to those of high rank, and a desire to increase their own status. These motives are not merely descriptive, but offer an explanation of why vervet society takes the form that it does. They account for what we observe, can be used to generate models that accurately predict social interaction in novel circumstances, and predict features of behavior that might otherwise have escaped our notice. For example, attraction to kin and to high-ranking individuals is additive and reinforces bonds within high-ranking families but counteracts and weakens bonds within low-ranking families. High-ranking families should thus be more cohesive than low-ranking families, a prediction that is borne out by observation.

The motives also make evolutionary sense. Attraction to kin reflects the evolutionary benefits to be gained through kin selection (Hamilton 1964); deferring to those of higher rank while simultaneously attempting to increase one's status reflects the best "mixed strategy" (Maynard Smith 1974) for animals in groups where high rank is often correlated with greater reproductive success. [See also Maynard Smith: "Game Theory and the Evolution of Behavior" *BBS* 7(1) 1984.]

There remains a crucial unanswered question, however. Has our analysis really revealed something about the essential nature of vervet monkeys, or has it only told us something about ourselves? After all, the motives we are attributing to monkeys are products of human minds, the minds of those who observe the monkeys. But the ultimate goal of our analysis should not just be social structure as understood by primatologists, but social structure as it appears to the animals themselves. No self-respecting anthropologist would return from two years with the Dobu content to report only what he thinks of Dobu social structure – the other half of the puzzle is what the Dobu think, and how their view of themselves differs from that of a foreign observer. Have the monkeys, who have certainly seen the same events and more, made the same deductions that we have? Do they *understand* kinship and dominance rank? Or are they just sleepwalking through life, acting out complex strategies without being in any sense aware of what they are doing? To probe further into the minds of our subjects, Chapter 3 presents a series of observations and experiments designed to test whether the regularities of social behavior we humans see might also exist in the minds of the monkeys themselves.

Chapter 3: Social knowledge

In a brief methodological digression, Chapter 3 describes the use of playback experiments to study vocal communication, social behavior, and social knowledge in primates. We began using this method in 1977, in collaboration with Peter Marler; colleagues in ethology, however, will recognize that many of the techniques we describe are not original but borrow extensively from work conducted on birds over the past 30 years.

To begin an experiment, we tape-record a vocalization from a known individual in a particular social context. Then, some days later, we select a subject, hide a loudspeaker in a bush or tall grass, begin filming the subject, and then play the vocalization while filming the subject's response.

Playback experiments can be designed simply to reproduce events that occur naturally. In such cases they allow us to present a call we know is coming, to select particular animals whose responses we want to measure, and to film their responses for subsequent analysis. Alternatively, playbacks allow us to confront the monkeys with a particular signal in a context where they are unlikely to have experienced it before. Such novel and perhaps even alarming material can elicit responses that reveal some of what animals know about features of their environment.

When conducting experiments, we set a number of conditions that must be met before a trial can proceed. For example, the subject cannot have been involved in interactions with a predator or another group during the previous hour, the animal whose call is being played has to be a member of the subject's own group and yet out of sight of the subject, no other experiment of the same type can have been conducted earlier the same day, and so on.

Despite these precautions, none of our experiments ever achieves the precision and control of some laboratory tests. We simply do not know everything that has happened to our subjects on the day they are tested, nor can we completely control the myriad contextual variables – birds, ungulates, insects, other monkeys – present under natural conditions. Despite our care in selecting equipment, no loudspeaker can ever perfectly duplicate a monkey's voice, and when we say "a speaker was hidden behind a tree or in a bush," this is a hopeful euphemism; a speaker may have looked well hidden to us, but almost certainly the vervets could often see it. Many of these problems can be alleviated by allowing different vocalizations to serve as each other's controls. If some aspect of our procedure in a given experiment is biased, for example, it should be equally biased in trials that involve different call types. In the end, we control what we can and hope that the benefits of experimenting on animals in their natural habitat will outweigh the imprecision of our techniques.

Our explanations of vervet behavior in Chapter 2 implied that monkeys possess a sophisticated knowledge of their social companions and the relations that exist among them. In Chapter 3 we offer some preliminary tests of this hypothesis.

Although individuals of many animal species seem to know a great deal about their own social relations, the social knowledge of primates is most striking when we consider what a monkey knows about the social relations of others in her group. A variety of evidence indicates that monkeys recognize the close associations that exist among others. For example, when we played back the scream of a two-year-old juvenile to a group of adult females, a significant number of females responded by looking at the juvenile's mother. They appeared to associate particular screams with particular juveniles and these juveniles with particular adult females. They behaved as if they recognized the kin relationships that existed among other group members.

As a second example, consider the phenomenon of redirected aggression. In many primate species, an animal that has been involved in a fight will "redirect" aggression and threaten a third, previously uninvolved, individual. Rhesus macaques (Judge 1982) and vervet monkeys do not distribute such redirected aggression randomly, but direct it toward a close relative of the prior opponent. Vervets were significantly more likely to threaten unrelated individuals following a fight with those animals' close kin than during matched control periods. This was not because fights caused a general increase in aggression toward unrelated animals. Instead, aggression seemed to be directed specifically toward the kin of prior opponents. Similar data emerge in patterns of reconciliation among longtailed macaques (*Macaca fascicularis* – Cords 1988), rhesus macaques (de Waal & Yoshihara 1983), stumptailed macaques (*Macaca arctoides* – de Waal & Ren 1988), patas monkeys (*Erythrocebus patas* – York & Rowell 1988), and chimpanzees (de Waal 1989; de Waal & Roosmalen 1979). Monkeys may also recognize the dominance relations of others – knowledge that allows them to construct a dominance hierarchy of their fellow group members.

Many animals appear to classify objects according to "concepts" – relatively abstract criteria that are not based on any single perceptual feature (Lea 1984). In oddity tests, for instance, subjects are presented with three objects, two of which are the same and one of which is different. They receive a reward only if they choose the different object. If a small number of stimulus objects are recombined trial after trial, subjects could achieve above-chance performance through associative learning. Many monkey species, however, achieve scores of 80 to 90% correct even when new stimuli are used for each problem and a given set of stimuli is presented for only one trial (e.g., Davis et al. 1967; Harlow 1949; Strong & Hedges 1966). Such levels of performance suggest that animals are using an abstract hypothesis, "pick the odd object." The hypothesis is called abstract because "odd" does not refer to any specific stimulus dimension, as does "red" or "square." Instead, oddity is a concept that specifies a relation between objects independent of their specific attributes (Essock-Vitale & Seyfarth 1987; Roitblat 1987).

Premack (1983; 1986) contends that such tasks as oddity tests require only judgments about relations between elements, not relations between relations. By contrast, judgments about relations between relations are involved in tasks like analogical reasoning. They are less fundamental and universal than judgments about relations between elements, and thus far have been demonstrated only in language-trained chimpanzees.

In his study of analogical reasoning in chimpanzees, Premack (1976; 1983) trained an adult female subject, Sarah, to make same/different judgments between pairs of stimuli. Once Sarah could use the words "same" and "different" correctly even when she was confronted with entirely new stimuli, she was shown two pairs of items arranged in the form A/A' and B/B'. Her task was to judge whether the relation shown on the left was the same as or different from the one shown on the right. Alternatively, Sarah was given an incomplete analogy (e.g., A/A′ *same as* B/?). Her task then was to complete the analogy in a way that satisfied this relation. Sarah performed these analogy problems with apparent ease.

Premack (1983) argued that the ability to form such abstract representations is enhanced by, and may require, language training. His claim is not that chimpanzees naturally lack the ability to reason abstractly. Instead, he believes that all primates have the potential for such skills but that only chimpanzees given language training are able to realize this potential.

Premack's tests prompt one to ask whether group-living primates might use abstract criteria to make relational judgments about their social companions. The data that most directly address this question come from a study conducted by Dasser (1988) on a group of 40 captive longtailed macaques. Dasser trained two adult females so that they could be temporarily removed from the group and placed in a small test room to view slides of other group members. In one test that used a simultaneous discrimination procedure, the subject saw two slides. One showed a mother and her offspring and the other showed an unrelated pair of group members. The subject was rewarded for pressing a response button below the mother-offspring slide. Having been trained to respond to one mother-offspring pair (five different slides of the same mother and her juvenile daughter), the subject was tested using 14 novel slides of different mothers and offspring paired with 14 novel pairs of unrelated animals (all of which included at least one adult female). The mother-offspring pairs varied widely in their physical characteristics. Some slides showed mothers and infant daughters, others showed mothers and juvenile sons or mothers and adult daughters. Nonetheless, in all 14 tests the subject correctly selected the mother-offspring pair.

Data on redirected aggression and reconciliation in vervet monkeys provide additional evidence that animals classify social relationships into types that are not restricted to the particular individuals involved. Our observations of vervets suggested that animal A was more likely to threaten animal B if one of A's own close relatives and one of B's close relatives had recently been involved in a fight. The same was true of reconciliation: Two unrelated individuals were more likely to engage in an affinitive interaction following a fight between their close kin than during matched control periods. Of course, the parallel is not exact: If the prior opponents were both adult females, this did not necessarily mean that the subsequent opponents would both be their daughters. Vervet families are simply too small for such perfectly balanced events ever to arise.

These results, we emphasize, are preliminary. They have not yet been replicated in other populations and possible confounding factors have not been eliminated entirely. Bearing in mind their speculative nature, however, this more complex form of redirected aggression in vervet monkeys supports Dasser's experiments in suggesting that monkeys recognize that certain types of social relationships share similar characteristics. When a vervet (say, A2) threatens an unrelated animal (B2) following a fight between one of her own relatives (A1) and one of her opponent's relatives (B1), A2 acts as if she recognizes that the relationship between B2 and B1 is in some way similar to her own relationship with A1. In other words, we may think of A2 as having been presented with a natural problem in analogical reasoning:

A1/B1 *same as* A2/?

A2 correctly completes the analogy by directing aggression to another member of the B family.

In summary, monkeys seem to recognize that bonds within families share similar features even when the individuals involved are of widely different ages and sex. Monkeys therefore seem capable of classifying relationships according to one or more abstract properties: They represent social relationships in their minds and compare relationships on the basis of these representations. Whether they are at all aware of what they are doing, or whether they can use this ability outside the context of social interactions, remains to be determined.

Chapter 4: Vocal communication

Early studies of primate communication drew sharp distinctions in neural control, development, and function, between nonhuman primate vocalizations and human language. Subsequent research paints a more complicated picture.

The vocal repertoires of nonhuman primates are far larger than scientists initially perceived them to be. The information each call contains is also more specific and less dependent on context than previously imagined. For example, vervets grunt to one another in a number of different social situations: when approaching a dominant member of their group, when approaching a subordinate, when observing an individual move into an open area, and on seeing another group. Even to an experienced human listener there are no immediately obvious audible differences among grunts, either from one context to the next or across individuals. Playback experiments, however, show that monkeys respond differently to each grunt type. Grunts to a dominant, for example, cause individuals to look toward the speaker, whereas grunts to another group cause individuals to look out, toward the horizon, in the direction the speaker is pointing. By their behavior, the monkeys seem to be saying that although their grunts sound more or less the same to us, to *them* each grunt transmits a specific sort of information. And because monkeys respond in consistently different ways to different grunt types despite variation in social context, we conclude that the information conveyed by grunts – like the information contained in human words – depends as much (or more) on a particular call's acoustical properties as it does on the circumstances in which it is given.

Free-ranging monkeys also use calls – for example, the grunt to another group – in a manner that effectively represents, or denotes, objects and events in the environment. Perhaps the best-known examples of such representational vocalizations are the alarm calls made by vervet monkeys in response to different predators, first described by Struhsaker (1967).

Vervets produce a loud, barking alarm call in response to leopards (*Panthera pardus*) and such other cat species as caracals (*Felis caracal*) and servals (*Felis serval*). We refer to this call as the vervets' "leopard alarm." When vervets on the ground hear a leopard alarm they run into trees, where they are safe from a leopard's attack. In contrast, vervets produce an acoustically different call (their "eagle alarm") in response to two species of eagle, the martial eagle (*Polemaetus bellicosus*) and the crowned eagle (*Stephanoetus coronatus*). Vervets on the ground respond to eagle alarms by looking up or running into bushes. Finally, when vervets encounter pythons (*Python sebae*), they produced a third, acoustically distinct, "snake alarm." On hearing a snake alarm, vervets on the ground stand bipedally and peer into the grass around them.

Struhsaker's description of vervet alarm calls initially attracted attention because the monkeys seemed to be using different vocalizations to denote different external referents (e.g., Altmann 1967; Marler 1977), an interpretation that directly contradicted views of primate vocalizations held at the time. Nevertheless, legitimate doubts were raised. For example, the psycholinguist John Marshall claimed, "Even the alarm calls of the vervet monkey which seem, superficially, to be 'naming' the type of predator are more plausibly regarded as expressing no more than the relative intensity of the fearful and aggressive emotions aroused by the various predators' (1970, p. 234). Given the information available at the time, Marshall was appropriately conservative in his interpretations. He distinguished between calls that provide information only about the signaler's emotional state or subsequent behavior (a relatively simple, straightforward explanation) and calls that denote a specific external referent (an explanation that implied more complex cognitive processes). There seemed no need to attribute sophisticated mental processes to vervet monkeys when simpler mechanisms could adequately account for their behavior. Not discussed was the possibility that calls might *both* denote external referents and signal other sorts of information.

Working jointly with Marler, we found that playback experiments conducted in the absence of predators evoked responses similar to those originally observed by Struhsaker. Playback of leopard alarms caused a significant number of monkeys to run into trees; playback of eagle alarms caused them to look up into the air or run into bushes; and playback of snake alarms caused them to stand on their hind legs and peer into the grass around them. There was, moreover, evidence that alarm calls did not simply reflect the caller's degree of fear or excitement: When we altered our tapes so that calls were made longer or shorter, louder or softer (two variations we assumed might approximate changes in a caller's level of excitement), this had no effect on the qualitatively different responses to each alarm call type. And because we presented alarm calls when there were no real predators around, we could rule out the hypothesis that the monkeys' different responses depended on what they had seen rather than on what they had heard. In summary, each type of alarm call, presented on its own, elicited the same response as would a particular predator itself. We concluded that alarm calls functioned as representational, or semantic, signals.

The use of alarm calls, grunts, and many other nonhuman primate vocalizations seems to be under relatively voluntary control, because call production can be conditioned in the laboratory (reviewed in Steklis & Raleigh 1979), and group-living animals routinely give specific vocalizations only in particular circumstances (e.g., Smith et al. 1982; Snowdon & Hodun 1981). Primates make subtle acoustic discriminations when distinguishing between calls, and one well-studied case provides evidence of left hemispheric specialization in vocal perception.

Vocal development in primates exhibits many parallels with the early stages of speech development in young children. [See Greenfield: "Language, Tools and the Brain: The Development and Evolution of Hierarchically Organized Sequential Behavior" *BBS* 14(4) 1991.] Monkeys begin by using certain vocalizations – some clearly pronounced, others more garbled – in particular social situations. They behave as if they were predisposed to divide events in the world around them into broad categories that require a grunt, a scream, an alarm call, or no vocalization at all. Over time, pronunciation improves and infants sharpen the relation between a call and the objects to which it refers. Older infants and juveniles begin to recognize that within each broad context there is a further subdivision into circumstances that call for a specific kind of grunt, a particular sort of scream, or an acoustically different alarm. Throughout development, comprehension precedes production.

Because nonhuman primates use vocalizations to signal *about* things, research on communication offers a glimpse of how they see the world. Studies of vocal communication reinforce the conclusion, already obtained from studies of social behavior, that monkeys classify members of their own species according to group membership, dominance rank, and behavior. Through vocal communication monkeys also reveal their knowledge of other individuals' social relationships and their knowledge of other species.

But what, in the end, do monkeys really *mean* when they vocalize to one another? Can we actually define "leopard alarm" or "grunt to a dominant" in the same way we define words like anarchist, bordello, or sycophant? Thus far, we have been deliberately vague where questions of meaning are concerned in order to present necessary background information. This done, we consider what it all means in Chapter 5.

Chapter 5: What the vocalizations of monkeys mean

We have called the vocalizations of vervet monkeys semantic signals and drawn an analogy with human words because of the way these calls *function* in the monkeys' daily lives. When one vervet hears another give an eagle alarm call, the listener responds just as he would if he had seen the eagle himself. It is tempting to suppose that in the monkey's mind the call "stands for" or "conjures up images of" an avian predator even when the bird itself has not yet been seen. The same is true of leopard alarm calls, grunts to neighboring groups, and many other nonhuman primate vocalizations.

Clearly, however, descriptive evidence of this sort does not elevate animal signals to the status of human words. Consider first the problem of whether a signaler intends (wants or desires) to communicate with others. In

Pavlov's classic experiments, dogs heard a bell every time they were given meat. After a while they began salivating whenever they heard the bell, even if meat did not appear. To Pavlov's dogs, bells evoked the same response, or "stood for," or "conjured up images of," meat, even when the meat wasn't there. This hardly proves that the bells intended to communicate to the dogs in the way that a human intends to communicate when he says, "Dinner is served," or, "Your understanding of semantics is really very superficial."

One issue we must confront, therefore, concerns the factors that cause one animal to vocalize in the presence or even the absence of another. Given that listeners treat vocalizations as conveying a particular sort of information, is it also the case that signalers *intend* for them to do so? As a guide to analyzing what may be the mental states of signaler and recipient, we draw on the philosopher Dennett's (1971; 1983; 1987) scheme for assessing levels of intentionality in animal vocalizations.

Dennett argues that there is much to be gained from assuming that animals are *intentional systems*, capable of mental states like beliefs and desires. But what kinds of beliefs and desires? Here Dennett's different "levels of intentionality" provide us with a number of alternative hypotheses. A vervet giving an alarm call, for example, could be a *zero-order intentional system*, with no beliefs or desires at all. A zero-order explanation holds that vervets give alarm calls because they are frightened. Alternatively, vervets might be *first-order intentional systems*, with beliefs and desires but not beliefs *about* beliefs. At this level, vervets give leopard alarm calls, for example, because they believe there is a leopard nearby or because they want others to run into trees. The caller does not need to have any conception of his audience's state of mind, nor need he recognize the distinction between his own and another animal's beliefs.

Vervets might also be *second-*, *third-*, or *even higher-order* intentional systems, with some conception about their own and other individuals' states of mind. A vervet monkey capable of second-order intentionality gives a leopard alarm call because he wants others to believe that there is a leopard nearby. At higher and increasingly baroque levels, both the signaler's and the audience's states of mind come into play. At the third level of intentionality, vervets give an alarm call because they want others to believe that they want them to run into trees.

Despite some recent progress, we still know relatively little about the cognitive mechanisms that might underlie vocal signals. At one extreme, simple explanations that posit no mentality at all (e.g., "monkeys give alarm calls because they are excited") can be ruled out for a variety of reasons – not just in the case of monkeys, but for many other animals as well (e.g., Marler et al. 1990). On the other hand, there is as yet no evidence that monkeys attribute mental states to others. Taking an intermediate position, we conclude that vervet communication is most consistent with Dennett's first-order intentionality: Monkeys give leopard alarms because they want others to run into trees, not necessarily because they want others to think that there is a leopard nearby.

As a second approach to the study of call meaning, we examine the relation between a vocalization and the objects or events it denotes. Here, once again, there are reasons for caution in drawing parallels between monkey vocalizations and human words.

When humans use words like "apple" or "eagle," we recognize the referential relation that holds between such signs and the things for which they stand. Referential relations can, for example, be distinguished from causal relations (Premack 1976): The word "eagle" does not cause a particular bird to appear or result in a particular pattern of behavior. Instead, the word stands for, or represents, an object even when it cannot be seen. We know, moreover, that there is no obligatory relation between the acoustic features of a word and its referent. When comparing words, we judge them to be similar or different not on the basis of their physical properties but on the basis of their meaning. Words that sound different, like treachery and deceit, are judged to be similar if they mean the same thing, whereas similar sounding words, like treachery and lechery, are judged to be different if they have different meanings.

To paraphrase Premack (1976), a monkey's call is a word if and only if the properties ascribed to the call are not merely those of a sound but those of the object it denotes. As a test of this hypothesis, we carried out habituation/dishabituation experiments in which vervets were asked to compare vocalizations with either (a) similar referents but different acoustic properties, or (b) different referents and different acoustic properties. Results suggested that vervets compare different calls on the basis of their meaning and not just their acoustic properties. When subjects heard from the same individual two acoustically different calls with similar referents – a *wrr* and *chutter* given to members of another group – they transferred habituation across call types. By contrast, when subjects heard from the same individual two acoustically different calls with different referents – in this case, a leopard alarm and an eagle alarm – they did not transfer habituation across call types.

Compared with our earlier experiments on the vervets' alarm calls, these tests addressed the question of meaning and reference more directly by asking animals to compare two vocalizations (i.e., to make a same/different judgment between them) and to reveal the criteria they use in making their comparison. Results suggest that when one vervet hears another vocalize the listener forms a representation of what that call means. And if, shortly thereafter, the listener hears a second vocalization, the two calls are compared not just according to their acoustic properties but according to their meanings. If we accept the notion that a monkey's call becomes a word when the properties ascribed to the call are not those of a sound but those of the object it denotes, *wrrs* and *chutters* seem to have become words. Vervets present us with a rudimentary semantic system in which some calls, such as leopard and snake alarms, are markedly different in meaning whereas others, like *wrrs* and *chutters*, are linked to a common referent and can be used to represent shades of meaning within a general class.

It would obviously be as incorrect to claim that monkey vocalizations convey information *only* about external referents as it is to argue that they convey information *only* about the signaler's motivational state or subsequent behavior. Clearly, as Smith (1986; 1990) has argued, whenever one animal vocalizes to another, a variety of information is made available – information about the identity and physical characteristics of the signaler, about what the signaler is likely to do next, and about events in the environment. From the listener's point of view, the sort of information that is most important will vary from one situation to the next. In human language, where the existence of external referents is not disputed, words and sentences convey information about a speaker's identity, mood, and subsequent behavior in addition to information about particular external referents (e.g., Johnson-Laird 1987). Language, in this respect, is both expressive and denotative: It conveys the emotions, thoughts, and beliefs of the speaker while simultaneously referring to objects or events in the external world. In emphasizing the external referents of primate vocalizations, our aim is not to minimize the importance of either emotion, contextual cues, or the caller's subsequent behavior. Instead, we hope to show that the communication of monkeys and apes – long known to be highly expressive (e.g., Jolly 1985) – can be denotative as well.

But what, precisely, do their calls denote? This is difficult to state, for two reasons. First, call meaning cannot be described in absolute terms (e.g., "'snake alarm' means the African python, *Python sebae*"). Instead, the meaning of a call can only be stated relative to the meaning of other calls in a species' repertoire. Among vervets, for example, "snake alarm" denotes something that is both different from the objects that elicit eagle and leopard alarms and different from objects that elicit no alarm at all, such as harmless snakes or lizards. Our dictionary of vervet words thus builds slowly, with each word being defined in relation to the others. Under these conditions, as Quine's (1960) imaginary linguist would no doubt have discovered, the more words you have the more precise your definitions can be. For us, in the land of vervet monkeys, words are relatively few and their meanings are ill-defined.

Our assessment of call meaning is also imprecise because we cannot yet tell whether a vervet's call should be glossed as a word (simply, "snake") or as a proposition ("Snake! Let's approach and mob it!"). Hence we make no absolute distinction between a call that provides information about an external referent and a call that combines referential information with information about the caller's attitude or disposition toward that referent.

Finally, we note that our inability to state the precise meaning of vervet vocalizations is not peculiar to work on nonhuman species, but parallels similar difficulties in the assessment of word meaning among very young children.

Chapter 6: Summarizing the mental representations of vocalizations and social relationships

To this point we have made two claims about the knowledge that underlies social behavior and communication in vervets and other nonhuman primates. First, we have suggested that in their social interactions monkeys do not simply associate some individuals with others but instead classify relationships into types. "Mother-offspring bonds" or "bonds between the members of family X" are abstractions that allow different relationships to be compared with one another. Second, we have argued that monkeys classify sounds according to the objects and events they denote. As in their assessment of social relationships, the monkeys seem to represent a sound's meaning in their mind and compare different sounds on the basis of these representations.

Although in each of these instances there seems to be a strong case for mental representations, or concepts, it is important to emphasize again how little we know about what actually exists in the minds of our subjects. At this stage, for example, it is unclear precisely how the monkeys' representations might differ from associations formed through classical conditioning, associations which can themselves be extremely complex (e.g., Dickinson 1980; Rescorla 1988). Similarly, we cannot specify how much information is contained within a representation, how the information is structured, or how it is coded in the nervous system. We can, however, consider what representations are good for and how under natural conditions monkeys might benefit from having them. Perhaps the content of representations can be elucidated, at least in part, from their function (Fodor & Pylyshyn 1981; Herrnstein 1990).

We argue that the monkeys' ability to represent social relationships has evolved because it offers the most accurate means of predicting the behavior of others (see also Humphrey 1976; 1983; Whiten & Byrne 1988). But there are also other advantages. Because relationships conceived in this way are abstractions, they can be more parsimonious and simpler than absolute judgments, which require learning the characteristics of every interaction (Allen 1989; Dasser 1985; Kummer 1982; Premack 1983). If a monkey can assess the relationships of others – rather than having to remember or observe all of their interactions – he may be able to predict what opponents will do next even when he has seen them interact only once or twice. In other words, a monkey would be considerably better off if he had some representation of a social relationship.

A similar argument can be made for the representation of meaning in vocal communication. Assume, for the moment, that we are dealing with cases in which signalers gain by providing truthful information to their audience – say, a female communicating with her offspring or a close relative (the problem of deception is discussed in Chapter 7). In these circumstances, we argue that the monkeys' ability to represent the meaning of vocalizations has evolved because of the advantages that accrue to individuals who can interpret sounds without relying on either contextual cues or the behavior of those who vocalize. In more complex systems of communication, the ability to link one or more vocalizations with a common referent (*wrrs* and *chutters*, for instance) and to compare calls on the basis of the things they represent, allows individuals to develop a rich semantic system in which some calls, like eagle and snake alarms, are markedly different, while others, like *wrrs* and *chutters*, show more subtle differences from each other and can be used to represent shades of meaning within a general class.

Beyond this, however, our understanding of monkeys' representations of social relationships and call meaning is fuzzy and imprecise. Given the difficulty of stating precisely what is contained in a monkey's mind, we turn instead to what may not be.

First, even if monkeys do distinguish among different types of social relationships, their ability to compare relationships may be relatively inflexible and limited to circumstances in which the individuals involved are familiar. In all the studies described to date, subjects have of necessity been tested only with the familiar social companions that make up their group. As a result, we cannot state conclusively that a monkey confronted with an entirely new set of individuals – a young male transferring into a new group, for example – would be predisposed to look for close bonds among matrilineal kin, linear dominance relations, and so on. More to the point, how long would it take for a vervet or baboon to learn that not all primate species have the same patterns of social interaction? If a vervet male transferred into a gorilla group, where females are seldom closely related (Stewart & Harcourt 1987), how long would it take for the male to *cease* expecting the females to interact at high rates? Would he ever?

Second, although monkeys may be able to represent social relationships, their ability to make use of such representations in reasoning or computation may be limited. Representations of social phenomena may therefore differ from the representations of rate, time, and space used by birds and other animals when computing, for example, feeding returns at alternative food patches (Gallistel 1989).

Third, as we noted in Chapter 3, we have no evidence that monkeys can label social relationships or give names to the criteria they use in classifying them. Although certain primate vocalizations do function in a manner that effectively labels different predators (e.g., leopards and eagles) or different classes of conspecifics (e.g., dominant or subordinate) we do not know whether monkeys have calls referring to "close partners," "friends," or "enemies" that could be used to classify relationships. Whether they could learn such terms under the appropriate conditions remains an open question: None of the ape language studies has ever asked subjects about each other's relationships. If the presence or absence of a label is some measure of an individual's awareness of classes and of relations between classes, then the ability of monkeys to compare relationships and generalize to novel social situations may be severely limited.

The monkeys' apparent lack of vocalizations to label superordinate classes may be symptomatic of a larger problem: The monkeys may be unaware of their own knowledge. In Rozin's (1976) terms, a monkey's knowledge of social relationships or word meaning may be *inaccessible*. Whereas monkeys can classify familiar relationships into types and even compare social relationships involving different individuals, they may not be able to examine their own knowledge, label it, apply it to new stimuli, or use it to deduce new knowledge. In addition, perhaps because monkeys cannot reflect on what they know about others, they may be unable to attribute motives and hence understand *why* some relationships are alike and others are quite different.

We have argued that monkeys, to succeed socially, must be able to predict the behavior of others. To do this well, they cannot rely on memorizing single interactions but must instead deal in abstractions, deducing the relationships that exist among others. For humans, the quest to predict behavior prompts us to search still further, for the knowledge, motives, and beliefs that cause some relationships to differ form others.

In the end, we confront the relation between an individual's behavior and his recognition of the mental states of others. Given that monkeys recognize relationships, do they understand the motives that underlie them? Given that monkeys can communicate selectively about things, do they use these calls simply to affect each other's *behavior*, or do they ever attempt to change each other's *minds?* These questions strike at the heart of how monkeys see the world and lead us directly into the next three chapters on deception, attribution, and the limits of primate intelligence.

Chapter 7: Deception

Whether or not animal signals are considered "truthful" or "deceptive" depends to some extent on the sorts of data one decides to emphasize. In most cases, signals do seem to provide accurate and reliable information to those nearby. In other cases, however, they clearly do not. Here we use an operational definition: A deceptive signal is one that provides others with false information. We review some of the evidence for deceptive communication and the detection of unreliable signals in primates and other animals. We describe the form that deceptive signals take and discuss their possible function, concentrating in particular on signals that convey information about the environment or the signaler's probable behavior, because these signals offer the widest scope for modification. Our primary purpose is to provide a framework, however rickety, within which we can consider the far more speculative and controversial issue of what deception might tell us about other animals' minds. We reserve this topic for Chapter 8.

Deception can take a number of forms. Among group-living animals, where the risk of detection is greatest, one of the most effective means of deceiving others is through silence: Withholding information that might be beneficial to others. Although signal concealment involves no active falsification of information and cannot distract attention from one event to another, it can certainly *function* to deceive.

Kummer (1982) reports observing a female hamadryas baboon who spent 20 minutes gradually shifting her way in a seated position toward a rock where she began to groom a subadult male – an act that would not normally be tolerated by the dominant adult male. From his resting position, the dominant male could see the back and head of the female, but not her arms. The subadult male sat in a bent position and was also invisible to the dominant male. What made Kummer doubt that this arrangement was accidental was the exceptionally slow, inch-by-inch shifting of the female toward the rock (for many similar accounts see Whiten & Byrne 1988).

What do such anecdotes tell us? They suggest, but clearly do not prove, that nonhuman primates do not simply monitor physical aspects of their world, such as the location of a food item or another individual; they also monitor and predict the mental states of other animals and the effects of their own behavior on the behavior of others. What is presently lacking is a method for systematically observing the frequency and consequences of such apparent attempts at deception. More important, we need some way of discriminating between explanations that assume that animals have the ability to monitor the thoughts of others and simpler interpretations that make no such assumption. In the case of the female hamadryas baboon, for example, explanations based on learned behavioral contingencies are as plausible as those based on mental attributions; the female could have groomed the subadult male behind a rock simply because she had learned from past experience that she could avoid attack by grooming other males out of sight of the dominant male.

Choosing between simpler and more complex explanations becomes particularly difficult when we attempt to compare apparent acts of deception across species. Although both chimpanzees and house sparrows, for example, modify the rate at which they utter food calls depending on the size of the food source (see Chapter 5), many investigators are inclined to believe that chimpanzee food calls are governed by mechanisms that differ from those of house sparrows. In the absence of any systematic information about the flexibility and modifiability of calls in each of these species, however, we are left simply with two very similar patterns of behavior.

Those taking an exclusively functional or evolutionary perspective (e.g., Krebs & Dawkins 1984) might argue that the mechanisms underlying the food calls of chimpanzees and house sparrows are irrelevant as long as the calls *function* to manipulate others. Mechanisms become more important, however, if we want to use deception as a means of studying the mental states and capacities of animals, or if we want to understand the constraints within which communication operates and the different forms that manipulation can take.

More direct manipulation can occur through the active falsification of signals. This strategy will be most effective if it occurs at low rates and if the circumstances surrounding successive acts of deception are varied – for example, if a false food call is subsequently followed by a false alarm call rather than simply by another false food call. Variation in the context of deception, at least among vervet monkeys, potentially allows individuals to maintain the highest rates of deceptive signaling without producing permanent skepticism among others in their group.

At the moment, we have no evidence that any animal species regularly varies the rate and context of false signals. Through more systematic observations and experiments it should eventually become possible to determine whether the intriguing anecdotes reported in the literature represent, at least in some cases, intentional signal falsification, as well as to specify more precisely the constraints under which deceptive communication is practiced.

Chapter 8: Attribution

When humans try to deceive each other, we try among other things to alter what another individual thinks. Conversely, to detect deception we must be able to read another's mind: to distinguish, for example, between a person who seeks help and genuinely intends to reciprocate and one who seeks help for more selfish reasons. The supplicant's behavior may in each case be the same – it is the difference between states of minds that must be detected. In other words, deceit and its detection assume (at least in the case of humans) that individuals can attribute mental states to others. Can the same be said of animals? Is it valid to assume, in Premack and Woodruff's (1978) words, that animals have "a theory of mind"? In Chapter 8 we consider the question of attribution and ask whether monkeys and apes recognize the existence of mental states in others.

The study of attribution in monkeys and apes is still in its infancy and continues to rely heavily on intriguing but largely unsubstantiated anecdotes. Although it is difficult to summarize an issue for which there is only a small amount of confusing and sometimes contradictory data, we risk a few speculations.

Monkeys and apes do occasionally act as if they recognize that other individuals have beliefs, but even the most compelling examples can usually be explained in terms of learned behavioral contingencies, without recourse to higher-order intentionality. What little evidence there is suggests that apes, in particular, may have a theory of mind, but not one that allows them to differentiate clearly or easily among different theories or different minds. Indeed, we cannot even state conclusively that nonhuman primates attribute ignorance to each other. The problem may be at least partially related to the animals' inability to recognize and represent their own knowledge.

Many different examples, reflecting many different approaches to the same problem, all support the hypothesis that monkeys are unable to attribute mental states to others. First, although monkeys can easily learn the necessary steps to complete a task, they apparently find it more difficult to learn the roles of others, perhaps because they cannot impute motives to other individuals. Second, although they do attempt to deceive each other, monkeys' attempts at deception seem aimed more at altering their rivals' behavior than at affecting their rivals' thoughts. Third, although their vocalizations certainly function to alert others to the presence of food, danger, or each other, we have no evidence that monkeys ever communicate with the intent of changing a listener's mental state or of drawing the listener's attention to their own mental state. Monkeys do not adjust their behavior according to whether or not their audience is ignorant or informed, perhaps because they do not recognize that such mental states exist. Fourth, although monkeys are clearly able to acquire novel skills from others through observation, social enhancement, and trial and error learning, there is little evidence that they imitate each other, again perhaps because they are unable to impute motive. Fifth, monkeys do not teach each other. Again, we would argue that this lack of pedagogy reflects the animals' inability to distinguish between their own states of mind and the states of mind of others. Sixth, although monkeys experience such emotions as fear and grief, they show no evidence of compassion or empathy and do not seem to recognize emotions in others. Finally, although monkeys are adept at recognizing their own position in a social network or dominance hierarchy, they show little self-awareness. This, too, is consistent with the view that monkeys do not know what they know and cannot reflect upon their knowledge, their emotions, or their beliefs.

Many of these generalizations may apply more to monkeys than to apes. Indeed, having gone so far as to suggest that monkeys, for the most part, lack a theory of mind, we speculate further and predict that many of the most fundamental differences between the minds of monkeys and the minds of apes will ultimately be traced to the apes' superior skills in attributing states of mind to each other (see also Mason 1978).

Although most of the data are anecdotal, there is strong suggestive evidence that chimpanzees, if not other apes, recognize that other individuals have beliefs and that their own behavior can affect those beliefs. Unlike monkeys, chimpanzees seem to understand each other's goals and motives. They deceive each other in more ways and in more contexts than monkeys, and they seem better than monkeys at recognizing both their own and other individuals' knowledge and limitations.

At the same time, however, chimpanzees may be like very young children in failing to attribute false beliefs to others. There is very little evidence that chimpanzees recognize a discrepancy between their own states of mind and the states of mind of others. They show little empathy for each other and do not explicitly teach each other. One study conducted with captive subjects has suggested that chimpanzees may be able to distinguish between a human trainer who is ignorant about the location of food and one who is knowledgeable. To date, however, there is no comparable evidence from free-ranging chimpanzees to suggest that signalers recognize whether their audience is ignorant or knowledgeable when, for example, they utter food calls.

Why is the attribution of mental states so important? Consider just one example of what one *cannot* do if one lacks a theory of mind. Suppose there exists a group of macaques in which one animal, like the famous Japanese macaque Imo, suddenly develops a new method for acquiring and preparing food. If the inventor deals only with behavioral contingencies, there is relatively little adaptive value to be gained from her discovery. She *can* feed herself better and raise healthier offspring. Other animals may learn that she, alone among her companions, can acquire this kind of food, and this may cause others to approach her when she is preparing the food and to handle the food as she does. Through such social enhancement, her offspring might eventually also acquire the skill. The female might also become a more attractive social partner to others, and her attractiveness might allow her to establish a relationship with a high-ranking female or male that might otherwise not have developed (Stammbach, 1988, provides a good example).

If the inventor can attribute ignorance to others and understands that mental states can affect behavior, however, there is an immense amount to be gained. Inventors who have a theory of mind can selectively transmit their knowledge to kin, much as they can selectively distribute their grooming. They can also selectively *withhold* their knowledge from rivals, much as they selectively withhold such other cooperative behavior as alliances. Finally, if inventors can recognize the difference between their own knowledge and that of others, they need not depend on the relatively slow process of observational learning to transmit their skill, but can instead engage in active pedagogy. Once again, an individual capable of attribution would seem to have a clear selective advantage over others.

We offer this hypothetical example to emphasize that the existence (or lack) of a theory of mind, long recognized as an important watershed in children's cognitive development, also has considerable evolutionary significance. Once individuals recognize that their companions not only behave but also think, desire, and have beliefs about behavior, they become much better social strategists and can use their knowledge much more skillfully to their own and their relatives' benefit.

The probability that a monkey with a typewriter would produce the complete works of Shakespeare is one in many billions. Even given thousands of years, the right combinations of letters and spaces would simply never arise by chance (Dawkins 1986). Monkeys would have to rely on chance because they lack a theory of mind. Even if a monkey could type and describe his characters' behavior, he could not reveal their minds. And without such attribution there could be no tragedy or comedy, no irony, and no paradox.

Chapter 9: Social and nonsocial intelligence

One of the striking features of adult human intelligence is our ability to apply what we have learned in one context to other, quite different circumstances. We take for granted, for example, that the basic rules of physics apply equally well regardless of whether we are playing billiards, hanging a picture, or building a house. In short, the knowledge we possess is in many cases accessible to us – we are aware of it, or know that we know it (Rozin 1976).

In contrast, animals often seem to have a kind of "laser beam" intelligence – extraordinarily powerful when focused in a single domain but much less well developed outside that narrow sphere. Birds (e.g., homing pigeons) can navigate enormous distances using the sun, the stars, geographical markers, or a magnetic sense to guide them (reviewed in Gould 1982). Honey bees dance to inform one another about the distance, size, and location of a food source, compensating as the sun changes its angle throughout the day so that the flight from hive to food is always indicated as a straight line (Gould & Gould 1988; von Frisch 1967). And yet we rarely think of animals like homing pigeons or bees as intelligent in the human sense, primarily because their sophisticated performance seems limited to specific, highly circumscribed spheres. In Rozin's (1976) terms, the homing pigeon's navigational and the bee's communicative skills are "inaccessible": The animals don't know what they know and cannot apply their knowledge to problems in other domains.

Most monkeys and apes are social creatures, and their expertise in the domain of social interactions is striking. But how accessible is their intelligence? Do they understand how much they know? Our first attempt to grapple with the question of "metaknowledge" came in Chapters 3 and 8, where we explored the extent to which monkeys not only engage in flexible, adaptive behavior but also *recognize* that they and others do so. Chapter 9 approaches the issue from a different angle, by comparing the performance of nonhuman primates in their social interactions with their performance outside the social domain.

It has been hypothesized (e.g., Humphrey 1976; Jolly 1966) that primate intelligence (including our own) originally evolved to solve social problems and was only later extended to problems outside the social domain. The argument predicts that at least some of the abilities that vervets and other primates demonstrate in their social interactions will be relatively inaccessible and not easily generalized to nonsocial problems.

Supporting this view, we find that vervets do not seem to attend to many aspects of their physical environment, even when to do so would be adaptive. Although the monkeys do recognize and respond to the different alarm calls given by birds and nonprimate mammals, they appear to ignore the visual and behavioral cues associated with some predators. Apparently, they do not recognize the relationship between a python and its track, nor do they understand that a carcass in a tree indicates a leopard's proximity, even though they have had ample opportunity to learn such associations.

Similarly, although vervets and other primates exhibit many forms of cooperation and reciprocity in their social interactions, comparable behavior using nonsocial currency (e.g., food sharing) is relatively rare. Monkeys behave altruistically and form alliances to achieve social goals, but they seldom cooperate to learn new ways of exploiting food.

This is not to say that primates never face – nor solve – challenging ecological problems. Clearly they do, as many field studies have shown (e.g., Boesch & Boesch 1984; Milton 1988; Sigg & Stolba 1981). Arguments about domain-specific intelligence do not aim to oppose one comprehensive ecological argument against an equally comprehensive social one. The evolution of specialized skills to solve social problems by no means precludes the evolution of other, equally specialized skills to solve problems in other domains. Instead, the hypothesis posits that *specific* abilities – or adaptive specializations (Rozin & Schull 1988) – have evolved to cope with specific social or ecological demands. As a result, skills exhibited in one context may not always be generalized to another. So, for example, although a male baboon might have no difficulty in assessing the relative ranks of other males, he might be unable to rank the relative amounts of water in a series of containers. Similarly, if the need to exploit widely dispersed and ephemeral waterholes has favored the evolution of complex spatial memory in hamadryas baboons, we should not necessarily expect hamadryas baboons to be better than other monkeys at remembering the genealogies of band members. Instead, we suggest that the monkeys' intelligence is largely inaccessible to them: Knowledge gained in one domain is not necessarily extended to another.

There is no need to limit these arguments to nonhuman primates. To cite just one example, white-fronted bee-eaters (*Merops bullockoides*) are colonial, communally breeding birds that display both highly developed spatial memory in their foraging behavior and primate-like social skills. Not only do they remember the location and boundaries of distant feeding territories, but they also seem to distinguish kin from nonkin, close relatives from nonrelatives, and unrelated neighbors from non-neighbors (e.g., Emlen & Wrege 1988; Hegner & Emlen 1987). It is not yet possible to say whether bee-eaters approach primates in recognizing both their own social relationships and the social relationships of others, because the relevant observations and experiments have not been conducted. We also do not know whether any of the specific skills that bee-eaters use in foraging are also manifested in social interactions. Do bee-eaters, for example, distinguish the mates of other, unrelated birds with the same acuity that they distinguish these individuals' territorial boundaries? If bee-eaters (or any other species) failed to extend an ability used in a foraging context to a comparable social context, this would provide further support for the domain specific view of intelligence.

Observations suggest that there is, in nonhuman and perhaps also human primates, an evolutionary predisposition that makes it easier for individuals to understand relationships among conspecifics than to understand similar relations among things. Among humans, the predisposition is more subtle but nevertheless apparent in the earliest years of childhood when infants seem remarkably sensitive to the emotions, behavior, and social relations of other people while at the same time remaining ignorant of much of the world around them.

Monkeys have a kind of laser beam intelligence. Although they solve social problems with little difficulty or training, they often flounder when confronted with the same problems outside the social domain. They do not always generalize their social abilities to other species or to inanimate objects, and in this sense their skills seem relatively restricted. Apparently, the animals do not know what they know and cannot apply their knowledge to problems in other domains.

Chapter 10: How monkeys see the world

When we study the social behavior of monkeys we are tempted to anthropomorphize and treat them as if they were human. This is not entirely inappropriate. Like the primatologists who study them, vervet monkeys observe social interactions and draw generalizations about the types of relationships that exist among individuals. The monkeys also use sounds to represent things and compare different vocalizations according to their meaning.

There are many ways in which vervets' view of their world is very different from our own, however. Though monkeys may make use of abstract concepts and have motives, beliefs, and desires, their mental states are not accessible to them: They do not know what they know. Furthermore, monkeys seem unable to attribute mental states to others or to recognize that others' behavior is also caused by motives, beliefs, and desires.

The inability to examine their own mental states or to attribute mentality to others severely constrains the ability of monkeys to transmit information, deceive, or feel empathy with one another. It also limits the extent to which monkey vocalizations can be called semantic. True, calls function to denote objects and events in the environment and, like words, are caused by the mental states of those who use them. Unlike our language, however, the vocalizations of monkeys are not produced with an intent to modify the mental states of others. Though monkeys are skilled observers of each other's *behavior*, they seem to be far less astute observers of each other's *minds*, and they seldom seem to proceed beyond other animals' actions to analyze the motives underlying their behavior. *We* attribute motives, plans, and strategies to the animals, but they, for the most part, do not.

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Commentary submitted by the qualified professional readership of this journal will be considered for publication in a later issue as Continuing Commentary on this article. Integrative overviews and syntheses are especially encouraged. All page references are to Cheney & Seyfarth's "How Monkeys See the World," unless otherwise indicated.

Monkeys mind

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In the last chapter of *How Monkeys See the World*, Cheney & Seyfarth (C & S) ask, "Do . . . mental states really exist in the mind of any animal? Or are they artifacts, invented by ethologists as the best means of describing what they have seen?" (p. 303) These questions are about ontological realism with respect to mental states in animals. At least one philosopher (Daniel Dennett) who has influenced C & S thinks that the answer to these questions should be "no" and "yes" respectively (see, e.g., Dennett 1987 and multiple book review, *BBS* 11(3) 1988).

Disappointingly (or, some might say, shrewdly), C & S do not answer these questions directly. They indicate that the use of anthropomorphic terms "is not entirely inappropriate" (p. 312). But even an eliminativist might accept this claim if all it means is that there are pragmatic or instrumentalist reasons for anthropomorphizing. They also consider the question of whether monkeys attribute mental states to each other (Chapter 8), concluding that, in general, they do not.

Neither a positive nor a negative answer to this question helps settle the issue of realism. Although Davidson (1975) holds the view that to have beliefs one must be capable of attributing beliefs to others, this is not a typical view, and it is reasonable to think that there is no logical incompatibility between having beliefs and not being able to attribute beliefs to others. So the negative conclusion produced by C & S does not rule out realism. Neither, however, would a positive conclusion support realism, because monkeys could be just as mistaken about a correct theory of mind as, some would maintain, humans are.

In other places, commitment to realism about monkey minds is clearly suggested by C & S. For example, they assert that "monkeys may . . . create in their minds a number of representations that describe different sorts of social relationship" (p. 304), and they describe evidence that makes them "suspect that such representations exist" (p. 305). In places where C & S produce this kind of claim, they usually justify it as a form of inference to the best explanation. I am inclined to agree with this strategy, but it will not satisfy those, such as instrumentalists in the philosophy of science, who are convinced that the explanatory power is no evidence for truth.

Thus, the explicit statements about whether animals really have minds are unlikely to be convincing on either side. But that is to miss the real strength of this book, which is the extraordinary detail it gives about the lives and abilities of vervet monkeys, both in their natural state and in comparison to other apes. The details described in this book show why a number of philosophers (myself included) are excited about developments in ethology and its potential as a testing ground for philosophical theories of mind. C & S present the material with a great deal of philosophical sophistication, which greatly enhances the value of this book for the purpose just mentioned.

So, when the last chapter fails to answer the question posed at the beginning, the disappointment is only relative. Indeed, it would be unreasonable to expect C & S to declare explicitly in favor of realism when philosophy of mind often appears to be dominated by those who disparage "folk" psychology. But here, work by ethologists can have a very large impact on philosophical theorizing. First, philosophy of mind has been dominated by considerations arising from the philosophy of psychology, particularly the explanation of individual behavior. Ethology, is concerned with the behavior in an evolutionary context, however. If brain mechanisms are to be given an adaptive explanation, it is worth considering the possibility that they are adaptive because of the connections they provide from the organism to its environment. Such connections may best be captured in terms of mental content. So, whether or not mental content is dispensable for the purposes of explaining individual behavior, it may be indispensable for ethology.

Second, philosophers who have argued against animal minds have emphasized the respects in which nonhuman animals differ from humans. Those who have argued in favor have emphasized similarities. But the proper attitude, admirably demonstrated by C & S, is to describe similarities and differences without having a particular axe to grind. We should not expect to be able to answer the question of whether animals have minds by looking at a single feature in isolation. As C & S show, in many respects the vervets are quite like us, and in many respects they are quite different. Telling such a complicated story requires an enormous amount of work, but without it philosophical attempts to understand animal minds are so much whistling in the wind.

So, what is it like to be a monkey? Nagel (1974) is right in thinking that no amount of neurophysiological information can answer this question and its cohorts. But, as C & S show us, there are other kinds of information pertinent to answering the question. With other humans, the more I know about you the better idea I have of what it is like to be you, even if your way of life is quite different from my own. The same should be said about other animals. After reading Cheney & Seyfarth's book one realizes that one is a lot closer to knowing what it is like to be a vervet than one might originally have thought possible.

Finally, the question of instrumentalism versus realism about animal minds cannot be as easily dismissed as might its counterparts concerning quarks or electrons. The ethics of our treatment of other animals is intimately tied up with our assessments of their minds, so whether or not we decide that monkeys really have minds might make all the difference in the world for the monkeys themselves.

Monkeys and consciousness

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A lesson of science is that if one is to understand and explain the workings of a complex system one should seek to analyze it in terms of cooperating subsystems. One should also consider and seek to explain the workings of any systems that approximate, but are simpler than, the complex system to be explained. These maxims are more than ever to be borne in mind when considering the most complex and most complexly operating system that we know of: the human mind. What the maxims dictate about the mind is, among other things, the closest attention to the mental processes of small children, nonhuman primates, and monkeys.

Through a study of vervet monkeys we may come to understand ourselves somewhat better. This, for me, is the major interest of Cheney & Seyfarth's (C & S's) fine book, although it has many other claims to attention.

One fundamental question about the mind that C & S's work helps to illuminate is that of *consciousness*. It cannot be doubted that these monkeys have minds. They mentally represent the world. They mentally represent themselves and the complex social relations in which they stand to the other members of their groups. In one sense, then, they are conscious, at least during their waking hours. They are functioning mentally, and at quite a high level. But they do not appear to have consciousness in the sense of "meta-self-awareness," defined by C & S as implying "that the individual is aware of his own state of mind and can use this awareness to predict and explain the behavior of both himself and others" (p. 240).

I suggest that this definition of consciousness should be restricted to its first half: "that the individual is aware of his own state of mind." The second half describes a function of consciousness that, C & S suggest, is the evolutionary explanation, or part of the evolutionary explanation, of why such awareness emerged in the first place. It is certainly a very interesting explanation. Consider the vexing question of whether vervets ever try to deceive each other. C & S give us the wonderful anecdote of the vervet Leslie who has a grudge against a lower-ranking female, offers to groom her, but in the course of the grooming bites the tail of her unsuspecting victim. Was this really deception in the full sense of the word? That depends on whether Leslie's grooming was intended to produce a *belief* in the mind of the other that Leslie had good intentions toward her. If that were the real content of Leslie's intention, then she would have to have had the notion of the other monkey having mental states. But could she ever have developed this sophisticated notion unless she had some awareness, however rudimentary, that she herself had mental states? This may seem implausible. That being so, we have to consider very seriously the hypothesis that all Leslie intended to do was to affect the other female's *actions*. An offer of grooming would produce relaxed quiesence, which would enable Leslie to attack. C & S know of no evidence at present available to rule out this second hypothesis; indeed they seem inclined to think that that is all that vervets are capable of. Apes are a different matter. It seems likely that they understand, and so are capable of, full-blooded deception. The evolutionary gain in this intellectual advance is the possibility of concealing intentions and manipulating others in a much more thoroughgoing way.

All this is exceedingly penetrating and suggestive. But it remains true that any hypothesis about the evolutionary value of consciousness is speculative at the present time. For myself, I should like to emphasize another idea. I do not think it is absent from C & S's thought, but I do think it deserves more prominence.

One of the most interesting things in C & S's book is the demonstration that the discriminations that vervets make in social matters are subtle and penetrating, yet that transfer of this capacity to some other matters that ought to be of extreme concern to vervets is quite lacking. This failure of transference is also to be found in the apes, though to a lesser degree, and even in ourselves. This is something that might be very obvious to a higher species that observed us as we observe apes and monkeys. Intelligence seems to start as a social affair and does not transfer easily. But when transference actually is effected, what is the mental mechanism for transfer? I think that consciousness, taken as awareness of our own mental states, plays a central role.

If the discriminations and insights achieved in one field are to be transferred to another, or a common feature is to be abstracted from both, ideas or images from both fields will have to be brought together in a common arena. But this, though necessary, is hardly sufficient. Some sort of mental work will have to be brought to the arena, if only a recognition that a certain association of images is a fruitful one. One way or another there must be *purposive* manipulation within the arena. It is well recognized that a necessary condition for the carrying out of purposes is that there be cognitive access to the situation as it develops. Only then can there be the cognitive feedback required for the successful prosecution of an enterprise. In the field of physical action, feedback is supplied by perception. In the field of the mental, feedback is supplied by awareness on the part of the agent of (some of) the agent's mental states. (Such awareness seems to deserve the term "inner sense," a phrase found in both Locke and Kant. This brings out the parallel with perception.) That, perhaps, is why awareness of our own mental states permits intellectual advance of a transferring or connecting sort.

How monkeys do things with "words"

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Cheney & Seyfarth's (C & S's) book is a fascinating excursion into both the mind of the monkey and the debates surrounding animal intelligence. Their conclusion on the existence of a theory of mind in nonhuman primates is appropriately cautious: There is no "tight" evidence that monkeys or apes recognize the mental states of knowledge or belief (although apes may recognize the mental states of desire and goal). Monkeys and apes clearly distinguish complex social relationships and behaviour, but they do not necessarily distinguish minds. This conclusion is altogether less controversial than Premack & Woodruff's (1978) position in *BBS* and is in line with Premack's (1988a) later work. This conclusion has implications for what might be going on in monkey communication.

C & S's work on monkey communication has been of major importance, illustrating how the experimental method can be used even in their subject's natural habitat of rural southern Kenya. Suppose we accept C & S's claim that monkeys can produce "words," because their vocalizations appear to *refer* to events in the outside world. This still leaves open the question of what monkeys are trying to *do* with their words. As Austin (1962) discussed, words can be used for a variety of different functions. C & S approach this problem at one point by means of a comparison between the way human children and monkeys communicate: "Although children quickly go on to much greater things, for a few brief months communication by the child and monkey is similar in the following respects. Both use sounds in a limited set of circumstances, *as if* they are denoting particular features of their environment or "commenting" on the situation in which they find themselves" (p. 173). I would like to unpack this comparison a little more, to see whether it really holds. Are words really used for "commenting" by both populations?

C & S illustrate this comparison with an example from a human infant who said "Zert!", whilst gesturing toward the refrigerator, apparently with the meaning, "Get me the dessert!" The equivalent communication by the monkey is a vervet vocalization with the apparent meaning, "Leopard!", and so on. This comparison seems reasonable when considered in this form: Both the human and the monkey utterances appear to be intended to get another individual to *do* something (produce the dessert, flee from the leopard, etc.). In this sense, they are both "protoimperatives" (Bates et al. 1975). The utterance, or speech act, is used to produce a particular outcome in the *physical* world – obtain an object, cause someone to move, and so on. (Human infants also point or gesture to achieve the same effect.)

But the comparison between human and monkey communication breaks down, I suggest, when the wider repertoire of human infant communication is considered. Whilst monkeys appear to be restricted to using words *only* as imperatives, human infants – right from the beginning of their communication – use words not only as imperatives but also as *declaratives* (Bates et al. 1975). Protodeclaratives (which may also be a single "word," or even a single, silent gesture) function not primarily to obtain a result in the physical world, but to direct another individual's *attention* (their mental state) to an object or event, *as an end in itself*. Thus, a human toddler might say "Plane!" apparently to mean, "It's a plane!" or, "Look! A plane," and so on. Here, the child communicates simply to share interest in something. This is "commenting" on a situation for its own sake. In this sense, the use of communication strongly suggests that the speaker is trying to affect the listener's *mind* (Baron-Cohen 1989a; 1991b). There is no overtly physical goal: The child is not trying to obtain the object, or get the listener to act, and so on.

C & S's claim that monkeys are also "commenting" (p. 173) may therefore need some qualification: Insofar as monkeys comment, they appear to do so to produce physical, not mental effects. Monkeys' communication may thus resemble human children's communication in its imperative but not its declarative use. As with the question of whether monkeys have a theory of mind, the evidence appears to still be quite fragile as to whether monkeys can use "words" purely as declaratives. We need further research of C & S's excellent quality to answer the question of whether monkeys or any other nonhuman primate can produce genuine declaratives. This is not just a question about communication: It may also be critical for understanding the evolution of a theory of mind, in that proto-declaratives and other forms of "joint-attention" behaviours have been proposed as a precursor in the development of a theory of mind (Baron-Cohen 1991b).

New elements of a theory of mind in wild chimpanzees

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Cheney & Seyfarth's (C & S's) book makes it clear that rather little is known about the social cognition of primates. They stress this point repeatedly and correctly remind us to be careful whenever we want to generalize. The role of scientists, however, is to draw conclusions, and given the present state of knowledge, they are bound to be proven wrong at one time or another when new observations are reported. Let me address some of the points related to the theory of mind that would benefit from new observations made with the Taï chimpanzees.

Imitation as a measure of attribution. Imitation has recently become the centre of a debate concerning the transmission process in primates. The conclusion, supported by C & S, is that primates are not copying each other; only humans do so. The new observations that cast doubt on such imitative abilities were mostly carried out in restricted, captive settings within artificial groups, however. Imitation presupposes a natural and trustful relationship between two individuals that should motivate one of them to copy a specific behaviour of the other. In comparison, captive studies provide a rather inappropriate social environment. In an attempt to investigate the imitative abilities of the chimpanzee, Tomasello et al. (1987) observed that captive young chimpanzees fail to copy the behaviour of others who were throwing sand or using reaching sticks. Well-fed zoo chimpanzees may not be very motivated to use a stick and even less to throw sand (interestingly enough, only four out of seven young chimpanzees and none of the older ones tested showed any interest in the raking tool during the test trials). In addition, the models (an adult female) may not have had the prestige of a mother or a familiar adult male to inspire imitation of their behaviour. Finally, the fact that rake use in an American zoo is not imitated by the chimpanzees tells us nothing about the learning processes involved in termite fishing or nut cracking by Gombe or Taï chimpanzees, two of the behaviours considered by some as possible candidates for cultural transmission through imitation. Evidence of imitation in chimpanzees must hence be sought in animals possessing behaviours that are candidates for cultural transmission, for example, the imitation of termite fishing should be studied with Gombe chimpanzees and nut cracking with Taï chimpanzees (Boesch, in press a).

Empathy as measures of attribution. As C & S mentioned, Gombe chimpanzees may show compassion for wounded individuals, but only between close kin. Whenever a nonkin presents a wound to a chimpanzee, this induces a reaction of fear or disgust (Goodall 1986), a reaction similar to that described in monkeys. Taï chimpanzees, however, totally independent of kin relationship, were regularly seen to tend wounded animals for extended periods of time. Once this care was observed for more than 2 months (Boesch in press b). Individual reactions tend to indicate that they are aware of the needs of the wounded, e.g., they lick the blood away and remove all dirt particles with fingers and lips, as well as preventing flies from coming near the wounds. In addition, empathy for the pain resulting from such wounds was clearly demonstrated by the reaction of other group members: After having received fresh wounds from an attack of a leopard, the injured individual is constantly looked after by group members, all trying to help by grooming and tending the wounds. Dominant adult males prevented other group members from disturbing the wounded chimp by chasing playing infants or noisy group members away from his vicinity. In addition, as wounds handicapped the movements of the injured animal, group members remained with him as long as he needed before he was able to begin to walk again; some just waited, whereas others would return to him until he started to move (three times the group waited for four hours at the same spot). Whenever he stopped, they waited for him. Such a difference with the Gombe chimpanzees may be explained by the high predation pressure Taï chimpanzees suffer from leopards (Boesch, in press b).

Teaching as a measure of attribution. C & S agree with most authors that teaching is unique to humans. Contrary to C & S's proposal that teaching only requires attributing ignorance to others, I suggest that teaching requires much more: In most situations, the naive individual has some knowledge of the task already, and in teaching one must not only attribute knowledge to the other individual different from that of the teacher, but this knowledge must be compared with the teacher's own in order to determine what aspect is incomplete or inappropriate and needs to be changed. This comparison between two mental knowledges has to be done accurately enough for the teaching to be understood by the naïve one and to improve his performance. Apart from the cognitive capacities needed to make judgements about another individual's errors and needs, biologists would say that teaching should appear only when it is necessary for improving the survival and reproductive abilities of an individual.

The lack of observations on teaching in apes might only reflect the fact that we have looked for tasks that didn't require teaching in order to be normally acquired. The nut-cracking behaviours in Taï chimpanzees require many years to be fully acquired and the mother who normally shares the nuts with her infant must at a certain moment interrupt her investment to be able to invest in a second infant. This period may be costly to the first infant, as it does not fully possess the nut-cracking behaviour. Nut cracking may therefore be one of the rare behaviours that could force the mother to accelerate the learning process of her infant if she does not want to harm her own fitness. In fact, Taï chimpanzee mothers were observed to *teach* some aspects of nut-cracking technique to their infants who already had some of these skills, either by demonstrating the right movement or by correcting an error in the infant's technique (Boesch 1991).

Cheney & Seyfarth's synthesis of their observations on rhesus monkeys is fascinating and I can only hope that their example will be followed with some of the monkeys and apes species about which we know so much less.

Looking inside monkey minds: Milestone or millstone

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Cheney & Seyfarth's (C & S's) is an important book, a seminal contribution to ethology. The authors are well-known for the elegant field observations and experiments on the function, ontogeny, and proximate mechanisms of vervet vocalizations and social interactions that are integrated here. In their evaluation of alarm behavior from three of the four classical ethological queries (Tinbergen 1963), C & S have shown the power of a multifaceted approach. They have also solved some important methodological problems, although it is ironic that to show complex cognitive recognition they rely on that most "primitive" of learning processes, habituation.

But what about the mentalistic and subjective language in which these studies are embedded and interpreted? This is a programmatic example of "closing the circle," acknowledging both instinct and mind in both human and nonhuman (Burghardt 1978). My comments here explore the "inside the mind" subtitle, and I would like to know from C & S whether I was at all able to get inside *their* minds.

Are mentalistic constructs necessary for conducting and presenting studies such as these? I have argued previously concerning the vervet work that the answer is no (Burghardt 1985), but not being necessary in principle does not mean that it was not an important factor for C & S. Did the mentalistic aspects develop gradually or were they explicit from the beginning?

It does seem true that much creative research in ethology and psychology relies on using an unacknowledged *critical anthropomorphism* (CA) (Burghardt 1985; Donnelley & Nolan 1990). This is the use of one's experiences, intuitions, and empathy, tempered by prior studies and knowledge of the biology and natural history of both people and the species in question, to design experiments and generate predictions about what other species (or other members of our own species) will do. [See Dennett: "Intentional Systems in Cognitive Ethology" *BBS* 6(3) 1983 and Précis of "*The Intentional Stance*" *BBS* 11(3) 1988.] C & S do an admirable job of using subjective analogical inference, applying the constraints that make the anthropomorphic stance critical. The last chapter (10) represents this point clearly. But do C & S feel that they have really gotten inside the mind of another species? Alternatively, is speculating on what monkeys may need to think about or entering their *Umwelt* (von Uexküll 1934/1957) heuristically useful, even necessary, in order to understand, model, or predict behavior? Chapter 10 suggests the latter, but the title and often the language promise the former. Perhaps both are true. This is not a trivial epistemological problem (Burghardt 1991).

Such confusion, so easily removed, yet so persistent in recent scholarly writings (Skinner 1990) suggests that something else is operative. An eminent scientific associate of C & S's told me years ago that a major benefit of Don Griffin's writings (e.g., 1976) was that they were liberating, opening up new kinds of questions that could be asked. For years I was perplexed because critical anthropomorphism, although receiving little systematic attention, has been around a long time. But now I think I understand. The liberation and freedom is in the Zeitgeist, the receptivity of granting agencies, the respect by the scientific and publishing elite.

Claiming to have as a goal "knowing" the mind, feelings, subjective world, or consciousness of a monkey (or other "high" mammal) is a decided help in getting people to pay attention to your results. Regardless of what behaviorist purists would say, it is clear that many of us, scientists and nonscientists alike, really *want* to have an understanding of other worlds, *long* for an entry to their private experience. Do not most scientists, our most "rational" subpopulation, have an intense devotion (if not time to devote) to theater, music, art, history, or literature? The popular press, films, and television respond, even pander, to a perennial affinity for "human interest" stories, a preoccupation with the bizarre and macabre in human psychology, and a fascination with serial killers, celebrities, royalty, the wealthy, the powerful, despots, and the long dead. Doesn't this reflect a universal yearning to know, "What would it be like to experience . . . ?"

Animals fascinate us the same way. Yet with animals we recognize difficulties in empathy and understanding, limits to our ability to read minds, that we are often reluctant to recognize when we evaluate the inner world and future behavior of friends, enemies, colleagues, lovers, children, and politicians. We so much need to rely on mentalistic inferences about other people in our everyday life that applying critical anthropomorphism in these contexts seems either demeaning or excessive.

How monkeys see the world is a milestone in cognitive ethology and may well be another harbinger of a substantial change in received scientific opinion about animal behavior and what can be known about comparative cognition (see also de Waal 1982). It delivers on Griffin's call to open up the question of animal mentality in a species where there is less resistance at least to discussing the issue than if bees or parrots were the focus. It does so using biologically meaningful behavior, ecologically realistic settings, sound experimental techniques, and data analysis. C & S acknowledge that their conclusions will stand or fall on the basis of empirical data rather than compounded anecdotes or profound sounding, but sometimes ethologically naïve, philosophical exegesis. It is less antagonistic in its discussion of behaviorism than some other writings have been, although the pioneering cognitive ethology of von Uexküll (1934/1957) should merit more than an aside provided by a reviewer!

But the book could also be a millstone. The new comparative mentalistic ethology and psychology may, if its practitioners are not careful, repeat the errors that caused the comparable late nineteenth century enthusiasm to be slowly, but firmly, discredited (Burghardt 1985). Workers in cognitive ethology must emphasize repeatedly the limitations of their conclusions and the need for careful empirical work grounded in accurate natural history and sensitive observation. It would be unfortunate if C & S's attempt to follow in von Uexküll's path is soon overgrown with vines and brambles as others try to skip the hard data gathering and "see the new freedom as an opportunity for free-floating uncritical fantasies about mental life" (Hilgard 1980, p. 15). Ultimately, effective attempts to enter the world of another organism, be it human or animal, are based on convincing illusion, the telling of a good story. As a species we seem better at telling stories than evaluating their truth value, one clear example being the continuing debates over creationism and other myths. Clearly the sophistication and presuppositions of the audience will influence how convincing a story seems. Although I suspect that C & S may really want to experience or appreciate the world as a vervet monkey does, they know that it is really their vision, their story, rich and insightful though it be, that is at stake.

Social versus ecological intelligence

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How monkeys see the world is an interesting, clear, and carefully written book. Drawing largely but not exclusively on their own research on vervet monkeys, Cheney & Seyfarth (C & S) attempt to set both upper and lower bounds to the cognitive abilities of monkeys as they apply to behavior that occurs in nature. Defining upper bounds seems to be the more difficult task, and here the discussion was most speculative. For example, do monkeys have a theory of mind? C & S conclude that although some anecdotal and experimental results are suggestive, "we lack examples that cannot be explained *except* in terms of a theory of mind." They judge the weight of the combined evidence to favor slightly the view that monkeys lack higher-order intentionality. And yet, at least partly because of methodological limitations, lack of evidence for a theory of mind is not necessarily evidence against it. A critical test is needed in which negative results have unambiguous meaning.

I am focusing on a second speculative aspect of limitations on monkey cognition, namely, whether specific cognitive abilities evolved largely as a result of selective pressures acting on individuals in a social or a nonsocial domain, and whether certain abilities are therefore limited to one domain or the other (Chapter 9). The social intelligence hypothesis is currently fashionable, despite the fact that there have been few attempts to operationalize it, or its leading competitor (namely, an ecological intelligence hypothesis), or to test them "against" one another. For these reasons, the case for social intelligence has not been entirely convincing.

C & S first point out that monkeys make spontaneous classifications of social entities (e.g., individuals, relationships) on various bases (e.g., kinship, similarity), but that they do not make similar spontaneous classifications of nonsocial entities. Is the latter assertion correct? We know very little about how primates organize knowledge of their nonsocial environment: Ecologists are usually not particularly interested in cognition. It seems quite plausible, however, that when primates choose a foraging route, they classify and compare various food sources on the basis of size, caloric or nutritional yield and even distance. If routes are planned more than one step at a time, relations between food sources might be classified and compared as well. It seems premature to rule out spontaneous classification in the nonsocial domain: Our understanding of the mechanisms underlying ecological tasks like food finding and orientation is simply too incomplete. [See also Fantino & Abarca: "Choice, Optimal Foraging, and the Delay-Reduction Hypothesis" *BBS* 8(2) 1985; Houston & McNamara: "A Framework for the Functional Analysis of Behavior" *BBS* 11(1) 1988.]

A second argument concerns the predisposition of primates to attend to social rather than nonsocial stimuli. Much of the evidence comes from studies of ontogeny, in which social stimuli are undoubtedly critical. But as C & S themselves mention, it is difficult to interpret such studies unambiguously as indicating preeminence of social stimuli in other than a chronological sense, because different degrees of experience with social and nonsocial stimuli cannot be controlled. It is a consequence of being an infant mammal that social stimuli are most important at an early age, and infant primates in particular depend on their mothers for almost everything.

A third argument is that primates solve problems involving social stimuli more easily than logically similar problems involving nonsocial stimuli. Experiments with adult humans have apparently led to exactly this conclusion, but data from nonhuman primates are sparse. C & S have themselves conducted playback experiments involving the calls of nonvervet species (i.e., nonsocial stimuli), comparing their results to those from similar experiments involving vervets. As long as the nonvervet species are relevant to vervets (i.e., they are predators, or they have common predators), vervets respond to playbacks much as they respond to playbacks of vervet calls. Thus, based on acoustic cues, vervets seem to recognize associations of other species, as they recognize associations of particular conspecific individuals. If the nonvervet species are not relevant to vervets, however, the vervets act as if they know nothing about them: C & S conclude that vervets are not good naturalists and speculate that humans would behave differently. I do not think that most humans are good naturalists either, however: The people living near the forest where I study monkeys know almost nothing about these animals, for even though they spend considerable time in the forest passing through and gathering wood, the monkeys are simply not relevant features of their world.

One puzzling finding is that vervets do not attend to visual cues indicating the proximity of predators (e.g., python tracks), even though predation is a major source of mortality, and humans find such cues informative. Several hypotheses may explain this apparent lack of ability, but the point to make here is that using inanimate visual cues in a referential way does not occur in the social domain either (e.g., vervets do not seem to monitor each other's tracks). The only secondary visual cues that vervets do use in a referential way are other vervets. For example, the approach of a particular individual may signal the subsequent approach of its relative. In a similar way, however, a monkey going through the motions of feeding or intensive food search signals the presence of food that may not be visible to an observer monkey. Perhaps vervets make more effective visual cues than inanimate objects simply because they are more familiar.

The idea that specific cognitive abilities evolved in specific domains is interesting, but at present its application to primate cognition is difficult, for at least two reasons. First, despite all we know about the social and ecological lives of primates, we lack a natural history of cognition that would allow us to assign particular abilities to particular domains; we lack especially analyses of the cognitive bases of common ecological tasks (e.g., orientation and food finding). Second, as others have pointed out, there is the very basic problem that social and nonsocial tasks are largely coincidental in primates: for many species, and especially those with the largest brains, exploiting the environment is done together with group-mates, and even relatively solitary tasks are often socially learned. Should this book stimulate further research into the nature and development of primate ecological cognition, it will have made an important contribution beyond the sometimes speculative conclusions it contains.

Surplusage, audience effects and George John Romanes

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Let me note first that I think Cheney & Seyfarth's (C & S's) a splendid book. The "cognitive revolution" has been penetrating animal behavior research in recent years and is transforming it as surely as did the earlier influences from ethology and sociobiology. Questions of intention, mind, and consciousness were banned from animal psychology around the turn of the century because scientists could not deal with the many problems they pose. We still have no definitive manner in which to deal with the animal mind other than by analogy with our own psychological experience. By adopting a "stance" reflective of intention, mind, or the like, however, we are led to ask interesting questions and discover fascinating features of animal behavior and, perhaps, the animal mind. C & S show that this can be done with an anthropomorphism that is both critical and insightful.

If behaviorism provided a thesis, it was Donald Griffin, more than any other scientist, who saw that it was time to provide an antithesis. [See Griffin: "Prospects For a Cognitive Ethology" *BBS* 1(4) 1978.] Griffin's approach was too extreme for many animal behaviorists. It sensitized them however, to questions long forgotten and helped to stimulate and pave the way for acceptance of the syntheses of such recent balanced work as that of Boysen, Gallup, Herman, Mason, Menzel, Pepperberg, Premack, Rumbaugh, and others. I believe that the C & S book will become a much-cited classic in the emergence of this synthesis.

Nevertheless, any good academic can find a few things with which to quibble. First, I am inclined, with C & S, to believe the "Jolly-Humphrey hypothesis," that social relationships provide the most likely selective pressures leading to the evolution of abilities tapped in many laboratory tests of primate cognition. Basically, this is a solution to the problem of "surplusage,"the problem of why animals should display potentials of learning ability in apparent excess of those required in evolution, which can be traced to Alfred Russel Wallace (see Boice 1977). My concern is with the falsifiability of the Jolly-Humphrey hypothesis. C & S suggest that primates may do well in laboratory tests based on the transitivity of relationships because they learn the transitive nature of dominance relationships in nature and thus can generalize to problems that are formally similar. They also rely heavily on the notion of "domain specificity," however, according to which abilities evolved in one context do not generalize to other contexts (e.g., p. 295). With these two principles of transfer and domain specificity in place, virtually any result can be explained. This is reminiscent of the situation with some aspects of psychoanalytic theory. [See *BBS* multiple book review of Grünbaum's *"Foundations of Psychoanalysis," BBS* 9(2) 1986.] It may be possible, with careful, empirical studies, to delineate conditions under which transfer or domain specificity prevail. We might then be able to make testable predictions. For now, however, there is a real problem of falsifiability with regard to the explanation of "surplusage" in terms of social relationships.

Another area of concern relates to the "audience effect." My first problem here is with the appropriation and use of a term already in the literature with a different meaning. For Zajonc (1965) "audience effects" are changes in levels of performance in various tasks consequent on the presence of an audience. Thus, improvements in such tasks as the pursuit-rotor performance, vigilance, multiplication, and word association occur as the result of an audience (see Zajonc 1965). For such authors as C & S and Marler (e.g., Marler et al. 1990), however, "audience effects" are adjustments in the emission of communicative signals contingent on the presence of conspecifics and suggestive of intentionality. The use of the same term in these two different contexts is unfortunate.

This leads to a somewhat deeper point. Zajonc (1965) uses some principles from learning theory to suggest that social influences can produce social facilitation through a nonspecific, arousal-related mechanism. He argues that high-probability behavioral patterns will be facilitated and low-probability patterns inhibited in the presence of an audience. Both C & S and Marler et al. consider and reject arousal as a possible explanation of their "audience effects." The changes in response probabilities need not be reflected in overt, physiological indices, however, they should be apparent in changed behavioral profiles. I think we need some old-fashioned ethological analyses with complete behavioral inventories to determine what behavioral patterns are and especially what patterns are *not* changed in the presence of an audience. Marler et al. (1990, pp. 200–01) appear to have made a start in this direction. The case made by C & S for referential communication is, in my judgment, a very strong one. I think that if it can be shown definitively that there is a clear selectivity with respect to which patterns are enhanced and inhibited in the presence of audiences, their case can be even stronger.

Finally, I must comment on C & S's remark about the "uncritical mentalism of Romanes" (p. 9). From my 1990s vantage point, I can be as critical of some aspects of Romanes's work as can C & S. Romanes was well aware of the dangers of anecdotes, however, as can be seen in the Preface to his *Animal intelligence* (1882). However much we may regret his interpretations, he was not uncritical, but concerned with important issues and trapped by the limited database available in his time. Romanes could be a critical thinker who was capable of very fine science (e.g., Romanes 1874; 1885a; 1885b).

It is an exciting time in the study of animal behavior as we return to the questions considered by Romanes with methods refined under the influence of behaviorism and the rich database now available.

Is the monkeys' world scientifically impenetrable?

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Cheney & Seyfarth (C & S) argue for a hybrid approach that "place[s] empiricist findings tentatively within the framework of a more mentalistic approach" (p. 9). The book is important in clarifying the value and limits of the intentional approach to interpreting monkey behaviour, particularly C & S's excellent field work with vervets. But, unintentionally, it also demonstrates that cognitive science is more a perspective than a scientific discipline. To illustrate this point, I consider the following topics: evolution of intelligence, concept formation, philosophy of science.

Evolution of intelligence. The starting point for most modern theories of intelligence in primates is the assumed discrepancy between their performance on learning tests in the laboratory and their "greater intelligence when dealing with each other" (p. 256; cf. social intelligence hypotheses). C & S emphasize this discrepancy while supporting a more general version such as Rozin's (1976) inaccessibility model of intelligence within different domains. They characterize their approach as a practical, functional perspective, but in the end it seems to be more a utilitarian than an evolutionary functional approach. I would argue that their approach to intelligence is in fact a version of the "sufficiency argument" first introduced by Simon (1981) in the context of AI. The essence of this approach is that intelligent creatures must be able to represent the important aspects of their environments in their knowledge of the world. Within a functioning monkey society the representational requirements of this knowledge are probably so severe that only a few modes of knowledge can meet them. Therefore, a demonstration that a particular mode of intelligence can support the wide range of representations monkeys obviously do use is powerful evidence for that mode as the appropriate model of monkeys' intelligence.

C & S are arguing that it is because of the dominance of social affairs that the social mode of intelligence has the required representational power. Nevertheless, it should be stated that the notion of intelligence is too vague to be the core of an evolutionary approach to cognitive behaviour or cognition. Even human intelligence is not a homogenous scientific concept (Howe 1988). Primate intelligence can be divided into several relatively autonomous abilities, for example, perceptual or motor skills, communicative or social competence, foraging knowledge, categorization or classification performance. Intelligence has evolved independently in different taxonomic groups and is neither a cognitively homogeneous nor a phylogenetically homologous behavioural function. Intelligent behaviour evolved because animals had to cope with the increasing environmental complexity resulting from evolutionary diversification. The increasing instability of animal/environment interactions could be handled only by reducing this complexity internally, that is, from a cognitive standpoint this can be called the duplicate complexity hypotheses).

On this basis, the critical evolutionary pressure could be social, but it could be any other relevant ecological factor. In the *Cercopithecidae*, as opposed to birds or other primate groups, it may have been mainly a social factor. But it seems to be of little value to stress one factor in intelligence (e.g., social knowledge) against others (e.g., nonsocial knowledge). Instead, we should adopt a functional approach (Dittrich 1988; 1990), taking into account all relevant factors, both proximate and ultimate in our attempt to understand behavioural processes. Such a view implies that intelligence did not evolve domain-specifically. On the contrary, domain-specificity seems to be the *result* of particular adaptation processes and not their starting point. In practice, the different selection pressures tending to produce intelligence are linked, so any claim that the evolution of intelligence can be explained on the basis of a single factor is highly implausible.

Concept formation. Surprisingly, C & S do not mention any of the important studies on the structural basis of the putative mentalistic processes called intelligence although it has (admittedly proved extremely difficult to isolate the comparatively simple but measurable mental processes presumed to form the building blocks of more general knowledge). Some causal investigations have been done (cf. Baddeley 1986; Griffin 1982; Mackintosh 1983; Weiskrantz 1985). C & S give no attention to such key areas as vision or memory and are highly critical of experimental laboratory tests (e.g., in the area of animal concepts). The results of experiments on concept formation are certainly controversial, but that is typical in areas under intensive research. There is a strong body of positive and reliable results highlighting different aspects of concept formation (e.g., Herrnstein 1985; Lea & Ryan 1990; Perrett et al., in press; Rosch & Lloyd 1978; Smith & Medin 1981). C & S correctly emphasize that the ecological context in these studies is sometimes missing. This is important not because ecology is *en vogue* but because it is a precondition for understanding the idea of concepts to grasp functional relations between external objects and internal cognitions.

To understand the mind of monkeys C & S look at the monkeys' immediate environment, but they ignore the organization of cognitive components as the basis for conceptual order. Laboratory experiments on concept formation perhaps do the reverse, but ecological factors are ultimately not enough to allow us to understand the diversity of monkeys' ability to categorize and classify (Dittrich 1988). Such cognitive factors as hypotheses (Gregory 1980) or schemata (Lorenz 1975) are operating in perception and memory and play an important role in goal-achieving behaviour. The reference points of mental representations, however, are not necessarily the goals to which behaviour is oriented or is intended to reach (McFarland 1989). According to the "connection principle" formulated by Searle (1990), intention and its communication is bound to consciousness, which monkeys probably lack. Thus, is the monkey's lack of what Searle calls aspectual shape the reason why mind-reading between monkeys will ultimately remain in the realms of science fiction?

Philosophy of science. Different approaches to primate ethology provide different perspectives and different perspectives lead to different observations. Different does not imply better, however. Does the utilitarian cognitive viewpoint proposed by C & S offer a broader or more realistic perspective for observation and data collection than the more cautious classical approach? The intentional perspective may be more convenient for describing primate behaviour, but it does not necessarily lead to a better understanding of what is happening in the monkey's mind. The reality of the monkey's mind or the "how to be like a monkey" is forever inaccessible but this is not, as C & S claim, an empirical matter, but a logical one. The achievable aim is to understand the effects and causes of monkeys' behaviour, including the roles played by their cognitions and the underlying physiological structures. The concept of intelligence is purely descriptive; it has not been established as a valid explanatory concept in scientific psychology (Howe 1988). The same holds for the concepts of mind, belief, and intention. The founders of ethology were acutely aware of the trap of believing that by describing animals' behaviour they were providing reasons for the behaviour; this is a necessary distinction, often lost in cognitive science when we account for behaviour in such terms as belief, desire, goal-orientation or intention. Can we transfer traditional ethologists' wisdom to modern primatology? Or will it turn out that human, scientific intelligence is domain-specific?

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Of monkeys, mechanisms and the modular mind

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Unquestionably, Cheney & Seyfarth (C & S) present us with a fascinating, insightful picture of vervet monkey life, a picture illuminated as much as is now possible from the vervet monkey perspective. More important, their work offers compelling reasons for exploring the domain-specific view of learning espoused by the new field of evolutionary psychology (Cosmides 1989; Cosmides & Tooby 1989). Many features of "domain theory" remain quite vague, however, especially when applied to animals. We explore this problem in the context of the vervets.

One major issue raised in C & S's book stems from the observation that monkeys do not behave as naturalists and in particular ignore cues based on other species' salient behaviors, cues that would presumably have enormous survival value. Vervets do not realize that a dead gazelle in a tree signifies a leopard in the vicinity or that a python's track indicates danger. C & S suggest that "that type of information" does not fall under the domain of social interaction, the most apparent and fine-tuned domain of vervets. But with 70% mortality from predation, as C & S note, "can it really be more important, in terms of survival, to develop a conceptual understanding of social relationships than to recognize the track of a python?" (p. 311). One immediately wonders what selection pressures are necessary to select for domain formation – when *do* we expect the evolution of a set of specialized cognitive abilities organized to solve a very specific kind of problem?

This is not a question about the *accessibility* of cognitive processes used in another, in this case social, domain. The issue is the absence of an autonomous "secondary predator-cues" domain for problems that do not seem intuitively to be any more challenging than those in the social sphere. If vervets have seen a leopard drag kills into a tree, the monkeys need only be capable of associating the objects dragged with the hanging body and thus a leopard's presence. Analogs to this type of reasoning certainly seem to exist in the vervets' domain for social interaction; yet we see no evidence of learning and reasoning directed at these situations. They are neither "accessing" the learning abilities used in the social domain nor have they developed a similarly specialized but no more complex "secondary predator-cues" domain.

Perhaps we have not accurately judged the cognitive mechanisms that (we imagine) animals must use to solve particular problems. Our assessment seems to follow a two-stage process: First, we apply our intuition based on what we believe about our own cognitive processes as they underlie human social interactions, and we infer how monkeys might carry out the similar actions we observe; then we try to understand how the underlying cognitive processes in the social realm would or would not be sufficient to solving antipredator problems. But not only is it possible that social cognition really is inadequate to the task of noticing and then linking inanimate cues of a predator with its possible presence, it is also possible that monkeys use cognitive mechanisms in their *social* interactions very different from those that humans use, mechanisms that may not be appropriate for learning to use secondary predator cues. Finally, we may not even have assessed correctly our own social cognitive abilities; monkeys may have similar abilities but we are not accurately describing theirs or ours. In essence, this calls for researchers to step back from the question of accessibility *across* domains to first examine the particular cognitive mechanism *within* particular domains.

These problems of inference from observed behavior to process, from human to animal, lead to one final comment. In a series of ingenious and arduous habituation experiments, C & S showed that vervets treat either a starling's alarm call or an impala's snort as if it provides information similar to their own alarm call. Vervets do not seem to learn that an impala's snort and a starling's alarm have the same referent, however. Their results were unlikely to be the result of the alarm calls of the starling and impala presenting information about different *types* of predators. C & S speculate that vervets cannot use information unless they themselves are one of the reference points. Alternatively, we suggest that from a vervet's point of view the starling's and impala's alarm calls provide different information about the *location* of the same predator. For instance, impalas sense by smell and are better at detecting predators hiding in bush, whereas starlings are better at sensing danger at a some distance. If this were the case, we might expect the wise vervet to habituate to either call, but not to transfer habituation across these calls.

Evolutionary psychology is a very young field and already owes a big debt to the work presented in this fine book. We agree with C & S who end with a discussion of domain-specificity and note: "We will make little progress in understanding the minds of nonhuman primates until we investigate these issues more thoroughly" (p. 312). We hope that investigation will not be limited to primates. Such studies as these promise to reintroduce some mechanisms and ideas about architectural constraints into the adaptationist realm of behavioral ecology. Obviously, scientists interested in animals and humans should keep very close watch on each other's progress and working assumptions.

Animal mentality: Canons to the right of them, canons to the left of them . . .

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Cheney & Seyfarth's (C & S's) book is a pleasure to read. The second chapter alone is a review of cercopithecine social structure so concise, current, and comprehensive that it could almost stand alone as required introductory reading for new students of primatology. It goes beyond a description of their basic natural history to convey a profound understanding of the social world in which these monkeys live. By doing so, it greatly facilitates the central mission of the rest of the monograph, which boldly attempts to probe what it is like subjectively to be a vervet monkey.

C & S have done an excellent job of marshalling all the available evidence both for and against the proposition that the observable behavior of vervet monkeys is indicative of certain levels of cognitive information processing of which many had previously thought nonhuman animals incapable. C & S have cautiously walked a tightrope between two very different approaches to the attribution of animal mental abilities. Although their degree of success in this precarious balancing act is noteworthy, I am going to try to make some trouble by sharpening this fine line. My intent is not to undermine their arguments, which I find generally well-supported and convincing, but to point out the broader implication of their work: We need to renew the dialogue in comparative psychology about the mental abilities of animals.

Two contrary positions have historically characterized the various approaches to this issue. One of these is the liberal interpretation, most often associated with Romanes (1882). This view emphasizes the continuity between human and nonhuman animals and does not hesitate to attribute human-like abilities to nonhuman animals wherever convergent behavioral phenomena are manifested. The other is the conservative interpretation, which emphasizes the principle of scientific parsimony in attributing mental abilities to animals. In comparative psychology, this principle is known as "Lloyd Morgan's Canon"; Lloyd Morgan (1894) proposed that we should attribute the behavior of an animals to no higher, or more complex, a level of mental ability than is strictly necessary to account for that particular behavior.

Methodologically, C & S appear to adhere to the latter, more conservative, point of view. They painstakingly show that it is indeed strictly necessary to attribute certain levels of cognitive processing to vervets to satisfactorily account for their observed behavior. Their field and laboratory experiments are routinely designed to address systematically any alternative hypothesis that the results might somehow be attributable to lower levels of cognitive processing. Although no single experiment is ever perfect, the cumulative corpus of C & S's work is compelling in this regard. Because I am certain that a disproportionate amount of the peer commentary will be on these methodological issues, I dispense with any more detailed critique and move on to my central point. It is when C & S are through laboriously demonstrating the need to attribute these mental abilities to vervets, and thus feel free to speculate on their adaptive functions, that a totally different point of view emerges. This is a more interesting perspective, differing from both the liberal and conservative positions, and thus merits more detailed consideration. In speculating on the evolution of these mental abilities, C & S do not typically ask what level is *minimal* to perform any given adaptive function, but what level is *optimal.*

For example, in considering the vervets' internal representation of social dominance, two different alternatives are presented. One of these is the "brute force" level of information processing, in which an individual is required to observe and remember the interactions between every troop member and every other to determine their social status relative to each other. The other is the ability to perform what is called "transitive inference" and thus to compute the relative status of every troop member with respect to every other with much more limited information. [See Bernstein: "Dominance" *BBS* 4(3) 1981.] Thus, if subjects observe that, in rank, A > B and B > C, they are not required either to observe directly or to remember explicitly that, therefore, A > C. In a large enough troop, this can be a tremendous savings in the costs of both initial learning and memory storage. C & S report that even trained human observers who have been there for some time have yet to observe interactions between every troop member and every other. If any individual monkey's internal representations of his natural social structure were either so chronically deficient or so late in full development, he would be at a grave social disadvantage compared to monkeys capable of transitive inference and thus the more rapid development of social competence. There is no question that, given the requisite genetic variability, rapid natural selection would rapidly competitively exclude the "brute force" cognitive strategy (even without taking into account the unknown neuronal "hardware" requirements of such extensive memory storage).

Thus, we go beyond the stark empirical question of what mental abilities nonhuman animals either possess or lack to the question of what mental abilities they *need.* What is implicit in this argument is a theory of optimal cognitive strategy (OCS) based on the adaptive cost-effectiveness of information processing algorithms rather than of overt behaviors. I wish I could go on to expound on the properties of such a theory, but that will have to wait until we have one. The only thing I can suggest is that C & S might apply this type of reasoning to their finding that many vervet mental abilities are so domain-specific. C & S seem to imply that it would much behoove the vervets to apply their intellectual talents to behavior in the antipredator domain, for instance, recognizing the characteristic tracks of pythons. This skill seems to be well within their mental abilities in the social domain, but is found curiously lacking. What I suspect is that if an OCS perspective can be invoked to explain the abilities that they manifestly *do* have, perhaps it can also explain the abilities that they manifestly *do not* have. Assuming that the basic design of the vervet mind permits adequate python-tracking abilities, is it just an historical accident that no python-tracking vervet mutations ever occurred? If not, why is python-tracking not naturally selected in vervets? Analogically, why don't monkeys have a "theory of mind?" How this type of question is answered may have substantial implications for how we think about the evolution of animal cognition.

Theory of society, yes, theory of mind, no

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Starting with the discovery of well-developed logical thinking in children who were profoundly deaf and consequently ignorant of a societal language (Furth 1966), I have been using Piaget's developmental theory as a fruitful framework for research for more than three decades and have expanded it into such new areas as Freudian dynamics (Furth 1987; 1992a) and social-societal construction (Furth 1992b; Furth & Kane 1992) in children. It has long been clear to me that a genuine grasp of what Piaget calls sensorimotor know-how is critical for an understanding of his entire theory. This is not surprising because in human development the sensorimotor is the transition from biological to object-symbolic knowledge; in contrast to the preceding and subsequent stages, it is itself aptly described as action knowledge. [See also Chevalier-Skolnikoff "Spontaneous Tool Use and Sensorimotor Intelligence in *Cebus* Compared with Other Monkeys and Apes" *BBS* 12(3) 1989.]

Even before I was asked to make some constructive comments on Cheney & Seyfarth (C & S), I had directed students to analyze the wealth of observations on the social life of vervet monkeys collected in this volume, to help us gain a better understanding of the scope, the richness, and the relative openness and flexibility characteristic of sensorimotor know-how. In this respect this book is a veritable treasure chest of carefully controlled and judiciously selected descriptions concerning the social action of vervet monkeys. It is sure to become an inexhaustible and lasting contribution to an evolutionarily valid science of psychology.

I now turn from the core to the theoretical framework in which C & S saw fit to present their research. I begin with the title, *How monkeys see the world: Inside the mind of another species*. There is no problem with the subtitle insofar as mind (or mental) is here used as equivalent to the word *psychology* (or *psychological*). But the main title is something else. It prejudges a case that should be scientifically explored rather than simply asserted. In the ordinary sentence, "I see the point," seeing does not refer to visual perception but to a theoretical positioning vis-à-vis a known object. To "see" the world therefore presupposes not merely that there is a world within which the species acts, but that the "world" is approached as an object, separate from the simple knowledge of how to act in the world. Perhaps these monkeys do not "see the world" at all, in this sense. This distinction – frequently dismissed as hairsplitting with no objective basis – is nevertheless merely a more explicit way of saying what C & S themselves repeatedly assert as a conclusion to their inquiry: Vervet monkeys do not know that they know, or, they know but the knowledge is not accessible to them. Now, what is knowledge that is inaccessible or unknown yet individually developed with respect to how to relate and act in the species world? It is nothing else but sensorimotor know-how. From inside the sensorimotor psychology (mind) of an animal with this knowledge it is literally meaningless to ask how the world is seen. Sensorimotor know-how is in the action of the species, it is not an object in the mind of the species.

No doubt these theoretical comments can never attain the persuasiveness of empirical evidence, but they carry a logical conviction once the overriding theoretical framework is spelled out. Nor would my criticism be valid if it were just a question of Piaget's theory – and my interest here is not at all Piaget's theory itself but the observations and their adequate interpretations. My claim is that C & S, following the current cognitivist tradition, have an inadequate, largely implicit theory of knowledge that leads not only to logical inconsistencies (as pointed out above), but to anthropocentric (or, more precisely, adult-human-centric) perspectives that seem quite alien to the evolutionary-ethological stance that generated the core contributions in the first place.

A preliminary point is the surprising inclusion – primarily to clarify or reinforce some cognitive argument – of a great variety of other species of monkeys, and also of apes, humans, and birds. Where I would have expected a taken-for-granted emphasis on species-specific adaptations, there is throughout the book the insinuation that more – in the sense of more similar to humans – is somehow better. Besides, is it legitimate to refer globally to the psychology of monkeys and apes when the genetic difference between vervet monkeys and chimpanzees is probably much greater than between the later and humans? I found it intriguing that vervets, who are keenly attentive to sounds and vocal signals, failed to be aroused by the sight of a dummy leopard or to infer the nearby presence of a real leopard from the sight of a freshly caught carcass (p. 284). I am not at all convinced that such a genetic selective inattention needs any more evolutionary justification than the genetic predisposition to selective sensory attention. C & S are prone to speculate on evolutionary possibilities and benefits – and in many instances they unfortunately consider only the hypothetical benefits (not the cost) of a more human-like competence. I would have thought that this human-centered way of doing comparative psychobiology had gone out of fashion a long time ago.

Why should C & S (who must surely know better than I the fallacy of anthropocentrism) appear to give the impression of this bias? I would blame the aforementioned cognitivist approach for this direction. Cognition seems to be taken out of its species context and treated as so many interchangeable and operationally defined specific mechanisms. Again and again C & S ask about the *exact* mechanisms that mediate a particular know-how. They imply that only a series of crucial experiments, preferably in a lab setting, could lead to a definite answer and thereby raise the truth value of heretofore anecdotal observations to a proper scientific status. Female vervets A, B, C have a decreasing rank order within the group. As B and C groom each other, the dominant A approaches. It is observed (p. 82) that of the two females who are approached, the lower ranking C is more likely to yield than B. Several mechanisms of understanding dominance hierarchies are then proposed, including a possible grasp of conceptual transitivity.

Here and in a great number of other instances that document reliable and psychologically fascinating social interactions, the readers are alerted to the supposed need for scientific rigor and the hope of some definite answers. The penultimate substantive chapter, in fact, the longest of the book, on attributing mental states to others, is indeed the high point of this entire cognitive preoccupation. I have only two things to say about it. First, in spite of an impressive series of experiments, there is not a single exact answer concerning the precise cognitive mechanism involved in any particular know-how. This is hardly surprising considering that we could not possibly get an adequate answer in connection with any ordinary human know-how. More pertinent for the gist of my comments, however, is the fundamental question: Why would animal psychologists adopt a theory-of-mind framework?

Unless you take an anthropocentric stance, you would never choose the cognitive mind as an evolutionarily significant goal. Moreover, you are in danger of limiting your perspective to the middle class Western ideology of the past three or four hundred years that emphasizes the role of the ego, the self and its mental state. As C & S so well document, knowledge in the animals under discussion is oriented toward the construction and maintenance of social interactions and in each individual case begins with a period of social dependency and attachment. This fact alone shows that sensorimotor development can be found only within a social context and is by evolution functionally oriented toward the social dimension.

Where C & S describe the vervets' vocalizations they have the brilliant idea to go beyond the usually observed alarm calls – in the service of defense – and turn to vocal communication within a positive social context (pp. 114–28). Their analysis reveals a number of astounding language-like features, at least if you initially assumed some mechanistic, associative signal-action connection. Here again is the focus on the richness and flexibility of sensorimotor know-how that I consider the major scientific contribution of the C & S's research.

I would like to suggest a similar turn from the largely defensive theory-of-mind posture – usually observed in active or passive deception experiments – to the positive interactions that ground a species's social cohesion. On this issue Premack (1988a) documented that humans make attributions by the time they are four years old, whereas chimpanzees' attributions even in a limited respect are questionable. C & S (pp. 252–55) agree that vervets show no evidence of mental attribution, but in the case of apes they speculate on the evolutionary connection between this cognitive competence and social cohesion. They mention briefly (p. 224) the absence of active pedagogy in nonhuman species and its presence in human children. To this can be added the significant fact that in humans successful learning is done in the mutuality of mental recognition and approval. We can conclude that monkeys, apes, and humans all have a theory of society (either sensorimotor or mental) that, in contrast to a theory of mind, is indeed a major evolutionary feature. The entire book amply attests to the fruitfulness of treating knowledge in relation to the social cohesion it serves.

In short, it would seem to me to be more adequate for a biologically grounded comparative perspective to accept species differences in social-societal relations and ask questions about cognitive mechanisms as a function of these differences. This would avoid focusing on some particular cognitive competence (as was done in parts of the present research endeavor) that may turn out to be a derivative specific to human societies and therefore unsuitable to guide a meaningful inquiry into subhuman cognition.

Social and nonsocial intelligence in orangutans

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Primatologists have long debated the relative importance of ecological variables versus social variables as selective mechanisms favoring the rise of primate intelligence. Cheney & Seyfarth's (C & S's) book can be seen as a treatise arguing for the primacy of the latter. In Chapter 9, entitled "Social and Nonsocial Intelligence," C & S argue that many of the problem-solving skills vervets demonstrate in a social context are domain specific and nontransferable to ecological contexts. Consequently, C & S contend that the vervet mind, and by extension the monkey mind, is equipped to solve social problems far better than analogous ecological problems. By contrast, C & S argue, many nonprimate animals excel at solving problems that relate to feeding ecology, yet comparable social skills for such animals have not been reported. As Harcourt (1988, p. 151) notes, the lack of data pertaining to the social lives of nonprimate mammals must surely stem from the fact that

> Many primatologists have sociological and ethological training and are, therefore, ready to recognize and note complexities of social interaction. However, non-primatological mammalogists are far more often ecologists, and neither they nor their studies are designed to analyze social behavior.

Therefore, adopting a dichotomous, domain-specific view of primate versus nonprimate intelligence should be cautioned against, at present.

The dichotomy between social and ecological variables as they relate to intelligence may be more convenient than real; both played a role in shaping the emerging primate mind. On this count it is illuminating that Jolly (1979), who was the first to articulate the social intelligence hypothesis, states, "Are not two or three good reasons for evolving a trait better than one? . . . why must we choose either/or?"

The primate on which my own research focuses, the orangutan, is an excellent case in point that the social intelligence hypothesis and the ecological intelligence hypothesis are not mutually exclusive. Orangutans exhibit higher order intelligence in their ability to store, process, and recall information pertaining to the location and phenological cycle of food sources. Moreover, they demonstrate complex sensorimotor intelligence when handling food during extractive foraging. Given the difficult problems orangutans face in feeding themselves, ample room exists for ecological selection pressures to favor their intelligence. Because of the orangutan's enigmatic status as least social ape, orangutan intelligence has always been regarded as problematic in the context of the social intelligence hypothesis. Nevertheless, orangutans do not live in a social vacuum and evidence exists that they possess a "Machiavellian social intelligence" (Byrne & Whiten 1988). [See also Whiten & Byrne: "Tactical Deception in Primates" *BBS* 11(2) 1988.] Like C & S's vervets, orangutans appear capable of recognizing other individuals, their own personal relationships, and the relationship of others, and they put this social knowledge to use during acts of coalition and deception (Galdikas & Vasey, in press). Hence, even among the relatively asocial orangutans, social factors probably play an important role in shaping orangutan intelligence. In their attempt to assess the nonsocial intelligence of vervets, C & S examine the ability of these monkeys to infer relationships between auditory and visual cues associated with various sympatric species. Given that numerous species demonstrate impressive nonsocial "intelligence" when faced with feeding problems, it may have been appropriate for C & S to examine the vervet's ability to infer nonsocial relationships pertaining to feeding ecology. I suspect that had such a methodology been followed, the vervet's performance in experiments testing for nonsocial intelligence would have been much better. For example, recent work by Menzel (1991) demonstrates that Japanese macaques have detailed knowledge and "inferencing" abilities that are frequently exercised in nonsocial feeding contexts.

These concerns aside, C & S have written one of the most exciting and informative books I have seen in a long time. The breadth of information and issues dealt with are truly impressive. What remains exceptional about *How monkeys see the world* is C & S's ability to make hypotheses about the primate mind explicit and operational in an experimental context. This book will long be cited as a successful investigation inside the mind of another species. The young field of cognitive ethology owes many thanks to the authors.

Perception theory and the attribution of mental states

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Radical behaviorism and indeterminacy of translation arguments (themselves directly inspired by behaviorism – cf. Quine, 1960) long ago forced psychologists and philosophers to reevaluate the principles that guide investigations of human and animal mentality. Cheney & Seyfarth's (C & S's) careful and illuminating study of vervet monkeys provides yet another occasion to consider the conceptual basis of mental state attributions – whether to members of other species or our own. How is it possible to write with confidence about the mental world of another species when questions have been raised about whether (and, if so, how) we can know very much about even the meanings human speakers attach to their words?

One powerful justification for appealing to mental states is the argument that they are required to explain observed behavior.[1] C & S claim the intentional ground for their primates this way by contending that monkeys need to represent both social relations and "meaning in vocal communication" to predict the behavior of their fellow critters and to use vital information about the environment (e.g., the location of a predator) that other monkeys' vocalizations make available. Naturally, C & S use similar arguments to claim the same ground for themselves, maintaining that *they* too are able to predict and explain the observed primate behavior effectively only if they are permitted to move beyond (e.g., Quine's) 1960 behaviorist strictures and make cognitivist attributions of mental activity to the primates.[2] These considerations are compelling, but C & S's case could be further enhanced, and other conceptual tools would be made available to them, if it were possible to link their ethological analysis more directly with other appropriate domains of psychological explanation – specifically to perception theory.

Much of C & S's study deals with what vervet monkeys know (or believe) about their environment and each other based on available perceptual information. They make it clear that these animals need to recognize and react appropriately to an extraordinary range of environmental and social properties. These include kin (they are adept at recognizing their own close relations, as well as those in the matrilines of others in their group), predators, potential mates, food, places affording safety from predators, and limits to their own group's territory as well as those of different groups. In addition, they need to recognize the meaning of a wide range of vervet behaviors signalling aggression, appeasement, dominance relations (in both their own groups and, possibly, in others), requests to form temporary alliances with one monkey against another, willingness to groom or be groomed, willingness to mate, and so forth. Presumably, therefore, it should be possible to establish links between psychological accounts of perception and the kind of ethological investigation under consideration. This would enable us to understand better both the monkeys' perceptual abilities and the use they make of them. Not all perception theories prove of equal value to this project, however. C & S's ethological approach does not fit easily with standard cognitivist or computational accounts of perception. Instead, their research is much more compatible with the more controversial ecological approach to perception developed by Gibson et al. (cf., e.g., Gibson 1979; Glotzbach & Heft 1982; Reed 1988).

Standard perceptual theories tend to define the perceptual project in terms of inferences or hypotheses that the perceiver must construct on the basis of (typically) ambiguous or at least incomplete sensory input (for an example, see Fodor & Pylyshyn 1981; Glotzbach, in press). Moreover, traditional theories tend to define the objects perceived (i.e., the "distal" environmental objects) in terms of their physical properties without any particular reference to the perceiving animal. But none of this has much to do with the task of the field ethologist. The latter needs to determine what environmental features inform the animals of the various *functional* properties of objects in the animals' eco-niche, and these functional properties need to be defined in terms relative to the animals themselves and their specific needs.

The ecological account of perception attempts to do precisely this. It *begins* not with a description of physical stimuli occurring at the sensory receptors but with a description of the environment in functional terms relevant to a perceiver. Such a description uses size and time scales which pertain to the perceiver's body size and activity rates, and it highlights *relational* properties relevant to the perceiver's ongoing projects. Such *relational* properties cannot be defined either as purely objective, physical properties of the environment or as entirely subjective properties of the animal considered in isolation from its surroundings. For example, a tree branch at a certain height off the ground may afford grasping to an adult vervet, but not to a young infant. Conversely, that same branch might afford running under (without ducking) for the infant, but not for the adult. Animals (and humans) routinely perceive such relational environmental properties, but such properties cannot be defined without reference to perceivers and their intended actions. Proponents of the ecological approach have attempted to extend this analysis to describe perceptible cultural[3] and social affordances as well. The latter are features of the social environment which (properly socialized) individuals can perceive as enabling or affording certain kinds of social actions. McArthur and Baron (1983) discuss research that indicates the existence of invariant, detectable stimulus information enabling observers to perceive many different kinds of social interactions, events, and such complex relational structures as "kinship, friendship, and status differentials" (p. 223), as well as emotions and causal relations in interpersonal interactions. Much of the perceptual activity described in *How monkeys see the world* fits neatly with these concerns, and such ethologists as C & S could only benefit from availing themselves of the conceptual tools the ecological approach to perception makes available.

Methodological comparisons between the ethological and ecological projects can be drawn as well. C & S point out important differences between what can be learned about animal mental processes from studies conducted in the wild and those restricted to laboratory settings. This is not to deny the utility of the latter investigations, but C & S emphasize the need to insure "ecological validity in laboratory experiments" (p. 11), a demand that can be met only by attending closely to species-specific relations between animals and their natural environments. Such methodological concerns also characterize the ecological approach to perception. Its proponents have worked to identify stimulus variables that have functional relevance to perceiving subjects as opposed to focusing on the abstract and often functionally irrelevant perceptual tasks that have been so frequently investigated in the perception lab.

Let me conclude by returning to my initial question about the evidence for attributing mental states. I have argued elsewhere (Glotzbach 1983) that Quine's referential inscrutability and indeterminacy of meaning theses are rooted in the overly restrictive account of perception built into his concept of stimulus meaning. A theory of language beginning with a more adequate account of perception – as represented by the ecological approach, an account that explains how perception places us in contact with determinate environmental objects – might offer a greater prospect for determining just what perceivers use their language to talk about. This remains to be shown in detail, but it is reasonable to make a similar conjecture here with regard to the ethological project. If we want to know what is on an animal's mind, we would do well to begin with a rich analysis of the environmental objects and features it perceives. Cheney & Seyfarth make extensive use of just such a methodological principle. The ecological approach to perception provides the conceptual foundation needed to justify that principle.

NOTES

1. Fodor (1981) aptly writes: "The strongest argument against behaviorism is that psychology has not turned out this way; the opposite has happened. As psychology has matured, the framework of mental states and processes that is apparently needed to account for experimental observations has grown all the more elaborate" (p. 115).

2. (Cf. pp. 178ff.) As C & S realize, this position has been explored and defended at length by Dennett (1978; 1987) and explicitly with regard to their own research in Dennett (1983).

3. For an example of a cultural affordance, consider a recognizable metal container of a certain characteristic size, shape, color(s) and location which affords mailing a letter (Heft 1989). Cultural affordances are obviously not relevant to the ethological project under discussion.

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In this best of all possible monkey worlds?

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In *How monkeys see the world,* Cheney & Seyfarth (C & S) have made significant advances toward answering the question of what life is like for a monkey. They bravely go where philosophers have feared to tread (or echolocate), undertaking the journey with remarkable insight and sobriety. As guides on this tour of the monkey mind, C & S recognize and alert us to virtually every philosophical and logistical pitfall that one might encounter in such an endeavor, and they are keenly aware of the limitations of particular methodologies and various lines of evidence. The conclusions drawn from their evolutionary perspective will be eminently satisfying to many, I suspect; monkeys are superb social strategists (their intelligence having evolved in response to *social* problems) but they lack cognizance of these skills and cannot apply them, to any appreciable extent, outside this narrow domain.

In the spirit of *BBS* commentary, I play devil's advocate and suggest that some of the studies discussed by C & S may overestimate the social skills of monkeys, whereas others may underestimate their general intelligence. This suspicion is based on concerns about the interpretation of insufficient and mixed evidence. As C & S note (p. 60), to date, too few experiments provide precisely the kinds of evidence needed for a valid assessment of, for example, monkeys' ability to attribute mental states to others. In addition, too few species have been investigated, and the likelihood of major differences, in particular cognitive abilities among species is great, given what we know about interspecific variation in (micro)social systems and the ecological conditions under which evolution has taken place.

The elusive "perfect" experiment. Rarely will hard-won data from even the most clever field experiments be sufficient for us to be confident about what monkeys know and how they use that information. Consider the issue of reciprocity. As C & S point out (p. 43), reciprocal altruism is highly relevant to work on social intelligence because its evolution will, in cases, be dependent on complex mental operations, including individual recognition, memory, the calculation of costs and benefits for different interactions, and the ability to detect cheaters. Very few well-documented cases of animal reciprocity have been reported – presumably because the necessary cognitive underpinnings for its evolution are absent in most species – and its occurrence in monkeys would argue for a complex social intelligence. No study of monkeys has actually demonstrated a critical component of reciprocity, however: that the animals are sensitive to the costs and benefits of assisting unrelated individuals. Data revealing that cooperative behavior between two animals has a direct and causal influence on future interactions have not been presented. C & S have come closest to doing so. They played one individual's threat grunts (calls given when vervets threaten or chase one another) to an unrelated animal under two conditions: after the two had groomed within the past 30 to 90 minutes and after a period when the same two had *not* groomed in at least two hours. Subjects looked to the speaker significantly longer when grooming had occurred than in its absence, leading C & S to speculate that "when a vervet monkey hears another calling for support, her decision to attend . . . is affected by . . . What has she done for me lately?" (pp. 68–69). A missing control condition – that *any* recent interaction, even a fight, might influence the tendency to attend to the call – is needed before the more cognitively complex interpretation can be entertained.

C & S underscore the difficulty of inferring mental states from behavior elicited from playback experiments, but sometimes answers to even less daring questions about animal behavior are elusive. Clearly, the logistical problems of conducting playback experiments with adequate controls in the field are many, for example, dwindling populations, problems of habituation (and the number of studies described in the book that make use of the technique is testament to C & S's patience and persistence); but often, then, the kinds of data needed to address what the animal knows, and how it uses that information, will be difficult to obtain. I am all too familiar with this problem, encountering it as I have in my efforts to interpret the agonistic screams of macaques.

"The" monkey mind. Most of the data discussed by C & S come from vervets, the monkeys that they have studied intensively and know best. How are differences in social systems and physical environments among Old World monkeys reflected in how the various species view their worlds? Some of the five features of vervet monkey social organization described in Chapter 2 no doubt characterize (to some degree) other species, but how have differences (subtle or profound), where they exist, influenced cognitive evolution? Smuts (1985), for example, reports that male and female baboons form "friendships" that increase the male's reproductive access to those females. Vervets do not exhibit long-term friendships between males and females. Studies by both Takahata (1982) and Enomoto (1978) indicate that Japanese macaques do, but these relations *decrease* the chance that mating will occur between the animals involved. We lack the data necessary to assess how such differences might be significant to the evolution of monkey cognitive abilities, but such variation should make us cautious about generalizing about monkey minds.

Finally, of the many studies and observations reported in this book, the most perplexing for me concern the apparent failure of the Amboseli vervets to respond to cues that (as C & S point out) they logically should attend to, such as the secondary *visual* cue of a carcass in a tree that signals a leopard's proximity. Some might view this as a dreadful failure of natural selection (or of adaptationist hypotheses). The vervets do seem to be sensitive to secondary auditory cues (e.g., cow bells), and C & S speculate about how this difference might relate to contrasts in the ways vervets use visual and auditory signals in their social interactions (the latter signals having referential properties). One might ask why, in the face of clear selective advantages to being sensitive to secondary visual cues, the ability has not evolved – presumably proficiency in one domain (auditory processing) does not preclude it in another. But then it is never very productive to speculate about why some trait or ability did *not* evolve. On the other hand, maybe the vervets are better with secondary visual cues than present tests reveal. C & S (p. 169) describe how vervets attacked by feral dogs on the Cameroon savanna respond with alarm calls and run to the trees, as they do for leopards. Elsewhere, the vervets are hunted by armed men who track the monkeys with dogs. Here the vervets flee silently into dense brush where the hunters cannot follow. In the latter case, the dogs would appear to be a useful secondary visual cue to the approach of hunters.

Cheney & Seyfarth have provided us with a carefully crafted and unified theory of the monkey mind. I am certain they would agree with the suggestion that much additional data will be needed before the final chapter is written on how monkeys see the world.

How autistics see the world

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In their stimulating discussion of monkey communication and socialisation, Cheney & Seyfarth (C & S) mention autism as an example of the dissociation possible between social and nonsocial intelligence (p. 270). We would like to suggest that autism is also an important example for many of the other points raised in this excellent book. Although the authors suggest a parallel between vervet and normal human infant communication (p. 173), an even more informative analogy can be made with the abilities and handicaps of people with autism. These individuals suffer from a circumscribed brain abnormality (from a variety of biological causes) that affects development from birth. The basis of their handicap is a specific cognitive disorder that results in the absence of theory of mind. This in turn leads to specific impairments in imagination, socialisation, and communication (Frith 1989). We raise only a few points here that arise from our explanation of the three core features of autism and relate to issues in primate research.

Pretence. One area where monkeys are more like autistic children than like normal infants, or Down syndrome children, is pretence. C & S conclude that there is little good evidence of pretence in monkeys. Autistic people – unlike normal two-year-olds – also fail to show pretend play (Wulff 1985). It was this observation that was crucial in suggesting that autistic people might also lack a theory of mind, because both require the ability to represent mental states (Leslie 1987).

Socialisation. The hypothesis that autistic people – like C & S's vervets – lack a theory of mind, can account well for the core handicaps of autism. Like monkeys, however, autistic people *are* capable of social skills that do *not* involve a theory of mind. So, for example, autistic children can use sabotage (manipulation of behavior) to achieve a desired end, although they cannot use deception (manipulation of beliefs, Russell et al. 1991; Sodian & Frith, in press). Primates, too, probably manipulate behaviour rather than mental states. Sabotage is seen, for example, in the bonobos who removed the means of escape from the moat to leave individuals below stranded (p. 211). Such behaviour seems to indicate an understanding of goal-directedness, and a parallel, rudimentary understanding of desires as drives seems to be present in autism (Baron-Cohen 1991a; Tan & Harris 1990). Autistic people can also understand and manipulate seeing (Hobson 1984; Leslie & Frith 1988) in much the same way a monkey or ape may be able to conceal information, for example, by hiding from dominant males behind a rock before grooming a subordinate male (p. 191).

Communication. C & S make a distinction between "calls that provide information only about the signaller's emotional state or subsequent behaviour . . . and calls that denote a specific external referent" (p. 104). This is clearly a useful distinction when the signaller's intention to signal is in question, but it may be that the distinction becomes less useful when dealing with deliberate communication – such as the vervets' vocalisations. Here a distinction between internal (as cognitively simple) and external (as more complex) referents could be misleading. Confusion could arise because the two categories above overlap with two very different categories that have been found to be useful in looking at the quality of autistic communication.

Expressive versus instrumental communication. In autistic people it has been fruitful to examine whether the gestures and speech produced are protodeclarative or protoimperative. Protodeclarative gestures indicate an external referent to communicate something about the signaller's internal state ("Look at that bird; I'm interested in it!"). Protoimperatives, on the other hand, indicate an external referent to achieve a behavioural end ("Look at that bird; give it to me!"). The cognitive and social sophistication underlying these two categories, however, is precisely the reverse of that ascribed to C & S's classes – protodeclaratives are more advanced because they require a theory of mind. Not surprisingly, then, protoimperative pointing is understood and used by autistic children, but protodeclarative pointing is not (Baron-Cohen 1989b). Similarly, autistic children use instrumental gestures but not expressive gestures (Attwood et al. 1988).

Ostensive versus coded communication. The protodeclarative/protoimperative distinction is also related to the distinction between ostensive-inferential communication and coded communication (Sperber & Wilson 1986; see also multiple book review of *"Relevance" BBS* 10(4) 1987). Ostention is behaviour that alerts others to and makes clear the actor's *intention* to convey some piece of information. We would predict that neither the autistic person nor the monkey would be capable of ostensive-inferential communication – because neither has a theory of mind capable of systematically representing and manipulating intentions. On the other hand, communication in autistics and monkeys can still be achieved by means of coded communication. Unfortunately, coded communication is relatively inflexible, because signals have a set meaning that is not dependent on contextual factors or the signaller's intention. In autism, the single-word instrumental speech and echolalia commonly seen (Paul 1987) may be a form of coded communication (i.e., "apple" always means, "Give me apple!").

No!" In monkeys the availability of coded communication and the lack of ostensive-inferential communication may explain the absence of an equivalent for the word "No!" in vervet vocalisations (p. 226). Humans use the word "no" in a flexible way; its precise meaning varies with context, because we understand this "vocalisation" in terms of a speaker's intention rather than as a code. Such flexible "loose usage" would seem to be impossible without an understanding of mental states (Happé 1991). It is not yet known how flexibly autistic people use this term, but we would predict that they use "No!" as a code with a set meaning (such as "Go away!").

Pedagogy. The presence of ostensive behaviour would greatly increase the likelihood of pedagogy. Unless individuals pay attention preferentially to ostensive versus nonostensive behaviour the opportunity for teaching is severely restricted. Ostention should receive preferential attention because it carries a guarantee of relevance (see Sperber & Wilson 1986).

Social and nonsocial intelligence. C & S claim that "a crucial distinction between humans and other primates may be that humans are better able to generalise, or extend, skills used in social interactions to nonsocial domains" (p. 262). It seems a plausible and exciting possibility that awareness of mental representations preceded and provoked the use of other representations, such as notational systems, in evolution. Within the individual and on the developmental time-scale, however, there seems to be relatively little proof of generalisation of social skills to nonsocial domains. In normal children, for example, social skills are relatively independent of other abilities (Nunez & Riviere 1990). Similarly, there are autistic people who have an above average IQ despite their handicaps in imagination, socialisation and communication (Gillberg 1991). In contrast, some high-functioning autistic people may be able to use their nonsocial intelligence to unravel the mysteries of social situations. These people seem able to solve social problems only by using their general intellectual capacities, or perhaps by reversing the evolutionary story and using such nonmental representations as pictures as a model for understanding mental states. Close connections between social and nonsocial intelligence in humans may therefore be a feature of abnormal rather than normal development.

We would like to suggest that some light might be thrown on the question of why social and nonsocial intelligence are so distinct in primates by studying their understanding of such nonmental representations as maps and photographs. The understanding of nonmental representations may tell us whether monkeys are more like young (pre-theory-of-mind) normals or more like autistic individuals. While autistic children seem to understand the representational nature of photographs (Leekam & Perner, in press; Leslie & Thaiss, in press), normal 3-year-olds do not understand "false" photographs (Zaitchik 1990) and have difficulty using a model as a map (DeLoache 1987). If primates can understand the representational nature of maps, models, and photographs, could they perhaps be taught to use such nonmental representations as models for understanding their own thoughts?

"How monkeys see the world." Why monkeys?

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"This very richness of information [on wild animals solving complex problems] makes it all the more challenging to specify how animals and humans differ," Cheney & Seyfarth (C & S) write. "As we hope to demonstrate, the social behavior of nonhuman primates offers a glimpse of almost-minds at work" (p. 4). C & S have jumped from using the cognitive abilities of all animals to tell us something about our own cognition to using just primates. Why is this a book about the cognitive abilities of only monkeys (and apes) and not about those of all animals? One answer is that monkeys are what C & S have so preeminently studied. But they review others' work, and their aim is to understand animals' minds, not just monkeys' minds. More specifically, C & S's interest is in the possible adaptive advantage of higher cognitive abilities, which means studying how animals use their minds in the natural environment. In the discipline of animal cognition, this is a huge step, but a vital one if we are to understand the evolution of the human mind. Most animal psychology is done in the sterile physical and social environment of solitary testing in the lab. No wonder it has told us so little about the evolutionary functions of mental abilities. So why concentrate on monkeys' minds? The answer lies in part in the long-standing assumption that primates are both more intelligent and more complex socially than nonprimates, and that the two attributes are functionally connected: Intelligence allows complexity, and survival in a complex social environment selects for intelligence.

Just how obvious is it, though, that primates are more socially complex, more socially intelligent than nonprimates? Some years ago I was asked to write a chapter for a book on social intelligence in primates, using data from my studies of wild gorillas. I replied that if I were to be totally objective, I could not see that gorillas were doing anything much more sophisticated than a sheep could accomplish. That was only a slight exaggeration, and it led me to ask what, if anything, did in fact distinguish primate from nonprimate levels of social intelligence.

Agreeing with others before me that social interactions involving three animals were probably more complex than those that involved two, I went to the literature on coalitions and alliances in contests. Do primates, I asked, use cooperation in competition differently from the way nonprimates do? The literature indicated that they might (this sentence is phrased very deliberately). In both primate and nonprimate societies, cooperation in contests temporarily and sometimes permanently improves the helpers' or the recipients' competitive ability; in both, individuals apparently make use of information about the relative competitive ability of contestants by preferentially joining contests on behalf of the more dominant individual, other things being equal; but only in publications on primates could I find evidence that individuals preferentially direct friendly behaviour up the hierarchy when no immediate benefit is apparent. One interpretation, in strictly functional terms, is that primates, but not nonprimates, use information about potential quality of group members as partners to establish friendly relationships now for future payoffs. Only primates, it seems, manipulate the probability of receiving help in the future from particularly valuable partners. For nonprimates, a coalition is an end in itself; for primates it is a means to an end.

Many peoples' work led to this conclusion, but C & S's highly elegant and sophisticated questions and analyses of social behaviour of monkeys in the wild weighed very heavily in the survey. Nobody else has taken the analysis of social complexity and of the implied cognitive abilities as far as they have. Least of all has anyone in this area taken the ethological tradition of experimentation in the field as far. Nobody is saying that the primates are consciously calculating future benefits against current behaviour, but the apparent taxonomic difference in nature of social relationships at least hints where to look for complex mental abilities. Maybe it is now up to the laboratory psychologists to work out the minimum cognitive rules necessary to programme an animal to produce the behaviours seen. The sterility of the laboratory, however, surely constrains the animals' behaviour. Perhaps only those who watch animals in the full moment-to-moment intricacy of survival in the wild can approach a realisation of the potential complexity of the social, and hence cognitive, abilities of the animals?

If the question of taxonomic differences is worth asking, how should we progress? We most certainly need to look at more species, not just of nonprimates but also of primates: The apparent dichotomy relies on evidence from just two families, the Pongids and Cercopithecines, and four genera of socially complex primates. In their statements about the advanced social intelligence of apes, C & S tend not to distinguish gorillas from chimpanzees. They have watched gorillas in the wild and believe that, despite appearances to the contrary, they are intelligent. So why do the animals not show it? In their environment, they might have no need to, for food is abundant and widespread. If the use of cooperation in contests is a mark of social complexity, then we need to study a species that uses resources, access to which is improved by cooperation. Finally, having chosen the species to study, we should use C & S's volume as the manual of what behaviours to study and how to study them.

In suggesting the course of action that I have – study more species and do it as C & S have – I am assuming that comparative psychology is an important discipline and that field experimentation produces valid answers of a sort difficult to produce in the normal animal psychologist's laboratory setting. I also assume that searching for a primate/nonprimate dichotomy is worthwhile. If success in solving complex social problems requires advanced information processing abilities, then the obvious difference between primates and nonprimates in the size of their information processing organ provides some expectation, if only crude, of where a difference might be found. After all, it is partly the size of our own large brain that leads us to expect a human/nonhuman dichotomy in cognitive abilities. Whether or not a difference exists, the search will help to "ultimately tell us how, in the course of our own evolution, some minds gained an advantage over others" (p. 4), as long as we remember that the abilities evolved not in animals placed alone in a steel box with pedals, but in the wild with its bewildering array of daily physical and social problems.

Are monkeys nomothetic or idiographic?

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"Are monkeys and apes as skilled at monitoring each other's states of mind as they are at monitoring each other's behavior? Do they recognize the distinction between mental states and behavior, either in themselves or others? Do they know what they know?" (p. 206). These are the fundamental questions asked in Cheney & Seyfarth's (C & S's) marvelously researched and well-documented book. The answer? Tentatively, no. According to C & S, "monkeys are primatologists who have spent too much time studying a single species" (p. 180); their reasoning consists of domain-specific computations and is restricted to first order intentionality (i.e., there is intent to modify the behavior of others, but no intent, or even conceptualization of the ability, to modify the thoughts of others).

C & S seem surprised at their own answer, because they have assumed throughout that "the moment that an individual becomes capable of recognizing that her companions have beliefs, and that these beliefs may be different from her own, she becomes capable of immensely more flexible and adaptive behavior" (p. 206). This argument is similar to that of Humphrey (1983), who hypothesized that human consciousness evolved because it enhanced our ability to predict one another's behaviors, thereby increasing the likelihood that we would interact with others in an adaptive fashion. Plutchik (1980) argued in a similar vein that the higher emotions evolved because they enhanced the likelihood of responding appropriately to external stimuli, especially social stimuli. I am not so sure that this assumption of adaptive value is correct, however.

At one point in the book (p. 252), C & S acknowledge the possibility that for a monkey, responding to the relevant aspects of the world might not require second order intentionality. They also document throughout the text that in spite of an apparent lack of second order intentionality, monkeys are very good at manipulating the behavior of other monkeys and are able to use what is essentially correlational data (learned contingencies), to behave in ways that might seem superficially to imply an understanding of other monkeys' behavior. The monkeys' nomothetic (actuarial or statistical) approach to the world is apparently accurate and reliable and does not seem to necessitate the utilization of additional idiographic (individualistic) hypotheses. In fact, the assumption that an empathy-based, idiographic approach to predicting the behavior of others is necessarily better than a contingency-based, nomothetic approach, seems to me a species-specific cognitive bias which may not even be correct for our own behavior, let alone that of another species. (See, e.g., Monohan, 1982, for a review documenting the clinical bias in favor of the idiographic approach in spite of the superiority of the nomothetic approach for predicting violence in humans.)

Reading C & S's description of monkey cognition reminded me of the time a student asked me if a psychopath would make a good psychologist. After thinking, I responded that I guessed it would depend on what one was looking for in a psychologist: By definition, psychopaths would be lacking in empathy, but on the other hand, they would excel at being able to manipulate the behavior of others by pulling all the right strings and pushing all the right buttons. The psychopaths' success at manipulating the behavior of others results in an apparent understanding of the thoughts, feelings, and motivations of others, that is, in an apparent empathy. Yet given the known absence of such empathy, it is presumably psychopaths' average to above average intelligence that is being used to learn the contingencies relating others' behavior to their own, and with which they (and presumably the monkey) manipulate others so well.

Although C & S have convinced me that they won't find empathy and second order intentionality in monkeys, I hope that they continue to look. Functionally, with respect to the monkeys' behavior among themselves, it will make no difference what they find. But our own bias toward putting such supreme value on empathy and intentionality in human interactions has caused us to undervalue other species that don't have empathy, or at least with whom we don't share it. In this sense, we have become the psychopaths of the world, manipulating things to our own advantage and to the detriment of others. (C & S point out in Chapter 2 that the monkey populations they studied fell from 215 individuals to 35 individuals during the period of 1978 to 1988, and that much of this reduction in population was from human-related habitat destruction.) I hope that we keep looking for empathy and intentionality in monkeys because I want it to be there; on the other hand, I hope that we never find the answer, because I am afraid that if the answer is no, we may find that we have no room left in our world for monkeys.

What are mental states?

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The aspect of the text that is the subject of this commentary turns on what "mental states" may legitimately be attributed by human observers to the other species whose behaviour they study, and what attributions and understandings those creatures have with respect to each other. It is never a matter of doubt for Cheney & Seyfarth (C & S) that other animals have these "mental states," whether or not they behave as though they do: "Animals may be able to behave adaptively simply by attending to the behavior of others; they may not *need* also to attend to their mental states" (p. 206).

We are even asked to entertain the possibility (following an account of an experiment in which a parent monkey communicates nothing to its offspring about salient features of the environment) that "monkeys *do* recognize the difference between their own knowledge and the knowledge of others, but . . . this recognition simply has no effect on their behaviour" (p. 223).

The question must be faced: What are these "mental states" that are so readily invoked, and invoked as "causal agents" (p. 209)? Are we talking about states of the visual cortex, the hypothalamus, the cerebellum? It appears not; rather, C & S use Dennett's (1983) phrase about "beliefs and desires" as mental states. If that is what we are considering, how are these states *mentally* manifested (as against behaviorally expressed), and how, in the absence of their behavioral expression – as in the experiment mentioned above – might these mental states be accessed by conspecifics? This is a puzzle that has gripped biologists and psychologists to the point where "thought transference" by means of ESP has been seriously proposed as the solution (Huxley 1983, p. 18). [See also Alcock: "Parapsychology" *BBS* 10(4) 1987.]

C & S don't seem to go that way, although their account of Menzel's (e.g., 1971) observations (p. 238) on chimpanzee communication couches it mysteriously. A chimpanzee shown a hidden food cache is returned to the holding cage that contains other chimpanzees and a few minutes later all are released from the cage. "There was no indication that the first chimpanzee (termed the *leader*) communicated what he had seen to the others before being released . . . Nevertheless, once released, the others behaved as if they knew not only *what* the leader had seen but also approximately *where* he had seen it" (p. 238). Any close reading of Menzel (e.g., pp. 228–30) will dispel the mystery – the conduct of the chimpanzees is readily understood in terms of their (sensorially) perceiving the contingencies of the micro-world (or macro-Skinner box) in which they are impounded. These include, for example, learning to associate the leader's level of excitement, and rate and directness of travel, with the experience of successful food-getting.

The contrast is drawn between the attributions of mental states that young children can achieve, and the evidence for such capacities in other primates. C & S provide an account of the emerging cognitive skills of children between the ages of about three and six years – the capacity to perceive that one has, and that others have, beliefs and desires; the capacity to perceive that others may harbour false beliefs or be ignorant of what one knows; the capacity to perfect the telling of lies. Not noticed in this account is that we are talking about creatures gaining these fairly "hard-won" capacities while immersed in cultural worlds of language-based experience – stories and fables; instruction in reading, drawing, writing; exposure to the narratives of TV. Instead, we are confronted with the prospect that these kinds of higher-order capacities are possibly resident in the "minds" of other primates, but somehow just a bit trickier to find evidence of.

C & S are frank in saying that monkey vocalisations do not qualify as language, and it comes through strongly that, for all their complexity, vervet calls seem to be less intricate than those of chimpanzees. The common ancestor between chimpanzees and humans looks to have been around 5 million years ago, and in our arguments (e.g., Davidson & Noble 1989; Noble & Davidson 1991) the point made is that from the archaeological evidence the hominid ancestors of modern humans should be seen as more like other apes in their behaviour than they were like humans. The vervet studies emphasise that similar issues must also be considered concerning the evolutionary separation of monkey and apes, which may have occurred about 18 million years ago (Groves 1989, p. 189). We note, incidentally, that those issues will require explanation by evolutionary processes similar to those proposed in Davidson and Nobel's arguments and that "special" features of humans should not be invoked to account for either branching.

The evolutionary emergence of human abilities involved the same processes of random appearance of new variation together with its directed selection. C & S's study of natural communication in vervets shows in what arenas complexities of communication may have selective advantage. The discussion of the relation between communication and predation (p. 113) demonstrates how natural selection can operate differentially on communicative performance. Equivalent research is urgently required into the natural communicative habits of chimpanzees and bonobos.

The grace of this fascinating book is the integrity of its authors and the richness of its narrative detail. Being described is a vast range of behaviour, and its varied expression within individuals, so that the student of evolutionary ethology can witness the bases (as in behaviour that functions to manipulate or mislead conspecifics) for later selection once the means to deliberately exploit such conduct have become available. The descriptions and analyses raise more questions than answers and in so doing put many significant issues about perceiving and communicating in sharper focus.

Calls as labels: An intriguing theme, but one with limitations

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Cheney & Seyfarth's (C & S's) is an excellent book, which bears this pair's usual trademarks – creative use of biologically valid field and laboratory methods to test hypotheses of substantial current importance and visibility. The writing is clear and entertaining, and the treatment of the empirical and conceptual literature is scholarly. If I were to offer a seminar in animal cognition, this book, with its emphasis on natural behavioral systems, would be one of my principal sources.

Like any thematic emphasis, the cognitive focus of this work as channeled research toward some fascinating discoveries and away from other topics, including some that I would enjoy learning more about. The questions I raise deal with the nature and ontogeny of communicative processes. C & S's interest in cognition, and especially their emphasis on vocalizations as labels, appear to me to have biased them toward a view of communication that is relatively static. The result, I propose, has been an approach that is insensitive to the more dynamic properties of communication and its development.

Might vervet calls be used in tonic communication? Most or all of the data on antipredator calling by vervets is based only on the first vocalization from each encounter. Given C & S's interest in the possibility that calls serve as labels for predators, this seems like the most rigorous methodological step to take. Only those calls that are free of the influence of the vocal labeling activities of other vervets should be included in the data set. A potential cost of this step, however, is that our attention is deflected from *most* of the calling that occurs, because vervets engage in prolonged vocal choruses during and after a predatory encounter (p. 219). Some consideration is given to the significance of such persistent calling, that is, during snake mobbing (p. 219), or as confirmation to "correctly" calling infants (p. 137). C & S generally ignore prolonged vocal responses, though, or even suggest they are a nonfunctional byproduct of vervet cognitive limitations (an inability to judge whether or not their audience is informed – p. 219). The immediate evocative effects of such calling may be lower than for initial calls. (Imagine the futility of yelling the label, "Leopard! leopard! leopard!" over and over again.) Persistent calling might still be functional, however, but toward different ends and in another time frame. Others (e.g., Smith et al. 1977) have proposed that persistent antipredator calling reflects a tonic communicative process in which individual vocalizations serve to maintain or foster a longer term outcome with repeated inputs, rather than producing a discrete effect with each vocalization (Schleidt 1973). During encounters with mammalian predators, California ground squirrels shift from initial nepotistic warning calls to a non-nepotistic repetitive vocal pattern that tonically sustains vigilance in others, perhaps to maintain an "early warning system" for the caller (Loughry & McDonough 1988; Owings et al. 1986).

Might calling by infants be appropriate for their age-specific circumstances? Another effect of treating antipredator signals as labels for predators is that when infants use calls differently from adults one is disposed to treat the infants as mistaken, or as not yet having refined the definition of the call (p. 225). Ontogeny, then, becomes the process of achieving adult levels of competence in signal use. C & S make a reasonably compelling case for this adult-focused view. Infant vervets really do behave as though they were more ignorant than adults about the quality and quantity of threat posed by various heterospecifics; this greater ignorance is revealed by the infants' tendency to call in reaction to both predators and nonpredators, and by the greater incidence of maladaptive responses to antipredator calls (pp. 129–36). Infants may in fact need the confirmation of adult calling in order to refine their assessment of heterospecifics (p. 137).

Although I do not contest this adult-focused approach, an age-specific viewpoint generates a complementary set of questions, that is, about how infant calling might be appropriate for the conditions under which vervets live at that developmental stage (e.g., see Galef 1981; Owings & Loughry 1985). For example, given that infants are ignorant, how do they cope with the resulting uncertainty? Perhaps occasional barks at warthogs reflect not mistaken leopard labels but efforts to recruit adult aid and guidance. Such initiatives by youngsters might be expected because adults show little tendency to teach their offspring about predators (p. 133).

Some of C & S's data are compatible with the general idea that infants do not attempt to cope with predators in the self-sufficient style of adults but use parents as a source of guidance. The sound of alarms often evokes protective behavior by the mother (p. 226), as well as flight to the mother by very young vervets (p. 135). If infants look toward adults before responding to an alarm call their behavior is more likely to be appropriate for the call's typical predatory elicitor than if they do not glance at an adult first (p. 136). Furthermore, infants evoke potentially important feedback by calling; adults chorus if a predator has elicited the infant's call but remain silent after infants call at nonpredators (p. 137). Such feedback might not only shape the development of knowledge about predators, but it might also be used immediately by infants to judge the danger they face.

Has the precision of predator labeling by infants been underestimated? The social dynamics of antipredator calling, combined with age differences in knowledge about heterospecifics, may have yielded an inflated estimate of the vocal "mistakes" infant vervets make. Given that youngsters really are less precise than adults, infants are statistically less likely to be first callers when a real predator is in the vicinity (because older individuals will also call), but they are more likely to call first when the disturbance arises from a nonpredator (because no other ages are likely to call). So the sample of infant first calls is probably more biased toward the nonpredator incidents than if infants had been tested alone. Such adult pre-emption of infant antipredator behavior has also been observed in California ground squirrels and can mask a remarkable amount of adult-like antisnake knowledge among infants (Coss & Owings 1985).

These comments are derived from a management approach (Owings & Hennessy 1984), which treats participation in communication, in part, as a multi-leveled process of managing the behavior of others. Application of a similar process-based approach to cognition would shift attention away from the contents of mind and move toward the processes of knowing (Michaels & Carello 1981).

Exploring the "boundary" between the minds of monkeys and humans

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Cheney & Seyfarth's (C & S's) book is a notable addition to the exploration of the cognitive differences between humans and other animals, an undertaking that has had religious and ideological as well as scientific motivations. Like many who enter the region from the animal behavior direction, C & S present evidence indicating that nonhuman primates can lay claim to more territory than traditional maps show. Careful observational and experimental field studies of monkey communication and social behavior undercut the anthropocentric view that monkey behavior is restricted to Dennett's (1987) zero order intentionality region, in which behavior is produced by mindless, reflexive responses to internal and external stimuli.

The strategic flexibility shown by monkeys in generating dominance and kinship structures and their ability to recognize classes of social relationships involving other monkeys as well as themselves require mental operations extending into the realm of first order intentionality, that is, action guided by goals and expectations. This implication is buttressed by evidence that monkey calls communicate information about environmental referents and are aimed at influencing the behavior of other animals, rather than being mindless expressions of internal motivational states.

After persuasively presenting the case for extending monkey cognitive competence into areas that some would reserve for humans, C & S refocus their discussion to emphasize a new boundary beyond which monkey cognition cannot move. Although monkeys shape their behaviors to influence other animals, they are shown to lack second order intentionality, that is, action guided by an understanding of its impact on the cognitions of its intended target. Despite the similarities in some aspects of the social systems of monkeys and humans, we are told that the two are generated by rather different mechanisms. Monkeys are shown to act to influence what their partners will do, not what they will think. Humans, on the other hand, are said to generate social patterns through a consideration of the impact of their actions on what their partners think or feel.

C & S draw the boundary between the mental mechanisms that produce the social systems of humans and other animals in front of second order intentionality, although not without some doubt about the placement of apes. This conclusion is quite consistent with the view taken by modern social psychology (Markus & Zajonc 1985). From this perspective, action is seen as the consequence of actors' understanding of the situations in which they act and a prominent part of this understanding involves attributions of mental states, *viz.* intentions, beliefs, attitudes, and so forth, to others (Jones 1990).

But boundaries can be breached in two directions. While animal ethologists and sociobiologists have been examining the possibility of higher order cognitive mechanisms among nonhumans, human ethologists and sociobiologists have been exploring the operation of lower order mechanisms among humans. Recently, some social psychologists have cast doubt on whether we always attribute mental states to our partners in social interaction (Weiner 1985). They suggest that we think about the mental states influencing the actions of others mainly when their actions are unexpected or have a negative impact on us. Thoughts about others' experience or knowledge may not occur in interactions that follow predictable, nonaversive paths. In such settings our attention may be focused on our goals and not on the minds of our partners. There are also human actions that occur at the lowest, mindless level of intentionality (Langer 1989). Such nonverbal human communicative behavior as smiles, assertive swaggers, dejected slouches, and so forth are common examples of zero level behaviors that can have significant social impacts. Important preferences among stimuli and perhaps among actions as well may have noncognitive determinants (Zajonc 1984).

If zero and first order intentionality plays a substantial role in the production of human social behavior, why do most social science theories of action agree with C & S about the ubiquity of higher order intentionality among humans? The answer may lie in how we justify or give accounts of why we act as we do. Because the goal of such explanatory interaction necessarily involves affecting the beliefs of others, we may simply carry over this orientation to the actions being explained. That is, the adoption of second order intentionality in the action of explaining ourselves to others may be inappropriately generalized to the actions being explained. In addition, introspections about behaviors characterized by zero and first order intentionality may be misleading because our responses may be influenced more by implicit theories about how our behavior is supposed to be determined than by actual recollections of the cognitions surrounding our actions (Nisbett & Wilson 1977). The assumption that human social interaction almost always involves considerations of the minds of partners may form a central part of laymen's implicit theories of action as well as of social science theories.

Questioning the role played by thoughts about the minds of others does not imply that these cognitions have no influence on human social behavior. Rather, it indicates uncertainty concerning how and when our actions are influenced by our intentions, purposes, and reasons (Quattrone 1985). This uncertainty should make us wary of placing all human action in the area of high level intentionality. The boundary setting task is made even more difficult by the possibility that evolved strategic constraints might influence some of the cognitive mechanisms involving considerations about the minds of others just as they constrain the lower level mechanisms seen in monkeys. For example, we may be particularly likely to think of social interactions as involving occasions for reciprocal exchange and especially to be alert to signs that our partners intend to cheat (Tooby & Cosmides 1989).

Although there is no reason to expect a book about monkeys to examine human mechanisms in detail, it would be unfortunate if readers interpreted C & S's persuasive justification of limiting monkeys to first order intentionality as simultaneously justifying a boundary between monkeys and humans at that point. Recognition of the operation of lower order intentionality among humans is important because it raises the question of how mechanisms producing this kind of action interact with ones that involve reliance on higher order intentionality. Answers to such questions should help us provide a more integrative account of human behavior than is possible when we assign monkeys to one cognitive territory and humans to another. It may be time to replace the boundary metaphor with one that focuses us on clines or transition zones.

On (not) attributing mental states to monkeys: First, know thyself

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It is clear from their twin chapters on deception and attribution (Chapters 7 & 8) that Cheney & Seyfarth (C & S) want to know whether nonhuman primates attribute mental states to themselves and to others. Unlike investigations of operant learning, however, comparative studies of social attribution must ask whether animals possess a capacity that is at present only known to exist for certain in humans. As a consequence, it is we who must initially be used as a standard for comparison. Thus, although we do not believe that monkeys and apes are developmentally arrested children, we advocate systematic comparative studies of human children and nonhuman primates. In particular, we believe that there are two compelling reasons why studies of attribution in nonhuman primates must begin by testing children. [See also Parker & Gibson, "A Developmental Model for the Evolution of Language and Intelligence in Early Hominids *BBS* 1(3) 1979; Chevalier-Skolnikoff "Spontaneous Tool Use" *BBS* 12(3) 1989; and Greenfield "Language, Tools and Brain" *BBS* 14(4) 1991.]

First, part of the challenge in developing tests of attributional capacities for nonhuman primates has been to create nonlinguistic variations of experiments traditionally conducted with children. To be sure that our linguistic tasks are really measuring the capacity in which we are interested, however, we must first demonstrate that children respond the same way to both variations of the test (linguistic and nonlinguistic). For example, let us suppose we wished to investigate the attribution of false belief in orangutans. First, a behavioral paradigm to test for its presence would be created. Next, a set of age-related hypotheses about the expected performance of young children would be constructed from existing data on the attribution of false belief in children, and appropriate-aged children would be tested using the paradigm that was to be used with the orangutans. If the task sorts children into age-classes according to previous research using linguistic tasks, we can be reasonably confident that the task is measuring false belief. If children younger than 4 years old solve the task easily, however, we should be conservative and accept the null hypothesis until there is unambiguous evidence that 3-year-olds do, in fact, possess a clear understanding of false belief.

The second reason to test children first is that if we cannot characterize a 3-year-old's theory of mind, how can we expect to characterize a vervet's? In Chapter 8, C & S do draw on the child development literature as a guide to understanding the minds of nonhuman primates, but their use of this material is patchy and at times inconsistent. They rely on a model that posits children moving from an egocentric theory of mind to a partial one in which they understand ignorance but not false belief, until finally achieving an understanding of false belief. A number of other frameworks that exist in the field of developmental psychology for conceptualizing how the child's theory of mind emerges, however (see Table 1 for a summary of some of the positions), and we refer to these frameworks as we proceed. Let us look at a few of the topics they address and help to expose their complexity.

Early language use. Following Bretherton (Bretherton et al. 1981; Bretherton & Beeghly 1982) and her colleagues, C & S argue that even 2-year-olds express explicit knowledge of their own and others' intentions, moods, and actions because they use words that refer to these states. Yet this same logic would force us to conclude that 2-year-olds have some adult-like understanding of such words as "know" and "think." Other pos-

Table 1 (Povinelli and deBlois). *Some theories of the child's theory of mind.*

Perner (in press) Flavell (1988)	Children younger than four years old have a behavioristic understanding of "mental states"; "to know," for example, is equated with "to act successfully"; older children develop a representational understanding of knowledge states akin to our own.
Leslie (1987)	Symbolic play is evidence that three-year-olds (and even younger children) can conceive of mental states as representations. These young children, however, fail to understand two critical aspects of mental states, (1) how mental states are formed (hence they attribute knowledge "miraculously") and (2) that mental states are what cause behaviour to occur.
Bartsch & Wellman (1988) Chandler et al. (1989) Bretherton & Beeghly (1982)	Even young children have some understanding of mental states as representations. That is, how mental states are caused and how they affect behaviour.
Siegal & Beattie (1991)	Young children understand the mind in terms of representational states (including false beliefs), but sometimes fail to respond to linguistic interrogation correctly because they fail to understand the experimenter's purpose when questioning them.

sibilities exist. Children may inhabit a fundamentally different social world where such words as "know" and "want" refer to contingencies between expectations and actions (the frameworks of Perner [in press] and Flavell [1988] in Table 1). Indeed, Piaget argued that very young children use words like "bicycle" in a preconceptual way. If children are using such concrete words differently from the way we do, why should we believe that their spontaneous utterances of "think" and "know" and "pretend," reflect an awareness of abstract mental states as opposed to behavior?

Symbolic play. Following Leslie (1987, see Table 1), C & S also adopt the position that symbolic play is evidence for an understanding of mental states. In particular, they claim it as evidence of an understanding of the difference between appearance and reality (p. 207), an ability to possess several different (mental?) representations of objects or behaviors (p. 207), an ability to manipulate knowledge in themselves and others (pp. 243–44), and the presence of self-awareness. Unfortunately, all of these conclusions are open to doubt. Symbolic play begins to emerge at the end of the sensorimotor intelligence stage and is signalled by the child's willingness to use one object (or person) to stand for something else. But contrary to what C & S seem to imply, this ability may reveal nothing about the child's conception of "appearance" and "reality" as mental states. Indeed, at least in the visual modality, children do not understand the appearance-reality distinction until about 4 years of age (Flavell et al. 1989). Take the case that C & S find striking – two young children involved in a pretend game together where they verbally agree to use, for example, a banana as a telephone. Should this be taken as evidence that the children are attributing mental states to each other? The problem, again, is how best to interpret their use of language. For all we know, the children may merely be agreeing to use the banana in the same way that they use a telephone. Do they *know* that it is not a telephone? Yes, of course, and this is what Piaget referred to as the use of objects as *concrete* symbols – using one object to stand for another. But this necessarily precedes the child's conception of *mental representation*. Indeed, rather than seeing it as an indicator of an awareness of mental states, Piaget saw early symbolic play as a consequence of the child's difficulty in grasping the social and representational aspects of language. For the same reason, we find C & S's claim that symbolic play is evidence of early attempts at manipulating knowledge states suspect. Finally, C & S offer no explicit support for their argument that symbolic play is evidence of self-awareness.

Self-recognition and self-awareness. Despite the fact that they cite a number of the key papers in the self-recognition literature, C & S fail to address the central issue that emerged from this research nearly a decade ago. Indeed, the conclusion of their abstract – that monkeys, but not chimpanzees, lack a theory of mind – has been repeatedly stressed by Gallup and his colleagues for nearly a decade (Gallup 1982; 1983; 1985; Gallup & Suarez 1986). C & S appear completely unaware of Gallup's argument that self-recognition is an empirical marker of the onset of self-awareness and that self-awareness provides an intuitive access to the minds of others (cf. pp. 241–42). Beyond this, it is unclear what new meaning C & S are trying to impose on the term self-recognition. Their definition – distinguishing self from others – would mean that we would have to talk about certain species of flowering plants as being capable of self-recognition (Haring et al. 1990). We understand C & S's desire to define self-recognition as distinct from consciousness, but the existing literature treats self-recognition as a specific *behavior* (the act of producing self-directed behavior in front of a mirror), which requires some degree of self-awareness (or consciousness of the categorical self). The distinction they attempt to draw is best left to two terms which refer to mental *processes* – self-perception (their "self-recognition") versus self-conception (self-awareness; see Butterworth, in press).

In a recent series of experiments in conjunction with our colleagues, we have attempted to subject Gallup's idea that self-recognition is a behavioral marker of a theory of mind to its first empirical test (Povinelli & deBlois, submitted; Povinelli et al., 1990; Povinelli et al., in press b; submitted). For example, in one test, we sought to determine whether chimpanzees and rhesus monkeys understand the causal connection between seeing and knowing. Each subject was given the opportunity to follow the advice of two people – one of whom had seen where food was hidden, the other had not. The results largely conformed to Gallup's predictions that chimpanzees, but not monkeys, ought to be able to attribute knowledge to the correct person. We remain cautious about inferring the complete absence of social attribution in rhesus monkeys, however, precisely because when we tested children on the same task, young 3-year-olds (who are quite capable of self-recognition) proved unable to distinguish between the person who saw candy hidden and the person who did not. Thus, although our research (as well as C & S's) may point to important differences between chimpanzees and monkeys, at present it argues more for the complexity of the chimpanzee's theory of mind than for the monkey's complete absence of one.

Although we have counseled that we must first understand the ontogeny of attribution in children before we can understand it in other species, we see a future in which the reverse is true as well. When studies of attribution in apes and monkeys become as disciplined as those with children, we may finally be at the threshold of answering critical questions about the evolution of attribution in our prelinguistic ancestors.

How do monkeys remember the world?

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I read Cheney & Seyfarth's (C & S's) book searching for information from monkeys' natural behaviour about their memory systems. Human memory is of particular interest to the clinical psychologist because it is profoundly disturbed in the neurodegenerative dementias, and memory loss is one of the most incapacitating consequences of brain trauma and encephalitis. Clinical psychologists look to the laboratory-based neuropsychologists to provide experimental support for the classification and subsequent neural localization of memory systems. But to study the neural substrates of memory in animals from a clinical perspective it must be shown that the psychological tests used in animals are drawing on the same components of memory as are affected in amnesia patients (Ridley & Baker 1991). Even using essentially the same tests in animals and patients may not suffice because a different mental strategy may be used by different species – for example, whereas humans may use verbal rehearsal animals presumably do not. It is in the realm of cognition (which uses data-based memory) compared to conditioning (which modifies habit-based performance) that the monkey becomes more important than the rat in psychological experiments; it is these higher order mental processes that are most relevant to human dementia and amnesia.

In assessing the type of memory a monkey or human is using a distinction must first be made between procedural skill and declarative knowledge (Cohen & Squire 1980). A procedural skill (e.g., climbing trees) is demonstrable only by the quality of an action and implies the existence in the nervous system of facilitated pathways in which activity leads to a relatively invariant response. (Climbing trees has to be learnt but it does not require memory). Declarative memory implies the existence in the brain of a data-based storage or memory system from which novel responses can be generated in different circumstances. Various examples are cited in C & S's book that imply the existence of declarative memory in vervets. For example, social responses by one monkey may be determined by the relationship between two other monkeys in a triad. This behaviour is therefore not an invariant response to either of the other two animals.

Declarative memory is next subdivided into semantic knowledge (mental representations of things [e.g., snakes are nasty] and abstract things, relationships rules, propositions etc. [e.g., avoid A who is friendly with B]) and episodic memory (memory of the occurrence of events [e.g., I saw a snake this morning; her sister bit me yesterday]; Squire 1986). [See also multiple book review of Tulving's *Elements of Episodic Memory BBS* 7(2) 1984.] This distinction is important because amnesic patients primarily lack the ability to form new episodic memories whereas semantic knowledge may be largely spared. C & S's book provides strong evidence that monkeys inhabit a declarative and semantic world. When an isolated infant vervet emits a distress call its mother will look toward it whereas the other mothers will look toward the mother. When an aggressive exchange takes place between two vervets there may be further aggressive acts between the relatives of the two aggressors. These and similar observations of social behaviour demonstrate that the relationships between monkeys in a group are facts that the monkeys understand and that they use both to guide their own behaviour and to predict the behaviour of others (e.g., when they avoid the kin of the aggressed). This supports our view (Ridley & Baker 1991) that the majority of discrimination tests carried out in the Wisconsin General Test Apparatus in the laboratory require the monkey to use semantic knowledge although it has been claimed that these tests merely assess habit formation (Mishkin et al. 1984).

The demonstration of episodic memory in monkeys is more difficult both in the field and in the laboratory but it is the most important form of memory from the clinical perspective. C & S claim that vervets' friendships and alliances are determined by memory of previous events, but this is not convincing. Each monkey may acquire a "credit-rating" that depends on previous alliances without the events of those alliances being remembered. Many supposed tests of amnesia in monkeys (Mishkin et al. 1982) test "recognition" of an object seen only once before. But monkeys may succeed on these tests because the object acquires the quality of familiarity rather than because the monkey remembers the event of its previous presentation.

For a monkey to recollect an event it must be able to distinguish between the events of the present and the events of the past. This may require an animal to have access to the content of its own mind. But are nonhuman primates aware of the contents of their own minds and the minds of others? These questions are not yet answered but the accumulation of anecdotal evidence especially involving deception and the withholding of information suggests that chimpanzees are sometimes aware of the content of the minds of others. Chimpanzees' ability to interact with their own reflection in a mirror, and language-trained chimpanzees' ability to use symbols about their own feelings and desires implies self-awareness in apes. But the evidence for a similar self-awareness in monkeys is inconclusive. They clearly have some self-awareness because they know their own dominance relative to all the other monkeys in a group. In addition, neurosurgical studies in the laboratory suggest a dissociation between "personal memory," that is, memory for one's own responses and memory for external events (Gaffan 1987).

Finally C & S point out that monkeys' cognitive skills may be far superior in the within-species social domain than in the world of objects and other animals. This may well be true, but where monkeys do succeed in object-based tasks in the laboratory it would seem reasonable to assume that they are using their most sophisticated mental processes even though these may have evolved in a social context.

It is to be hoped that ethologists and neuropsychologists will form closer alliances in the future. This is necessary because our ability to understand the mental processes of other species is as surprisingly poor as is monkeys' apparent inability to acquire and use the same level of mental representation of relations in the inanimate world as of relationships amongst their own species.

Knowing thyself, knowing the other: They're not the same

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In this commentary we address Cheney & Seyfarth's (C & S's) claim that monkeys do not know what they know, and cannot access or examine their own mental states. The evidence for this conclusion is quite indirect and tenuous, as the authors admit (p.

240). C & S focus on monkeys' apparent failure to monitor the thoughts and motives of others. They then take this to imply that monkeys do not monitor their own thoughts and motives. Thus C & S seem to suggest that the absence of other-awareness indicates the absence of self-awareness.

Our first concern about this conclusion is empirical. We believe our own research shows that monkeys can easily and adaptively monitor and exploit their own mental states of doubt and uncertainty (Smith et al., in preparation). In our paradigm, animals receive discrimination trials which range from very easy to impossibly hard, together with food rewards or long time-outs for correct and incorrect responses, respectively. The twist in our experiment is that animals can also use a "bail-out" response which lets them escape, at some cost, into easier guaranteed-win trials. Both of the rhesus monkeys we have tested bail out sparingly and selectively on only the most difficult trials. Various analyses and arguments converge on the conclusion that their bail-out responses reflect metacognitive reactions to subjective uncertainty rather than lower-level reactions to (e.g.) objective stimuli or conflicted behaviors. That is, monkeys bail out when they *know they do not know* the solution to a trial. Thus a direct assessment of monkey self-awareness yields positive evidence. Although it could be that rhesus monkeys have more access to their own mental states than do vervet monkeys or that our task creates the very metacognitive capacity it measures we believe that wild vervets living in complex biosocial environments have an equivalent need to access their own mental states and will demonstrate an equivalent capacity to do so when "asked" directly if they can.

Our second concern is theoretical. C & S almost conflate the concept of other-awareness with that of self-awareness. For example, in summarizing their chapter on attribution they pass imperceptibly from the "hypothesis that monkeys are unable to attribute mental states to others" (p. 253) to the "view that monkeys do not know what they know, and cannot reflect upon their knowledge, their emotions or their beliefs." (p. 254) For C & S, having a theory of mind apparently means having both kinds of awareness. This assumption may reflect an implicit theory – that self-awareness is the necessary and sufficient condition for other-awareness. This theory would justify the authors' belief that self-awareness is a measure of other-awareness (p. 240), and their belief that the absence of other-awareness indicates the absence of self-awareness.

We question this theory. Self-awareness could be easier to master than other-awareness, and it might emerge earlier (phylogenetically or ontogenetically). Moreover, self-awareness could be adaptive even without other-awareness and therefore it might exist by itself in some species. These possibilities cannot even be explored until these two modes of metacognition, these two theories of mind, are clearly distinguished.

Nonetheless, we do agree with Cheney, Seyfarth, Humphrey (1976) and others that self-awareness (when present) might support or encourage other-awareness. Furthermore, our own laboratory results suggest that monkeys *are* self-aware. In that case, they may also be well-prepared to understand each other's mental states or social behaviors. Indeed, this notion could explain C & S's observation that their monkeys' cognitions about the social world are much more sophisticated than their cognitions about the natural world. It may be that monkeys bring special skills (introspection) to the social arena. They may get 'inside information' about things social from insight into their own thoughts and feelings.

Following this line, one is led to rethink the monkeys' seeming failures of other-awareness. One possibility is that monkeys are other-aware, but clumsy at carrying through the Machiavellian strategies suggested by that awareness. [See also Whiten & Byrne: "Tactical Deception in Primates" *BBS* 11(2) 1988.] And, as C & S admit, there are alternative explanations for many apparent vervet ineptitudes. For example, vervets make snake calls whether or not other monkeys already know about the snake. On the one hand, this might suggest a failure to take the other's state of knowledge into account. But on the other hand, a redundant snake call does no harm, because monkeys and people can forget or be careless. We suspect that Cheney would make a redundant snake call to Seyfarth under similar circumstances, yet we know that she is both other-aware and self-aware. The point is that monkeys too may have other-awareness *competence* beyond their other-awareness *performance*.

Thus, we do feel that the distinction between other and self-awareness must be sharpened. Nonetheless, we applaud this extraordinary volume for its skillful blend of psychological analysis and behavioral observation. Cheney & Seyfarth have clearly advanced the discussion of animal awareness and animals' theories of mind. Some important next steps will be to distinguish more clearly other- and self-awareness to gather direct evidence for each and then to explore their empirical and theoretical relationships.

The sounds of silence

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Cheney & Seyfarth's (C & S's) book is a remarkable achievement. Few scientists have studied a nonhuman species as thoroughly as they have. There are a limited number of species for which we have such details of social and mental life. No other scientists have refined the playback technique so carefully nor applied it with such creativity. Often there is a tension between field and laboratory workers, with field workers decrying the "unnaturalness" of captive environments and laboratory workers decrying the lack of experimental rigor and control of field studies. C & S have adapted experimental techniques to field research in a way that few other scientists have. Of these other scientists none have worked with nonhuman primates. The authors have been extremely careful in their experimental designs and extremely cautious in drawing conclusions about what is in the minds of vervet monkeys. It is accordingly quite difficult to be critical of the book. I raise four points that arose in my reading, however.

First, I am concerned that silence is used as support for the existence of deceptive communication. I think the argument is based on the assumption that alarm calls or food calls must be strictly referential. If an animal should always call when a potential predator or food is present then the failure to call might be interpreted as deception. However, if the call has an affective component as well or does not function as a scientist suspects it ought to, then failure to call may be an index of lack of interest rather than deception. For example, chimpanzees call infrequently when given five prunes and call more frequently when given 20 prunes (Hauser & Wrangham 1987). Failure to call when given five prunes may simply indicate that five prunes are not worth commenting about, not that deception has occurred. Recently, Elowson et al. (in press) found that cotton-top tamarins have distinctive calls that are associated with food. The tamarins were also tested for their preferences for different foods and then call rate was correlated with food type. For eight of nine monkeys there were positive correlations with individual preference and call rate. Thus, calling is not deceptive and for tamarins, at least, failure to call indicates a low preference for a food. Marler et al. (1986) also report a correlation of food-call rate with food preference for chickens.

Second, the playback technique is quite powerful for demonstrating that an animal can respond to the information encoded in a signal in the absence of any other contextual features. Thus, we can use the technique to determine which acoustic features in a call are critical for monkeys or birds to classify a call as to whether or not it is from a member of their own species or a familiar individual. We can use the technique to determine whether animals classify their call types the same way human observers have classified them, to understand how animals perceive their own vocalizations, to determine an animal's expectations about its neighbors (such as playing back a neighbor's call from the neighbor's normal location or from a different location), to determine whether animals can habituate to calls from an unreliable signaller (and therefore whether animals can detect deceptive communicators), and to determine how a perceiver represents the semantic aspects of different calls.

The playback technique, however, can only tell us about the perceptual and cognitive processes of the listener and not those of the communicator. As human observers we are able to make many inferences from the communication patterns of infants and even other adults without the content of those patterns being explicitly semantic or linguistic. For example, cries or shrieks can be associated with certain contexts for certain individuals; so a cry from an infant or a shriek from a colleague next door can lead to fairly predictable inferences about what has just happened, even though no words are exchanged. We also know that young children at a given age understand language at a more complex level than they can emit language. Thus, the ability of animals to form inferences from playback studies that appear to be semantic or referential does not mean that the communicator is able to produce utterances at the same cognitive level. The playback studies with vervet monkeys tell us much about the cognition of perceivers, but they may not tell us as much about the complexity of what is being communicated. Either we will need to develop some new experimental techniques to evaluate what a calling monkey is intending or we need to accept C & S's view that monkeys may not be able to communicate in the strongest semantic sense of trying to modify the mental state of another animal.

Third, it is extremely puzzling with predation being an important pressure affecting their survival and with their otherwise high levels of cognitive functioning that vervets should be so obtuse concerning secondary characteristics of predators. Why don't they avoid a bush that has a python track leading to it? Why don't they express fear when they see a gazelle stashed in a crotch of a tree? Sand wasps can learn subtle features around their nest when they exit and use these cues to find their nests again (Tinbergen & Kruyt 1938). White rats can readily learn to associate a light or a tone with an aversive event and avoid that event when the signal is present. Why can't vervets learn the same types of signals in the real world where the consequences of not attending to such signals can be fatal?

Finally, why haven't vervets and other monkeys evolved teaching techniques to assist in the survival of their young? Achieving optimal reproductive success should place demands on the evolution of the teaching of young animals. Monkeys can learn to be fearful of predators through observing another monkey behave fearfully or calling to a predator. If the ability to teach is predicated on an understanding of the knowledge base of another animal and if even our closest relatives do not understand enough about mind to teach their offspring then there is truly a fundamental gap between human and nonhuman animals that will not be bridged by demonstrations of self-recognition, self-awareness, or even teaching analogues of language. Cheney & Seyfarth, who sought initially to demonstrate the parallels between humans and other primates, have concluded with a profoundly conservative view of the vast gap between humans and other animals.

Cognitive ethology comes of age

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How monkeys see the world is not meant to be a comprehensive review of cognitive ethology but it is a very thorough selective review, and marks, I believe, a turning point in the field. Most of the early proponents of cognitive ethology were so concerned with establishing the legitimacy of animal cognition as a field of study that they appeared to be cheerleaders rather than hard-headed scientists (e.g., Griffin 1976). Certainly, working to establish legitimate phenomena for the field was a reasonable endeavor at the time, especially in the context of a hostile behavioristic psychology and a traditional ethology not concerned with cognitive or other proximate issues. But the challenge is now to move beyond the simplistic question of "do they or don't they?" and Cheney & Seyfarth's (C & S's) book provides an interesting proposal for how to begin to do this.

One of the most important indications that cognitive ethology is coming of age is the concern with what animals cannot do, a negativity that is only possible in a field secure in its foundations. In the classic tradition of comparative psychology, C & S compare the cognitive abilities of vervet monkeys to those of other species, including humans, so that monkeys' limitations as well as their competences are investigated (cf. the similar strategy of Tomasello, 1990, for chimpanzee social learning). Thus, based on a comparison of a variety of primate species including humans, one of major conclusions of the book – and one that is likely to surprise people who only know C & S's earlier writings unabashedly boosting monkey abilities – is that "monkeys cannot communicate with an intent to modify the mental states of others because, lacking attribution, they do not recognize that such states exist" (p. 310). This is not to detract from the very complex picture of sophisticated monkey communication that has been developing over the past 15 years; it is simply to try to take the picture from all angles.

C & S argue that monkeys have a "laser beam intelligence." Despite recent arguments for the modularity of human intelligence, it is all relative, they argue, and domains of monkey intelligence do not seem to communicate with one another (are not "accessible" to one another) in the same way as do those of humans. C & S invoke this argument in particular in discussing why monkeys who make many complex inferences about social relationships fail to make seemingly simpler inferences in nonsocial domains, for example, in failing to use secondary cues of predator presence (dead carcasses, snake tracks, etc.) to enhance their very immediate chances of survival. In addition, in the nonsocial domain, C & S distinguish visual from auditory cues and provide evidence that only the latter are used by vervets in making inferences about the presence of predator species. They argue that this is further evidence of the modularity of monkey intelligence.

I, for one, do not find C & S's arguments for modularity convincing. Vervet monkeys do make complex inferences about predator presence, comparable to their social inferences, from auditory cues. And very different learning conditions obtain in the visual modality. For example, the sound of cow bells is almost always associated with humans and often precedes their presence – supporting, perhaps, some type of causal inference. The visual detection of gazelle carcasses is only sometimes associated with leopards; and when it is, there is no canonical temporal ordering pattern, making causal inferences more problematic. C & S address these issues, but I remain unconvinced that the differential use of visual and auditory cues of predatory presence results from anything other than differences in general learning conditions (or the causal inferences that may result). I should add that this modality difference in making nonsocial inferences may also be a case of a behavioral phenomenon peculiar to vervet monkeys from which C & S have overgeneralized; it certainly does not hold in many other primate species.

Another problem with the modularity thesis is in the social domain itself. The various social-cognitive skills of vervet monkeys all show similar abilities and disabilities. Thus, from their very interesting review C & S conclude that vervet monkeys do not show second-order intentionality in their communication, deception, empathy, instruction, imitation, social relationships, cooperation, and self-awareness. (In cases in which the distinction is meaningful, they do show first-order intentionality.) This would seem to imply a cross-domain application of concepts of intentionality, and this is so even across tasks that are primarily auditory (e.g., communication) and those that are primarily visual (e.g., imitation). The overall problem concerns what should be considered distinct domains. Perhaps all of these tasks are best considered parts of a single social intelligence. Who knows? I know of no criteria, either in this book or elsewhere, that provide satisfying ways for determining what should be considered discrete domains of intelligence once we focus our attention on animals with a cortex. Although there is no doubt that specially adapted, domain-specific learning devices must play an important role in any theory of behavioral ecology or comparative psychology or cognitive ethology, my own suspicion, and that of many others, is that such devices are much less prevalent and important in primates than in most other orders of animals.

The proof of any new scientific paradigm is in the eating – the scientific discoveries and theoretical advances to which it leads. Cognitive ethology is a kind of controlled anthropomorphism, and, as C & S point out, "anthropomorphism works." It has led us to discover all kinds of animal competences simply not discoverable by looking through the myopic lenses of behavioristic psychology or traditional ethology. The truly exciting thing is that this approach to animal cognition, along with judicious use of the comparative method, is now yielding fruits in the reverse direction as well. For example, Premack's investigations of the chimpanzee Sarah's "theory of mind" were begun, almost certainly, by anthropomorphizing Sarah to some extent. [See Premack: "Does the Chimpanzee have a Theory of Mind?" *BBS* 1(4) 1978.] The resulting findings stimulated, in turn, what is arguably the most exciting area of human developmental research today, children's "theories of mind." This human work is now informing the work of such other primatologists as C & S, as evidenced most especially by their Chapter 8 on attribution. In general, much of what has been discovered about the centrality of social cognition in the evolution of primate intelligence – note especially deWaal's (1982) study of chimpanzee politics – derives at least in part from viewing nonhuman primates in human terms (see also the papers in Parker & Gibson 1990). And all of this will, in the very near future I predict (and hope), reverberate back on the study of human cognition, mired for so long in the totally noncomparative and asocial metaphor of the computing machine.

C & S's book, by their own admission, has many gaps and speculations. But it is an overall success precisely because it goes beyond the available data. The authors do an excellent job of revealing many of the exciting possibilities that open up when primate social cognition is viewed in comparative perspective. Comparisons to nonprimate species would reveal, I believe, that Cheney & Seyfarth have overemphasized to some degree the modularity theme. Overall, however, the book may be seen as an important first step in a balanced view of how primates see the world, and, one hopes, in bringing about a truly comparative cognitive psychology in which social intelligence plays a major role.

Is lack of understanding of cause-effect relationships a suitable basis for interpreting monkeys' failures in attribution?

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This is indeed a very stimulating book! It presents a large body of evidence differentiating monkeys from apes in their abilities to attribute states of mind to others. My attempt here is to present the hypothesis that monkeys' lack of understanding of cause-effect relationships is a factor that can account for their inability to take into account other conspecifics' state of mind, as well as to comprehend such other phenomena as tool use and imitation. I accordingly try first to clarify (to the extent this is possible) what a cause-effect relationship is and then to validate my approach by examining a few examples reported in Cheney & Seyfarth's (C & S's) critical review, especially the last chapter, which provides excellent material to work on.

C & S state that "many of the results we describe could result from associative learning; they might also involve a more complex process that includes an understanding of causal relations between different events" (p. 272). In fact, C & S sometimes do suggest the importance of the understanding of causal relationships for the cognitive skills they consider (e.g., p. 289), but they do not discuss this point thoroughly.

The study of perceptual illusions prompted Leslie (1988) to argue that early in life we have a "perceptual mechanism operating automatically and incorrigibly upon the spatio-temporal properties of events yet producing abstract descriptions of their causal structure" (p. 187). His experiments have shown that six-month-old infants do not simply perceive spatiotemporal properties; they appear instead to identify causal relationships (Leslie 1986; Leslie & Keeble 1987). Such an understanding can promote particular ways children will subsequently draw inferences and organize their knowledge (see also Bower 1989). Because of this, children can greatly improve the kinds of reasoning they adopt for dealing with life.

Experiments similar to those performed with infants will be more than welcomed in primatology, especially because we do have scattered evidence that monkeys do not seem to use a cause-effect framework to represent their knowledge about their physical and social world (whereas apes probably do, at least to some extent).

What is a cause-effect relationship between A and B? How is it different from an association between them? Events in space and time can be organized as associations of two or more elements or as cause-effect relationships. In the latter case, two requirements must be fulfilled: A must be a *sufficient* condition for B to occur (not a *necessary* one, because B can occur for other causes as well), as in the logical relation of "material implication" $A \supset B$. Usually some kind of proportionality is also expected: B should be stronger or more evident if A is strong and evident. Knowledge deriving from specific associations is often not sufficient for behaving adequately when new conditions are presented. In contrast, an analysis in terms of a cause-effect relationship that incorporates the available information and suggests a temporal and causal relation between elements allows us to formulate possible explanations when new conditions are presented. On this basis it is then possible to draw inferences, to incorporate new elements, and to test, practically or mentally, the relative plausibility of different cause-effect interpretations. The comprehension of cause-effect relationships allows one to connect events and to produce further hypotheses about how things might be affected by the same or similar causes. Hypothetical results can be compared with those produced by practical experimentation.

The attribution of mental states to conspecifics is clearly advantageous. Attribution requires that an individual recognize that others' mental states can be different from his own, and that what the other individual thinks can have an effect on behavior (C & S, p. 249). I would add to this that attribution also requires the understanding that what another individual "thinks" is in turn caused by specific external/internal factors. Given this consideration, let us then examine whether monkeys' lack of success in attribution can indeed be accounted for by a lack of causal understanding.

To know what a conspecific thinks, believes, feels (empathy), or fails to master (thus requiring additional teaching), it is necessary to have had, or to have witnessed, similar experiences and to be capable of evaluating the effects of external/internal circumstances (or other factors) on the conspecific. For example, to feel empathy toward a mother whose infant died, it is necessary to know (or imagine) the feelings caused by such a loss. I suggest that in addition to the other requirements currently emphasized by primatologists as *conditio sine qua non* for mind reading (e.g., Whiten 1991a) and imitation (Whiten & Byrne 1991; see also Whiten & Byrne "Tactical Deception in Primates" *BBS* 11(2) 1988), mind reading and imitation also require the ability to draw inferences about the basis of cause-effect relationships. For mind reading, associations would be too time consuming, too unflexible, or impossible for rare events, and not rapidly adjustable to new situations overall. For example, deception requires an understanding of how knowledge and lack of knowledge are caused. Similarly, to teach something to somebody it is necessary to identify the cause of the flaw in that individual's performance and to selectively teach and draw attention to the erroneous step (see Boesch 1991).

Analogously, the failures reported for "naturalist monkeys" (p. 277) can be accounted for by this framework. The reports (Chapter 9) that baboons or vervets are not alarmed when they see a carcass of an animal recently killed by a leopard or the track left by a snake suggest that the monkeys have not previously associated carcasses with the presence of leopards, or snakes with tracks, nor can they infer from the carcass that a cat has recently been in that area or that a snake has just passed by. A causal approach to knowledge of events would have allowed the monkeys, on the basis of elements of their past experience, to infer the causal origin of a carcass. By the way, what is really a predator for a monkey? It seems that a predator means much more to us than to the animal itself: Thorough knowledge of a predator involves its behaviors, its leftovers, the likelihood of encountering it in different habitats, and so on. In contrast, for a monkey the concept of a predator seems to be much more restricted and cognitively simple.

Our experiments on tool-using skills and imitation in capuchins (*Cebus apella*) also suggest that when these monkeys use tools successfully, they have little or no comprehension of the cause-effect relationships involved in using them (Visalberghi & Limongelli in preparation; Visalberghi & Trinca 1989). The finding that capuchins' performance is full of errors, which do not disappear through time, can also be accounted for by a lack of understanding of what is wrong, that is, of the factors that cause errors. Similarly, it can be argued that capuchins consistently fail to imitate proficient models in tool-using tasks because they do not understand the cause-effect relationship linking objects, action, and outcome (see also Koehler 1925; Piaget 1945; Visalberghi, in press; Visalberghi & Fragaszy). In this sense, mind reading, tool use, and imitation *sensu strictu*, all seem to depend on causal understanding.

An understanding of cause-effect relationships allows one to identify the possibly relevant aspects of a phenomenon and to select the potentially important elements within the wide range of the existing ones, such that future experiences can confirm or disconfirm the hypothesis. Whenever a phenomenon is understood at a further level, its functioning is clarified so that several possibilities can be discarded beforehand. The understanding of a phenomenon produces explanations of its functioning on the basis of which further predictions can be made.

In short, apes (or at least a language-trained chimpanzee such as Sarah) are able to infer the solution of a problem they have never solved before (Premack 1988b) and to understand the cause of its solution. For example, Premack has demonstrated that Sarah could answer such questions as: "What caused this?" In addition, there is evidence that apes have a theory of mind (see Whiten 1991a) and that they are capable of deception (Woodruff & Premack 1979). Recent experiments have shown that chimpanzees are capable of modeling the visual perspectives of others (Povinelli et al. 1990) and that they know the relationship between seeing and knowing. The chimpanzees are thus able to infer the state of knowledge of another individual resulting from seeing and not seeing. Povinelli et al. (1990) argue that there can be no other basis for making such inferences about the other's state of knowledge without an understanding of the relevant causal relationship.

As C & S put it, "our inclination is to accumulate information about the world that is not directly relevant to the getting and spending of daily life" (p. 277). In addition, humans seem able to adopt causal relationships to organize such information. This opens up countless possibilities, for further inferences, predictions, and interpretations about how things happen, work, and are affected by a given set of elements. In contrast, whenever there are two new elements a new association needs to be learnt, and only then can it be used on later occasions.

Mind reading, pretence and imitation in monkeys and apes

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How monkeys see the world offers an object lesson in how deep questions about other primates' minds can be addressed in an imaginative but rigorous empirical fashion (Whiten 1991b). I think there are two major reasons why it has so advanced our understanding.

First, the work has synthesised field observations of natural social interactions with controlled experiments, in both field and laboratory. Although excellent models for this approach have been available for some time (e.g., Krebs & Davies, 1990, for a review; Tinbergen 1951; 1959), their application to primates has been all too limited despite the pioneering attempts of Kummer (1971; 1981). Second, Cheney & Seyfarth (C & S) have devised ways of testing, usually for the first time, the operation in monkeys of a number of mental processes generally considered central to human social cognition: in particular, social knowledge, the representation of meaning, and attribution of mental states to others.

All this can hardly fail to raise many matters for discussion. Here I restrict myself to those that, as the authors say, "strike at the heart of how monkeys see the world" (p. 183) and thus occupy the longest chapter, on the attribution of mental states to others (mind reading).

C & S describe two series of experiments testing for (and interpreted as indicating a lack of) mental attribution. As the first attempts to do this for monkeys these are admirable, but there are difficulties of interpretation. In one experiment (pp. 218–23), mothers failed to make more attempts to inform infants about foods or potential "predators" when the infants were ignorant of these than when they already knew about them. However, as described, the experiment appeared to separate mother and infant with a steel partition, so the mother would herself be ignorant of whether or not the infant could see the critical events. In successful tests of the attribution of knowledge versus ignorance by chimpanzees (Povinelli et al. 1990; see also summary in Premack 1988a), the mind-reading subject could see directly whether the other individual could see the events.

C & S's second experiment, to be able to discriminate between ignorance and knowledge the subject was first required to understand the operation of one-way windows. Although subjects were familiarised with such windows, however, there is no check that they had understood their significance. Moreover, it would seem that for the experiment to work, just to understand the implications of the one-way mirror for the potential object of mind reading would itself require attribution of a sort that may be particularly sophisticated: an appreciation that another individual would suffer the same particular state of ignorance to which one had oneself earlier been subject when looking through the window one way, but not the other.

Despite this, the experiment produced several results predicted by the attribution hypothesis, as well as negative results. C & S are inclined to dismiss the positive results because they say they could have been produced by the subject discriminating behavioural rather than mental states: However, if this is the case, then one must ask what results the authors *would* have accepted. Was the experiment a valid test in the first place?

Therefore, it does not seem appropriate to dismiss mental attribution on the basis of these first experiments with monkeys. It may be, however, that C & S have aimed rather high in testing for attribution of *knowledge* in the first instance. In child development, there is evidence that attribution of such epistemic states does not occur in the earliest stages, but rather is preceded both by what Wellman (1991) calls a "simple desire psychology" in which others' *wants* are represented, and by some understanding of *seeing* (Flavell et al. 1981). Attribution of these "easier" states by monkeys might thus be more rewarding of investigation. Whiten and Byrne (1988a) found that most of their catalogue of primate tactical deception was concerned with the monitoring and manipulation of others' seeing and visual attention, and Whiten and Byrne (1988b) argued that episodes of partial hiding in monkeys (in which A hides a part of itself from B while still watching B) suggested the ability to take the visual perspective of another: But this has yet to be tested experimentally.

Cheney & Seyfarth and Whiten and Byrne (1991) are in agreement that (1) neither observational nor experimental studies to date have supported attribution in monkeys, whereas (2) on the basis of both observational and experimental evidence, "chimpanzees do indeed seem more skilled than monkeys at attributing states of mind to others" (p. 252); is the expression "*more* skilled" to imply that monkeys may mind-read a little? It would be interesting to know what the authors are thinking of here).

Whiten (1989b) and Whiten and Byrne (1991) have suggested that this apparent monkey/ape difference in mind reading may be part of a much larger mental pattern, which also finds parallels in developmental distinctions. Interestingly, the phenomena of *pretence* and *imitation* which we incorporate into this analysis are also addressed in C & S's chapter on attribution, but with different interpretations.

Whiten and Byrne (1991) extended Leslie's (1987) argument that an ability to represent representations (metarepresentation) is not only fundamental to developing a theory of mind, but is first expressed in children's development of pretend play. Leslie offers evidence for this both in the sequence of development in normal children and, perhaps more powerfully, the dual deficits in both theory of mind and pretend play in autistic children. Whiten and Byrne showed that, as predicted by this model, chimpanzees showed dual superiority in the evidence for both mindreading and pretend play, and we further suggested that imitation fits the same underlying monkey/ape difference in metarepresentational ability. Thus, what we offer is the prospect of a unifying explanation for a cluster of mental abilities, and for both developmental and phylogenetic changes in these.

C & S discuss much of the same evidence for pretend play, but instead they use it as one of several "measures" of mental attribution (others include deception and teaching, for example). They suggest that self-awareness is one measure of attribution and that pretend play is suggestive of self-recognition. Quoting F. Scott Fitzgerald ("The test of a first rate intelligence is the ability to hold two opposed ideas in mind and still retain the ability to function" [p. 205]), they suggest that the mind reading of an ape falls short of attributing mental states *different* from its own, and that equally, pretend play does not require the handling of different mental states: "Although an individual who engages in pretend play must entertain multiple representations of an object or an event, these representations are *shared* by all of the game's participants. To recognise someone else's belief, on the other and, an individual must consider multiple representation that *differ* among those involved" (p. 244). It is certainly true that no ape has yet passed a test of false belief attribution (Premack 1988a; Premack & Dasser 1991). The point of Leslie's analysis of pretend play as metarepresentation, however, was that holding "two opposed ideas in mind" was essential. The child pretending a banana is a telephone cannot merely represent the object as a telephone: It is essential for the child's maintenance of a grasp on reality to preserve a *primary* representation of the object as banana, *together with a metarepresentation* of the banana as telephone. Chimpanzees playing quite complex games with imaginary objects (C & S, p. 206; Whiten & Byrne 1991) must equally have in mind a representation of the true absence of the object, together with a representation of its pretend presence and properties.

Turning to imitation as another possible measure of attribution, C & S suggest that "chimpanzees and other apes seem more adept than monkeys at learning to use tools through observation, possibly because they are more adept at imputing purpose to others" (p. 229). I agree about the apparent ape/monkey difference in imitation (Whiten 1989a; Whiten & Ham 1992). However, is this because apes mind-read others' purposes? This may explain some aspects of chimpanzee observational learning, such as the tendency to copy the results of others' actions rather than their precise form, as reported by Tomasello et al. (1987): However, it does not account for the ability to copy a range of actions with no apparent purpose, such as spinning on one leg and pulling faces, as tested by Hayes and Hayes (1952). Instead, Whiten and Byrne (1991) have suggested that the imitative capacity of chimpanzees may also be explained by an underlying metarepresentational facility. To imitate a complex action pattern requires, in effect, getting the behavioural programme out of the other's head and into one's own. This is not merely a matter of visual perspective taking in the sense of knowing what the other can or cannot see, but rather of being able to represent the actions as controlled from the other's perspective and translating this into one's own action programme.

Cheney & Seyfarth's achievements in this book lead to some optimism that such differing interpretations of the evidence will be subjected to further imaginative and careful testing.

Authors' Response

Characterizing the mind of another species

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We want to begin by thanking our reviewers for their many complimentary remarks and for their constructive criticism. If we are ever to gain an understanding of how another species thinks, the enterprise will have to be a joint venture, approached from many different perspectives. It will be a pleasure to work with colleagues whose thinking is so often less woolly than our own, even though the same issues seem to confuse us all. It is evident from the reviewers' sometimes conflicting advice and criticism that the goals and methods of our (and their) research have not yet been clearly delineated or defined. On the other hand, if they were, the pursuit would be much less fun. If, in this riposte, we occasionally point out the ways that reviewers contradict each other, this is not to belittle their comments but only to emphasize that similar goals need not preclude quite different approaches and attitudes.

1. The general approach. Is it scientifically respectable to talk about what monkeys might need to think about? Perhaps Nagel, Wittgenstein, and Quine (in addition to many others) are right to suggest that it is foolish hubris even to attempt to understand how another species (or even another human) thinks. Yet we believe that this "Panglossian paradigm" (Dennett 1983) is at the very least heuristically interesting, if only because it addresses intriguing problems and generates predictable hypotheses. It seems to us that one can learn little about the mind of another species if one does not at least attempt to tackle the issue from that species' own perspective, with some consideration of that species' functional needs.

Although our book purports to examine how monkeys think, we can in fact say little about the mental mechanisms (cognitive or otherwise) that underlie behavior. Our definitions and methods are operational. We rely exclusively on behavior to infer mental processes, and our approach is at best indirect. **Allen** correctly points out that we have fudged the issue of realism. This is partly because of our ignorance of philosophy, but it also seems to us true that talk about mental content – whether philosophically reasonable, logical, or not – is indeed indispensable to adaptive explanations of behavior.

Many reviewers in addition to **Allen** are aware of the dangers of attempting to chart a course between the Scylla of subjective mentalism and the Charybdis of behaviorism. Indeed, **Burghardt**'s remarks suggest that we may have to spend the rest of our careers removing the hurdles we have erected in our paths. There is little consensus among reviewers, however, about precisely which hurdle is most likely to bring us down. **Dittrich** cautions that the utilitarian approach to animal cognition may be doomed because we will never really be able to understand how another species thinks. **Figueredo** and **Glotzbach,** in contrast, think that an approach that attempts to determine what animals "need" to know may be helpful in explaining not only what abilities animals do have but also what abilities they manifestly do not have. We do not claim, as Dittrich thinks, that getting inside the mind of a monkey is a simple empirical problem. It is a truism that we will never understand completely what it is like to be a monkey, simply because we are not monkeys. But this logical fact need not bring on empirical paralysis.

For anyone who believes that it is logically impossible even to attempt to understand how another species thinks, the second part of our book's title is futile pretense. **Furth,** in contrast, has no problem with this part of the title; his quarrel is with the first. For Furth, our use of the word "see" presupposes that monkeys are in some sense able to reflect actively upon the world, rather than simply behaving in it. Because we conclude that monkeys do not know what they know, we can really only talk about sensory-motor know-how. Thus our use of the word "see," rather than being a heuristic term (our original intent), instead reveals our inadequate theory of knowledge.

It is clear from the reviewers' comments that the role of anthropomorphism in studies of animal cognition remains a matter of some debate. **Tomasello** believes that anthropomorphism can help scientists discover all kinds of competences in animals not previously uncovered by behaviorism or traditional ethology. **Snowdon** finds our comparisons of vervets' abilities with humans' and other animals' at least conceptually interesting. Why, for example, don't vervets teach their infants when it would seem adaptive to do so? Why don't they recognize python tracks when sand wasps are capable of recognizing the subtle visual cues that demarcate their nests? **Burghardt** agrees that critical anthropomorphism can be a valuable tool in designing experiments and generating predictions about behavior, but only *if* (and here he is not particularly reassuring) one can avoid its many pitfalls. **Povinelli & deBlois,** in contrast, believe that our work is flawed because we have not been anthropocentric enough. In their view, we should link our research with comparable work on children more explicitly.

Furth also believes that our work is deeply flawed, but for the opposite reason. For him, our adult-human-centrism prevents us from viewing behavior in terms of species-specific adaptations. Rather than being puzzled about such apparently maladaptive behavior as the failure to attend to a dummy leopard (actually no such result was ever mentioned in our book, but the example will suffice), we should be content to explain behavior as the result of species-specific, genetic predispositions to engage in particular sensory-motor activities. Furth believes we would be better off accepting species differences and asking questions about cognitive mechanisms as a function of these differences. This is certainly a reasonable long-term objective; it is worth noting, however, that to date there have been surprisingly few studies on issues like "intentional" communication, theories of mind, or domain-specific knowledge in group-living nonhuman primates. It is therefore inevitable that the behavior and communication of vervet monkeys will sometimes be compared with that of humans, but this does not imply a judgment that humans are better than monkeys.

Moreover, **Furth** does not say how it is possible to investigate species differences without at some point making at least implicit comparisons. How can we even ask whether monkeys, for example, recognize mental states in others if we do not already know that adult humans do? How can we assess the vocalizations of monkeys without some comparisons with those of birds and humans? How easy is it to recognize the absence of an ability in one species without first identifying its presence in another?

2. Social behavior. How do monkeys keep track of social relationships and past interactions? **Ridley** describes several means by which vervets might remember past interactions and argues that their memory is unlikely to be episodic. Rather than remembering past events, Ridley argues, monkeys may simply judge individuals and events as familiar and act accordingly.

None of our observations or experiments allow us to identify the mental processes that underlie behavior. Nevertheless, we wonder whether a vervet's memory of previous social interactions is based solely on familiarity. Consider, for example, the "reciprocal altruism" experiment described in Chapter 3 and discussed by **Gouzoules** (see below). In these experiments, recent affinitive interactions with a particular individual influenced whether or not a monkey would attend to that individual's solicitation for aid, but only if that individual was a nonrelative. If the solicitor was a close relative, subjects responded regardless of whether or not they had recently interacted. This selective attention suggests, at the very least, that monkeys remember past events involving particular classes of individuals quite differently. It is also difficult to apply the familiarity principle to the spite shown by chimpanzees toward individuals who have recently formed alliances against them. On the other hand, whether or not monkeys or even apes have conscious access to their own memory is indeed doubtful. If episodic memory requires such access, then perhaps a vervet's memory is primarily declarative, as **Ridley** suggests.

Our discussion of primate social relationships draws heavily on laboratory research on animal "concepts." We are therefore puzzled by **Dittrich**'s assertion that we are critical of such research. In fact, we discuss this research at some length on pp. 86–91, citing some of the same authors as Dittrich does. Although laboratory experiments do indeed sometimes lack ecological and social relevance, they can also be more successful than field observations in addressing the mechanisms that govern behavior. Clearly, research on animal cognition requires an integrated approach that makes use of both laboratory and field data. Because studies of animal cognition have tended to be strongly laboratory-oriented, however, we agree with **Harcourt** that it is important to emphasize how much can be learned from less precisely controlled but perhaps more functionally relevant field observations and experiments.

Our experiments and observations are far from complete. Many gaps and holes need to be filled, and many experiments need to be replicated and elaborated. **Gouzoules** points out one important missing control in our "reciprocal altruism" experiment. Let us add another. One conclusion of this experiment is that prior affinitive interactions affect the willingness of vervets to cooperate with nonrelatives. In contrast, vervets' willingness to aid kin appears to be unaffected by recent events. In fact, to document this kin effect more convincingly, we should have controlled for overall interaction rates. Generally speaking, monkeys interact more with kin than nonkin. As a result, our apparent kin effect could simply have occurred because two individuals who interact at high rates pay less attention to any particular recent event than two individuals who interact infrequently. Future tests of the reciprocal altruism hypothesis should control for this potential confound more explicitly by exploiting the variation in interaction rates that exists among kin and nonkin. If we could show that monkeys are more willing to attend to kin with whom they seldom interact than they are to unrelated "friends" with whom they interact at high rates, we could claim with more certainty that cooperation with kin is not dependent on reciprocity.

3. Communication. Any discussion of a topic as broad as communication will suffer from sins of omission; ours is no exception. Thus **Owings** is right to point out that first alarm calls may serve different functions from second and subsequent alarm calls. Similarly, the apparent "mistakes" that infants make when giving alarm calls may indeed be requests to recruit adult aid and guidance, and the silence of adults after infants' alarm-call at nonpredators may shape the infants' developing knowledge about predators. We only wish that Owings had provided some suggestions about how we might go about testing these hypotheses.

We also agree with **Dewsbury** that we need more old-fashioned ethological studies to investigate not just what stimuli (social or otherwise) cause calls to be given or modified, but more especially to determine what patterns of behavior are *not* changed by the presence of an audience. It is always more difficult and inconclusive to study negative results, but we will learn much about the production and function of signals by doing so.

We are puzzled by **Dewsbury**'s claims that we reject the arousal hypothesis as a possible explanation for the "audience effect." We were not aware of doing so. Indeed, as we state explicitly in several places in the book, our assessment of the meaning and function of vocalizations depends almost entirely on the responses these signals evoke from others. As a result, we can never state precisely what the motivational basis of a signal is. It may well be that signalers are simply more aroused when they are in the presence of their relatives than when they are alone or near unrelated animals, and that as a result they call more when kin are present.

The fact that we assess the meaning and function of vocalizations almost exclusively through responses, however, is a fundamental weakness of our observational and experimental methods, and we wish that **Snowdon** were less perspicacious in revealing this flaw. Because our definition of the meaning and function of calls is derived from listeners' responses, we can say little about the perceptual or cognitive processes underlying the production of calls. If we observe that a female fails to give an alarm call and that her rival is consequently eaten by a leopard, we can say that the female's silence *functioned* to deceive, but we cannot say that the signaler *intended* such deception.

Under natural conditions, the only way to measure the meaning of a call from the signaler's perspective is to elicit a call, and this proved very difficult with vervet monkeys. One can attempt to elicit calls by presenting subjects with either a visual stimulus (e.g., a stuffed python) or an auditory stimulus (e.g., a leopard's roar, a starling's alarm call). Until holograms become a routine part of field experimentation, most sorts of visual stimuli will remain impractical – how does one present a monkey with a stuffed version of a close relative or a higher-ranking female? Playbacks of auditory stimuli are more feasible, but in the case of vervet monkeys they failed to elicit vocal responses. Vervets are relatively quiet and seldom answer each other's calls. As a result, we could assess the meaning of calls only through the behavioral responses that our playback experiments evoked.

Many other primate species, however, are considerably more "conversational" than vervets. Baboons not only answer each other's calls at high rates but also regularly split up into foraging parties that maintain contact through calls and answering countercalls. In such species, presumably, signalers have the option of "choosing" to provide or withhold information to those who are out of sight and to answer other individuals' calls with acoustically similar or different calls. It seems possible, therefore, that playback experiments could successfully be used in other species to reveal the meaning of calls from the signaler's perspective.

Happé & Frith discuss some fascinating parallels between the communication of nonhuman primates and that of autistic humans. They point out that both monkeys and autistic humans fail to represent or manipulate other individuals' intentions. The communication of intention is a rich contextual component of human speech, and one that apparently makes speech far more flexible than the vocalizations of monkeys. The suggestion that monkey vocalizations have a semantic meaning that is relatively context-independent was initially controversial. Ironically, however, as Happé & Frith point out, this context-independence actually places severe restrictions on communication. Like autistic humans, monkeys appear to have a form of coded communication with set meanings that, although certainly capable of transmitting information, is relatively rigid.

Baron-Cohen makes the related point that most vervet calls appear to be imperative, in the sense that they are given with the intent of causing another individual to do something, like flee from a leopard. Children's utterances, in contrast, tend to be declarative comments about the world. Whereas monkey vocalizations function to influence behavior, children's comments appear to be given with the intent to influence attention. It is indeed difficult to think of any vervet calls that are declarative rather than imperative. The one that comes closest is the grunt that vervets give when they are about to move into a potentially dangerous area of their range, or when they are observing others do so. This "MIO" grunt appears to function to direct listeners' attention to the group progression, and can be interpreted to mean something like "Look! I'm (or Claude is) moving into a dangerous area. Watch out for me (or Claude)!" Nevertheless, because this call can also be viewed as a low intensity alerting or warning call, the line between declarative and imperative function is blurred.

If declarative comments do indeed suggest that the speaker is trying to affect the listener's mind, as **Baron-Cohen** suggests, we might expect to find more evidence of declarative comments in apes (or at least chimpanzees) than in monkeys. Unfortunately, there have as yet been no detailed studies of the vocalizations of any of the great apes. It is interesting to note, however, that in Greenfield & Savage-Rumbaugh's (1990) study of the spontaneous "sentences" generated by the language-trained bonobo Kanzi, most of Kanzi's messages appeared to be imperative rather than declarative. Although Kanzi combined many signs into novel two- and three-word sentences, the majority of these were imperatives, directed at causing the trainer to do something for Kanzi. The paucity of declarative sentences may reflect a genuine cognitive difference between bonobos and children. It may also, of course, simply reveal that a bonobo is not particularly motivated to make profound or interesting comments about life to a member of another species.

4. Attribution. The majority of commentators focused their remarks on our chapter devoted to theories of mind, thereby reinforcing **Furth**'s suspicion that the emphasis on self and mental state is the product of middle-class Western ideology.

It is clear from the reviewers' comments that there exists no single definition of a theory of mind, and that many different patterns of behavior will have to be examined before we understand the extent to which monkeys and apes attribute mental states to each other. **Whiten,** for example, cites Leslie's (1987) work on pretend play in children as one of the earliest manifestations of the child's developing ability to represent representations. In contrast, **Povinelli & deBlois** are not convinced that pretend play is anything more than evidence of the child's inability to grasp the representational aspects of language.

Similar problems arise in the reviewers' discussions of self-awareness. **Povinelli & deBlois** deride our definition of self-recognition (as opposed to self-awareness) as implying that "we would have to talk about certain species of flowering plants as being capable of self-recognition." But this is precisely the point. All animals – and even cells – recognize themselves to some degree. At what point, however, do individuals become able to reflect upon themselves, in a sense to interview themselves? We are sure that many reviewers will be as surprised as we were to learn that self-recognition is defined primarily in terms of one "specific behavior . . . (- that) of producing self-directed behavior in front of a mirror." Surely, just as there are many levels of self-recognition and self-awareness there must also be more than one criterion by which to measure the ways individuals in any species recognize themselves.

Other commentators are more willing than **Povinelli & deBlois** to discuss self-recognition outside the mirror context. **Armstrong** agrees with our suggestion that monkeys do not have "meta-self-awareness," as evidenced particularly in the apparent inability of monkeys to transfer a skill acquired in one context to another. Clearly, however, monkeys have some degree of self-recognition. **Ridley** agrees that monkeys' ability to recognize their own dominance rank relative to others requires some form of self-recognition, even if they do not have complete access to the contents of their own minds. Monkeys obviously have a form of self-recognition different from that of a flower or the immune system. But how should we characterize it?

As we mention in Chapter 8, even when self-recognition is defined solely in terms of mirror tests, results are ambiguous. On the one hand, monkeys do not seem to recognize their own reflections. On the other hand, they do not always treat mirror images as other monkeys and can learn to use mirrors to guide their own hands. If we restrict the definition of self-recognition to the ability to recognize one's reflection in a mirror, then clearly chimpanzees have self-recognition and monkeys don't. This stark dichotomy obscures some important information, however. Research on self-recognition will be severely constrained if we limit our definition to one narrow behavior, particularly one whose function is not at all clear.

This point is reinforced by **Schull & Smith,** who contend that monkeys have considerably more self-awareness than we (and those who define self-recognition in terms of mirror recognition) had previously believed. They cite an experiment in which captive rhesus macaques learn to "bail out" of discrimination trials that are too difficult for them. Such behavior, they argue, indicates that monkeys are able to monitor and exploit their own mental states of doubt and uncertainty.

We never argued that monkeys are unable to monitor their own abilities. Indeed, no doubt many animals in addition to primates regularly do so. Vervet monkeys assess not just their opponents' but also their own dominance ranks and fighting abilities before deciding whether or not to intervene in a dispute. Similarly, in many animal species (including many species of insects), competitive interactions between males are characterized by prolonged, ritualized displays that apparently function to allow opponents to assess each other's relative fighting abilities. If a rival's fighting ability seems greater than his own, a male will "bail out" before the contest escalates, just as rhesus macaques "bail out" of difficult discrimination tasks. Our point is not that monkeys and other animals are incapable of assessing their own abilities but that they may not be aware of making such assessments. The methodological and conceptual challenge is to determine some way to measure the extent to which a rhesus macaque (or any other animal) can actively reflect and report on his actions.

Povinelli & deBlois correctly argue that tests of attribution in nonhuman primates have much to learn methodologically from similar tests in children. We also believe, however, that we should be cautious in drawing too many parallels between children and nonhuman primates, if only because we are dealing with entirely different species that have evolved under different social and environmental selective pressures. Because children are not monkeys or apes, we can never be sure that a monkey or ape "fails" or "passes" a test of attribution for the same reason a child does. Precisely for this reason, it seems advisable to investigate nonhuman primates' theories of mind in as natural a setting as possible.

Laboratory tests with captive animals are valuable for many reasons, and research on the cognitive capacities of animals would be severely constrained if it relied exclusively on free-ranging populations. At the same time, however, we agree with **Harcourt** that laboratory studies should not ignore their subjects' natural history or be conducted in a functional vacuum. The tests conducted by Povinelli and his colleagues have suggested that chimpanzees, but not rhesus macaques, attribute knowledge and ignorance to human trainers (Povinelli et al. 1990). These experiments are potentially important, because they provide some of the first nonanecdotal evidence of chimpanzees' recognition that two individuals can simultaneously hold different beliefs. Povinelli's experiments are complicated, however, by the fact that they rely on human informants and depend on a surprisingly large number of training trials. Differences in the frequency with which chimpanzees chose knowledgeable humans over ignorant ones reached statistical significance only after at least 100 trials (for the most accomplished subjects). Even then, subjects continued to choose ignorant guessers approximately 30% of the time.

Circus trainers know that it is possible to train a pig to put coins into a piggy bank, but no one imagines that this behavior occurs in the absence of human training or has any function outside the circus context. Instead, the banking behavior reflects pigs' natural predilections for rooting and digging. Similarly, the fact that captive chimpanzees can be trained to distinguish between knowledgeable and ignorant humans suggests – but does not prove – that they might also be able to recognize ignorance and knowledge in their companions under less constrained conditions (although the fact that they continue to choose the ignorant trainer at such a high rate remains puzzling). The experiment tells us nothing, however, about why chimpanzees might need to assess mental states in others or the circumstances in which they use this ability under natural conditions.

From our own perspective, Povinelli's experiment would be far more illuminating if it dispensed entirely with human training and used other chimpanzees as informants. If untrained chimpanzees could be shown to recognize the difference between a knowledgeable and ignorant *conspecific* (as we suspect they could), we would be in a better position to begin to address some of the "critical questions about the evolution of attribution in our prelinguistic ancestors" that concern **Povinelli & deBlois.**

Boesch also argues that evidence of imitation, teaching, and empathy must be sought in contexts where such abilities are functionally relevant; he cites several examples of apparent teaching and empathy among chimpanzees in the Tai forest. Boesch's data have been painstakingly gathered and make a major contribution to the study of animal cognition. The examples he cites suggest that chimpanzees are far more adept than monkeys at monitoring the mental states of others. We wonder, however, whether the Tai chimpanzees are as different from Gombe chimpanzees as Boesch claims. For example, it is clear that the Tai chimpanzees recognize injuries and physical handicaps in others and tend to injured individuals in ways that monkeys do not. We cited several similar examples from Gombe in our book. Although chimpanzees respond to visual manifestations of disabilities, however, there is still no convincing evidence that they empathize with other individuals' *feelings*, especially those (like grief) that have no obvious, overt manifestation. There have as yet been no reports, for example, of chimpanzees comforting a mother whose infant has died.

Similarly, if the Tai chimpanzees really differ from other populations of chimpanzees in showing active pedagogy, why does teaching occur so infrequently? **Boesch's** explanation that teaching is rare because most skills can be learned through other methods is not very satisfactory; even when it is not absolutely essential, teaching can nevertheless be far more efficient than trial and error or observational learning. Despite hundreds of hours of observation, Boesch (1991) has recorded only two clear instances of teaching among the Tai chimpanzees (although he has observed many hundreds of instances in which mothers apparently stimulated and facilitated their infants' learning). Assuming that chimpanzee mothers really do compare their own knowledge with the knowledge of their offspring, is active teaching rare only because it is generally not essential to survival and reproduction? How do we test this hypothesis?

We agree with **Boesch** that we must not exaggerate the importance of teaching, informing, and imitation when considering how information is exchanged. As **Perloe** points out, even among humans, communication often occurs at the level of zero- and first-order intentionality. One implicit theme running through these commentaries, however, is that we must develop some means of measuring the significance of the *absence* of a behavior. This is not just silly metaphysics; it is not the same as asking why competing male butterflies have not evolved machine guns, or how many angels could dance on the head of a pin. Animals do many things that we think we can explain; they also do many things we can't explain and fail to do many things we think they ought to do. If we see an apparently maladaptive behavior, we should not conclude immediately that it is beyond the animals' sensory capacities to do otherwise, as **Furth** argues. Similarly, if we fail to see some expected pattern of behavior we should not be too quick to conclude that the behavior is not functionally relevant. Both these conclusions are likely to be correct, but we will miss a lot if we don't at least entertain other explanations.

Like **Boesch** and **Perloe, Mealey** questions whether a contingency-based strategy for interpreting other individuals' behavior is really less effective than one based on the analysis of other individuals' mental states. One simple answer to this question is that it depends on the strategy used by one's audience. Mealey correctly argues that even humans often use a contingency-based strategy when interacting with each other; she cites a hypothetical psychopath who, though lacking empathy, is able to manipulate the behavior of others. As a thought experiment, this example is instructive. It would have been more helpful, however, if Mealey had provided some real-life examples of psychopaths who lacked *any* knowledge of other individuals' thoughts, knowledge, or feelings and yet were able to manipulate other peoples' behavior in noncoercive ways.

Whiten agrees with our general conclusion that few studies to date have found convincing support for the attribution of mental states in monkeys. He also feels, however, that we have aimed rather high in defining attribution in terms of the recognition of knowledge. His argument is not just theoretical, but also empirical. He questions our interpretation of an experiment in which macaque mothers apparently failed to recognize ignorance in their offspring and therefore failed to alert them to food or danger. Because their offspring were hidden behind a steel partition, he argues, mothers could not know that they could not see the critical events. We would reply that because offspring were absent when the critical events occurred, any mother capable of doing so should have inferred that her offspring was ignorant, particularly because the offspring's subsequent behavior (e.g., approaching a dangerous area) indicated ignorance. In fact, however, neither absence during the event nor subsequent behavior cued the mothers to their offspring's ignorance. Just as in the mirror experiments mentioned by Whiten, it seems that if one is incapable of recognizing how knowledge is acquired, one may not be able to recognize knowledge or ignorance in others.

Whereas **Whiten** feels that we may have prematurely dismissed the possibility that monkeys attribute mental states different from their own to others, **Noble & Davidson** think that we have concluded the reverse. They infer from our use of the term "mental state" that we have concluded that monkeys attribute mental states to others. This is an alarming misconception because it suggests that our prose was so inscrutable that we conveyed precisely the opposite of what we intended. What we thought we said was this: Monkeys may indeed have mental states (although we are vague about what forms these mental states take), but they do not apparently recognize mental states in others or even in themselves. Apes (or at least chimpanzees) seem better than monkeys at recognizing ignorance and even false beliefs in others, but the data are inconclusive and sometimes contradictory. Our interpretation of Menzel's experiments depends neither on ESP nor the attribution of mental states (although both remain possible). Indeed, as we state on p. 238, "some aspect of the leader's *behavior* (our emphasis) allowed the rest of the group to recognize when the hidden item was aversive."

5. Domain-specific intelligence. It is a tautology that animals are adapted to perform some tasks more easily than others and that apparently stupid animals sometimes do remarkably intelligent things. Leaf-cutting ants, for example, cultivate fungus farms to feed themselves and their larvae. Leaves are cut from trees and "planted" in the nest to allow a particular form of fungus to grow on them. The growing fungus is carefully cultivated and any undesirable species of fungus that might begin to grow on the leaves is carefully weeded out (as described by Gould 1982). The ants behave as if they recognize that fungus is caused by keeping leaves in dark, damp places, and their careful cultivation of the fungus crop seems as purposeful and goal-directed as any human farmer's. Nevertheless, even though we can apply a purposeful, intentional vocabulary to leaf-cutting ants, it is extremely unlikely that the ants are aware of their goal-directed behavior, or that they understand the causal relation between decomposing leaves and growing fungus. Instead, the behavior is a relatively inflexible adaptation to a particular environmental selective pressure.

It is intuitively easy to explain leaf-cutting and fungus cultivation in terms of specialized neural structures adapted to perform in specific, narrow contexts. It is more difficult, however, to imagine that some of the apparently complex patterns of behavior manifested by nonhuman primates might also be confined to relatively specific contexts. Is it really possible that a baboon who uses transitive inference to assess companions' dominance ranks would be unable to assess the amount of food contained in different-sized containers?

The notion of domain-specific intelligence is unsatisfying from both a conceptual and empirical perspective. Indeed, so little research has been done on this topic that we cannot even state for certain that we are dealing with a real phenomenon. As **Tomasello** points out, it has proven difficult to define precisely what a domain is and what mechanisms underlie it. Is a domain a discrete perceptual system, as Fodor (1983; see also *BBS* multiple book review of Fodor: "*The Modularity of the Mind*" *BBS* 8(1) 1985) might argue? Are we talking about differences in sensitivity to particular stimuli, learning, memory, or some combination of all these factors? In addition, what, if anything, delineates one domain from another, and how much communication occurs between domains? We discuss these points to some extent in Chapter 9, but **Dugatkin & Clark** are right when they emphasize that we know as little about cognitive mechanisms within apparent domains as we do about the transfer of skills across domains.

Part of the difficulty results from the fact that we still know little about the neural structures underlying behavior and perception. Moreover, even though it may eventually prove possible to correlate some forms of behavior with specific neural structures, similar correlations for other patterns of behavior will no doubt remain more elusive. It is now possible, for example, to identify specific areas in the forebrains of birds that control song production and comprehension. We also know that there are specific areas in the left hemispheres of monkeys' brains that are important in the perception and production of species-specific calls. As yet, however, we cannot even begin to talk about specific areas in the brain that affect the ability to attend to social, as opposed to nonsocial, stimuli.

There are also methodological problems. First, as is true for other aspects of the research discussed here, we have no satisfactory means of dealing with negative results – the failure to perform a task or recognize a stimulus. Second, there have as yet been almost no attempts to present animals with logically similar problems that differ only in stimulus type. As a result, there is still very little evidence that any monkey species is really more adept at solving social problems than ecological ones.

These problems disturb many commentators. **Dewsbury** is concerned that the social intelligence hypothesis is so vague that, when combined with the principle of transfer, it can be used to explain almost any observation. **Cords** worries about the appropriateness of opposing a socially based view of intelligence with an ecologically based one, because food-finding occurs in conjunction with social competition. We make this same point in Chapter 9. Cords also emphasizes that we know very little about how monkeys classify and compare food resources. Experiments with birds, for example, have shown that animals are often very adept at assessing the relative value of two widely separated food patches, and they apparently compare food patches before deciding to abandon one patch for another. Comparable experiments with nonhuman primates simply have not been done.

Like **Cords, Dittrich** and **Galdikas** argue that the evolution of intelligence should not be viewed in terms of selection acting in a single, narrowly defined domain. In fact, we never make this claim as baldly as Dittrich implies. As we state on p. 295, "the argument about domain-specific intelligence does *not* aim to oppose one comprehensive ecological argument against an equally comprehensive social one" (original emphasis). Rather the hypothesis is that abilities manifested in one context may not always be generalized to others.

Implicit in much of the "social function of intelligence" hypothesis is the assumption that nonhuman primates have more complex social relationships and know more about their social companions than nonprimates do. This assumption, however, has never been tested. Many nonprimate animals live in complex social groups composed of both kin and nonkin. Some also manifest complexities of social structure that may be unequalled by any primate species. For example, like nonhuman primates, male dolphins form coalitions against rival males. Occasionally, however, two coalitions will also form an alliance against a third. Such coalitions between coalitions have not been observed in monkeys or apes (but see Abegglen, 1984, for one possible exception). **Harcourt**'s survey suggests that nonhuman primates may be the only animals that manipulate the probability of receiving help in the future when establishing friendly relationships with other group members. [See also Caporael et al.: "Selfishness Examined" *BBS* 12(4) 1989.] Although it is often assumed that monkeys (not to mention apes) are somehow more intelligent than other mammals, and although laboratory tests occasionally suggest that this intuition has some substance, it remains difficult to point out many examples from the natural behavior of monkeys that indicate greater social complexity. For this reason alone, such surveys as Harcourt's deserve further attention.

Of all the data and observations discussed in our book perhaps the most puzzling and least satisfying is the vervets' apparent failure to attend to the inanimate visual cues left by such predators as pythons and leopards. Many commentators offer their own explanations for our observations. None of these explanations is easily dismissed, if only because so little is known about how monkeys perceive their environment. Indeed, the lack of empirical data is what makes this avenue of research so frustrating, and so much fun.

Dugatkin & Clark suggest that we should not attempt to link the vervets' failure to recognize carcasses in trees with a comparable ability in the social domain. Because vervets also fail to attend to secondary visual cues in their social world, the question of accessibility is moot. Nevertheless, they do not attempt to explain why vervets might ignore important predator cues. **Cords** suggests that people, too, are often remarkably ignorant about apparently irrelevant aspects of their environment. She does not venture a guess, however, about whether humans are better than vervets at recognizing visual cues of danger. **Tomasello** is unconvinced that the apparent differential use of secondary auditory and visual cues results from anything other than differences in general learning conditions. He adds that the apparent modality difference in making nonsocial inferences may be peculiar to vervet monkeys, and not monkeys in general. **Gouzoules,** on the other hand, speculates that perhaps this difficulty is specific to Amboseli vervets, and not vervets in general.

Gouzoules cautions us about drawing too many conclusions about "the" monkey mind until we have investigated the cognitive capacities of monkey species whose behavior is quite different from that of vervets. He also suggests that vervets in other areas of Africa might be better than Amboseli vervets at recognizing the secondary cues of predators. He mentions vervets in the Cameroon, who recognize dogs as a secondary visual cue of the approach of human predators. But dogs are animate visual cues, and Amboseli vervets are also adept at recognizing such cues – they readily recognize, for example, that cows and donkeys signal the approach of Maasai tribesmen. Instead, Amboseli vervets appear not to recognize *inanimate* visual cues – dust and python tracks, for example. We simply do not know whether vervets in other areas of Africa are better than Amboseli vervets at recognizing inanimate visual cues.

How should we go about testing the modularity hypothesis? How can we determine which features of the environment are relevant to an animal and which are irrelevant? **Glotzbach** suggests that Gibson's ecological approach to perception may be valuable to these investigations, and no doubt he is right in suggesting that the environment cannot be defined without reference to perceivers and their intended actions. By so doing, we may be better able to identify the stimuli that are functionally relevant to the species under consideration.

Visalberghi argues that the apparent failure of monkeys both to attribute mental states to others and to recognize the visual cues left by predators may result from their inability to understand causality. Deception, for example, requires that an animal understand how behavior causes knowledge. Similarly, the recognition that a python track is dangerous requires some understanding of how a python track is caused. We think Visalberghi's point is important, and we agree that future research should focus more explicitly on the mechanisms underlying learning. If vervets acquire knowledge about each other and their environment primarily through associative learning rather than an understanding of causality, they may indeed find it difficult to learn that a python track means danger.

We will never be able to evaluate the domain-specific hypothesis until we are able to design logically similar experiments that differ only along stimulus dimensions. We agree with **Dugatkin & Clark** that such research should not be limited to nonhuman primates. Indeed, some of the best comparative work on domain-specific memory is currently being conducted on caching and noncaching birds. Clark's nutcrackers, for example, are better than pigeons at remembering the location of food caches, apparently for the simple reason that under natural conditions Clark's nutcrackers store food and pigeons do not (Balda & Kamil 1988; Balda et al. 1987). It would be interesting to determine whether such spatial memory is also context- or stimulus-specific. Are Clark's nutcrackers better than pigeons at remembering where their neighbors' nests are located? Such experiments would provide a crucial test of the domain-specific view of intelligence.

It remains for future research to determine whether the apparent failure of monkeys to attribute mental states to others, to recognize causality, and to attend to certain aspects of their environment are real. There is a huge amount of work to be done. Issues concerned with vocal communication, the attribution of knowledge, and domain-specific intelligence do remain vague, ill-defined, poorly developed, puzzling, and perhaps even incorrect. But this is also what makes the investigation of these issues so compelling.

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