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Tactical deception in primates

A. Whiten and R. W. Byrne
Psychological Laboratory, University of St. Andrews, St. Andrews, Fife KY16 9JU, Scotland

Abstract: Tactical deception occurs when an individual is able to use an "honest" act from his normal repertoire in a different context to mislead familiar individuals. Although primates have a reputation for social skill, most primate groups are so intimate that any deception is likely to be subtle and infrequent. Published records are sparse and often anecdotal. We have solicited new records from many primatologists and searched for repeating patterns. This has revealed several different forms of deceptive tactic, which we classify in terms of the function they perform. For each class, we sketch the features of another individual's state of mind that an individual acting with deceptive intent must be able to represent, thus acting as a "natural psychologist." Our analysis will sharpen attention to apparent taxonomic differences. Before these findings can be generalized, however, behavioral scientists must agree on some fundamental methodological and theoretical questions in the study of the evolution of social cognition.

Keywords: animal cognition; deception; evolution; intentionality; manipulation; mind; primates; representation; scientific method; social intelligence

1. The significance of tactical deception

Deception is currently a topic of significant theoretical interest in the behavioral sciences (Mitchell & Thompson 1986a; Trivers 1985). Such attention follows in the wake of the prediction that communication should be molded by natural selection so as to *manipulate* other individuals, rather than to serve the function traditionally attributed to it, of efficiently transmitting honest information (Dawkins & Krebs 1978). Thus, although the subject of deception may once have appeared a narrow one, its nature and distribution within the animal kingdom has now shifted to center stage in our attempts to understand social evolution. For those who are concerned not only with social evolution but with the evolution of mind, there are further implications of deceptive behavior to consider; notably, the utility of a capacity for "mind-reading" in any society where overt behavior belies deceptive intentions (Humphrey 1983; Krebs & Dawkins 1984).

The flexibility of response allowed by such a capacity is by no means characteristic of all deceptive phenomena. This is perhaps clearest in the best-known classes of natural deception – bodily camouflage and mimicry (Wickler 1968), where a display borrowed from another species or part of the environment becomes a permanent fixture. Even if we turn from morphological deception, which usually occurs between different species, to behavioral deception within a species, we find that the commonest cases reported are those where some individuals of one sex exhibit a fixed mimicry of the behavior of the other (e.g., Thornhill 1979; review in Weldon & Burghardt 1984). It was to contrast this with the flexibility of the deceptions we observed in baboons that we introduced the term "tactical deception" (Byrne & Whiten 1985). We defined the behaviors of interest as "acts from the normal repertoire of an individual, used at low frequency and in contexts different from those in which it uses the high frequency (honest) version of the act, such that another familiar individual is likely to misinterpret what the acts signify, to the advantage of the actor" (p. 672).

"Tactical" thus refers to the capacity to shift one part of the behavioral repertoire flexibly into a deceptive role, according to the context. One example we gave involved a baboon suddenly adopting the alert posture and horizon watching normally shown when an important entity like a neighboring group or predator has been spotted, although in this case no such entity existed. The baboon was being chased aggressively by another, who stopped to look for the focus of interest and as a result never resumed the chase.

We specified "familiar individuals" to emphasize that (again by contrast with most of the inter- and intraspecies cases already mentioned above) such acts are taking place in a community of mutually recognizable individuals which places severe constraints on the exercise of deceptive behavior (Cheney & Seyfarth 1985b; van Rhijn & Vodegel 1980). "Crying wolf" is likely to be recognized as such if repeated too often with the same dupe, and there may be other ramifications of such detection, such as the loss of help from others, which can be so important in primate society (e.g., Datta 1983; Nishida 1983; de Waal 1986a).

Primates might be thought to be well established as exponents of tactical deception, but their reputation for this particular type of social expertise has largely been promoted by just two representatives of the order: by chimpanzees, and by people who have studied them. This is not a flippant remark, because members of the latter group – such as Kohler (1925), van Lawick-Goodall (1971; 1986), Menzel (1974), Woodruff and Premack (1979), and de Waal (1982) – appear to share a common scientific attitude, perhaps best described as a lack of

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sympathy with "mindless" behaviorism, which the skeptic might suggest is at the root of chimpanzees' notoriety. Does chimpanzee research attract scientists who are keen to find "cleverness," or are chimpanzees really special?

We shall return to this question later, but we introduce it here to emphasize that a situation has developed in which it is not unusual for chimpanzee deception to be discussed in the literature, whereas, in our experience, deception in other primates is seriously discussed informally by primatologists but has seldom found its way into the respectability of scientific publications. In our earlier paper (Byrne & Whiten 1985) we suggested that this might be because deception of familiar individuals living in the intimacy of a primate group is likely to be subtle and restricted to relatively rare occurrences (see Moynihan 1982 and Quiatt 1984 for further analysis of such dynamics). Any one researcher thus collects only anecdotal data and is coy about trying to publish these. How can science come to grips with such phenomena?

Here we present one approach. Together with the definition of tactical deception given above, we have published our own records, following this with a questionnaire circulated to 115 primatologists selected mainly from membership lists of the International Primatological Society. We will not rehearse here the details of the questions asked (see Whiten & Byrne 1986) because for present purposes it is sufficient to know that we asked for records of behavior matching our definition and examples. Our intent was not to collect a "representative primate sample," for although this would be desirable it is not possible given the current state of observational opportunities across the primate order. Rather, we wished to explore the possibility that a small corpus of material from one study, when pooled with that from others, might start to reveal some overall primate pattern.

In Section 2 we describe the resulting picture of tactical deception in primates, discussing as appropriate the evidence required for ascription of deception in the first place, and evidence that actors are representing psychological attributes of other individuals involved. In Section 3 we consider the nature of the differences between primate species in the repertoire of deceptive action, the role of creative intelligence in producing such behavior, and how we should study such phenomena in the future.

2. The range and scope of tactical deception in primates

2.1. Admissibility of evidence

We can consider whether a proffered example is truly a case of deception from the point of view of either the actor or the recipient. If one individual is deceived by the behavior of another it does not necessarily follow that the actor's behavior was intended to achieve this, or that the deceptive behavior serves some biological function; an individual may be deceived, for example, by accident. To say that the behavior is deceptive from the *actor's* point of view will, however, imply that its "purpose" (either in the sense of its functional design, or its goal or intention) is to deceive (Thompson 1986).

We have received examples which are dubious on either count – that the behavior did not have a deceptive purpose, or that nobody was actually deceived – but we have included in the corpus all 75 records submitted as *candidate* instances of tactical deception. The aim is simply to throw the net widely and open-mindedly to begin with, if only to generate a discussion of the ambiguities that need to be resolved in order to build more refined databases in the future. For the moment, however, we will exclude cross-species deceptions that have long been known to occur in the artificial conditions of captivity (Hediger 1950); such data raise special problems of interpretation. On the other hand, we have included the few published records we have discovered that are interpreted by authors as instances of deception.

2.2. The players

There are only a limited number of "parts" that primates play in these sorts of social interaction. We find it helpful to label some of these. All examples necessarily involve at least an actor (the AGENT) whose behavior we are classifying, and at least one individual (the TARGET) who poses the problem that the AGENT's behavior appears designed to deal with. Things are more complicated, however, because another individual may be involved. The DUPE is the individual deceived by the AGENT, and all the examples in this article naturally include a DUPE; however, in some triadic interactions we shall see that the DUPE is not necessarily the TARGET; rather, the DUPE acts as a social TOOL whereby the AGENT manipulates the behavior of the TARGET. Finally, there is the case where a FALL GUY is the victim of the AGENT's manipulation of the TARGET.

2.3. Classifying the players' performances: A functional taxonomy

To understand what is happening in the variety of instances which make up our corpus of material, some form of classification is necessary. It will always be possible to classify such data in a number of ways (see Miles 1986; Mitchell 1986; Russow 1986; de Waal 1986a). We offer here a classification based on the functional consequences of the deceptive act concerned – what the AGENT can achieve by influencing the psychology and behavior of the TARGET and others. Thirteen subclasses of deception distinguished in this way are grouped within the five major functional classes of *concealment, distraction, creating an image, manipulation of target using social tool,* and *deflection of target to fall guy*. Each example quoted in illustration is annotated with author, species, and its reference number in the complete catalogue of records collated up to 1985 (Whiten & Byrne 1986).

2.4. Classifying states of mind: A psychological taxonomy

A functional classification, since it refers only to the consequences of the AGENT's behavior, says nothing in itself about the AGENT's psychology. Many of the records encourage us to believe in the possibility of a more revealing taxonomy, however, one that distinguishes differences in the psychological demands on the AGENT; in particular, capacities for "mind-reading." We have taken the opportunity to assess such capacities where the data justify it, but to attempt a complete taxonomy of deception that reflects the true mental complexity involved would be rash given only the data currently available.

The evidence which allows us to make distinctions on the basis of mind reading is patchy but merits public scrutiny and discussion because of its significance for our understanding of the nature of primate cognition. To this end, we focus on what must be *represented* in the brain of the AGENT about the states of other individuals – states for which psychological, or what Dennett (1983) called "mentalistic," terms are appropriate descriptors. Two of the concepts used here require further explanation.

2.4.1. Representation. By "representation" we mean simply a neurally coded counterpart of some aspect of the world. We shall not be concerned here with *how* the social world is coded in the brain so much as with *what* is represented: What entities are discriminated during the classification and decision processes of primate social cognition?

The concept of representation is of course central to artificial intelligence (AI), and it has been argued that one of the main benefits of AI to behavioral scientists lies in an enforced precision of expression in models of psychological processes (Boden 1977). This and other links between AI and the study of human cognition have accumulated so fast that a hybrid discipline of cognitive science is now established (e.g., Anderson 1983; Pylyshyn 1984). Although links with animal psychology are more tentative and contentious (Dickinson 1980; Roitblat and commentaries 1982), framing hypotheses about primate social cognition in terms of hypotheses about specific entities represented in the AGENT is an important step in the study of social intellect. The notion that the evolution of primate intelligence can be explained as an adaptation for handling the complexities of the social environment (Chance & Mead 1953; Humphrey 1976; Jolly 1966) is an exciting one and the focus of increasing attention in primatology and related disciplines (Cheney & Seyfarth 1985a; Cheney et al. 1986; Byrne & Whiten 1988). However, much of the existing literature involves assertions which are frankly woolly. We aim at a more explicit approach to just where it gets us to consider primates as "natural psychologists" (Humphrey 1983).

2.4.2. Psychological terms. To be of real interest as a "psychologist," the primate must be representing not merely the behavior of others, but phenomena which merit psychological labels, such as what others "believe" or "intend." We already began relying on such terms in the functional classification described earlier, for it is difficult to envisage any definition of deception which does not entail some conception of the deceived individual *misinterpreting* the behavior of the deceiver. We are forced to use psychological terms such as "misinterpretation" to talk sensibly about the behavior of interest. For example, the first major functional class, *concealment*, requires the observer to decide that a consequence of the AGENT's behavior is that the TARGET *doesn't know* where the concealed object is. But in a psychological classification, the issue becomes whether the AGENT, in addition to the observer, is representing the possibility that the TARGET doesn't know the object's location.

We want to argue that this does not imply an anthropomorphic approach, nor indeed any kind of subjective approach in which there is no room for *objective* agreement between observers about the class to which any particular instance of behavior belongs (see Silverman 1983). The worry among many behavioral scientists [e.g., Skinner: "The Operational Analysis of Psychological Terms" *BBS* 7(4) 1984] is of course that psychological terms must refer to private mental states which are beyond any objective study. But we would appeal here to Wittgenstein (1953), who argued that a child, as an apprentice language user, must gain an understanding of a psychological term by noticing which aspects of *behavior* are picked out publicly by the language-using community as being relevant to that term. On this model of the way in which psychological terms acquire meaning for us, the attribution of mental states to others should not be regarded as a subjective affair. If the language-learning child, working from observable behavior, finds psychological terms both indispensable and unproblematic, it is surely wise to assess their utility for scientists working under a similar constraint.

One objection to this approach is that the implied close correspondence between a behavioral description and a psychological category appears to make the latter redundant, and we seem to be drawn back to a mindless behaviorism. Our reply is that a psychological representation is an *economical* one. A psychological term picks out a limited set of relevant characteristics in a complex array of behavior and context; these could be described in nonpsychological terms, but only at much greater length (Bennett 1978). Our argument about the human as scientific psychologist applies also to the case of the primate as psychologist.

First, psychological distinctions *can* be made by a primate on the basis of observations of behavior. As Bennett (1976) argues:

> There can also be languageless beliefs about beliefs. We human observers can get plenty of evidence as to what animal B believed on various occasions, and can establish and test theories which support predictions about what he will believe on some further occasion; and all our data for this consists in behavior of B's which is perceptible by animal A as well. So A can have all the epistemic intake that would be needed for beliefs about B's beliefs. (p. 110)

Second, decision making is likely to be more efficient if information processing (by the AGENT, about animal B) proceeds up to the appropriate hierarchical level (Dawkins 1976), which in some cases will be the one corresponding to the patterns of B's behavior and its context which we distinguish and label with psychological linguistic terms. To quote Bennett again (1978) commenting on Premack and Woodruff's evidence (1978) that chimpanzees can represent other individuals' states of mind: "for Sarah to get from [behavioral] data to prediction by a route which didn't attribute beliefs to the human actor she would need an extremely complex inference, whereas she could get there without undue complexity if along the way she had hypotheses about the human's mental states" (p. 559). Premack and Woodruff themselves conclude that "the ape could only be a mentalist . . . he is not intelligent enough to be a behaviorist" (p. 526). To illustrate the implications of this conceptual framework with a concrete example anticipating the data section which follows, we propose that one monkey must be representing another's *attention* if and only if, when faced with extensive variations in the second monkey's eye, head, and body movements, the *one common factor* to which adjustment is made is the monkey's attentional focus.

The way the resulting psychological classification maps onto the functional one is that the form of any representation is related to the goal the act in question is designed to achieve. So, for example, where the AGENT adjusts his behavior to attain the functional consequence of concealing an object from a TARGET (in other words, *concealment* is a goal for the AGENT), the AGENT must represent the relevant psychological state of the TARGET – that the object is concealed, from the viewpoint of the TARGET. Accordingly, after giving the functional definition of each subclass followed by illustrative examples of it, we discuss the *further* evidence required to demonstrate the AGENT's corresponding mental representation of another individual's state of mind.

2.5. A taxonomy of deception in primates

In what follows, each example is quoted together with its index number in the complete catalogue (Whiten & Byrne 1986). Undated records are responses to our questionnaire.

2.5.1. Concealment (A). Here the AGENT's behavior functions to conceal something from the TARGET.

Hiding from view (A1). The function of this class of behavior is to hide an object (or a part of the AGENT, or the AGENT's whole self) by screening it from the TARGET's view. In the case of this first subclass, we will be generous with examples that also illustrate the psychological distinctions we are pursuing.

Example (No.26, Kummer, hamadryas baboons): The unit was resting. An adult female spent 20 minutes in gradually shifting in a seated position over a distance of about 2 metres to a place behind a rock about 50 cm high where she began to groom the subadult male follower of the unit – an interaction often not tolerated by the adult male. As I was observing from a cliff slightly above the unit, I could judge that the adult male leader could, from his resting position, see the tail, back and crown of the female's head, but not her front, arms and face; the subadult male sat in a bent position while being groomed and was also invisible to the leader. The leader could thus see that she was present, but probably not that she groomed. The only aspect that made me doubt that the arrangement was accidental was the exceptionally slow, inch by inch shifting of the female. This had in fact caused me to focus on her behavior so long before she had reached the final position.

Example (No.66, de Waal 1982, chimpanzees): Dandy and a female were courting each other surreptitiously. Dandy began to make advances to the female, whilst at the same time restlessly looking around to see if any of the other males were watching. Male chimpanzees start their advances by sitting with their legs wide apart revealing their erection. Precisely at the point when Dandy was exhibiting his sexual urge in this way, Luit, one of the older males, unexpectedly came round the corner. Dandy immediately dropped his hands over his penis concealing it from view.

Example (No.67, de Waal 1982, chimpanzees): On another occasion Luit was making advances to a female while Nikkie, the alpha male, was lying in the grass about 50 metres away. When Nikkie looked up and got to his feet, Luit slowly shifted a few paces away from the female and sat down, once again with his back to Nikkie. Nikkie slowly moved towards Luit, picking up a heavy stone on his way. His hair was standing slightly on end. Now and then Luit looked round to watch Nikkie's progress and then he looked back at his own penis, which was gradually losing its erection. Only when his penis was no longer visible did Luit turn around and walk towards Nikkie. (p. 49)

De Waal quotes other similar examples (Nos.99, 100) and Dunbar describes one like Kummer's, except that the female hid her whole self from her harem male while mating with a subadult male (No.12, gelada). The following differs in that a physical object is concealed:

Example (No.3, Altmann, yellow baboons): An animal that has found a choice food item sometimes tries to keep the food out of sight, e.g. by turning its back.

What in these records would constitute evidence that concealment is achieved by means that are fruitfully interpreted as acting like a "psychologist?" Is the AGENT's behavior adjusted to, and therefore dependent on, representation of the ability of the TARGET to perceive an object depending on its screening from the TARGET's viewpoint?

Such evidence is apparent in the Kummer example, for the female was not acting on any simple egocentric or AGENT-centered rule such as moving until she could not see the leader; rather her posture was finely adjusted to what the leader could see and thus based on a TARGET-centered representation. By contrast, there is insufficient data in the first chimpanzee example to cast doubt on the simpler idea that the behavior was driven by an AGENT-centered rule like "put hand in front of penis." That a TARGET-centered rule more like "avoid Nikkie seeing my erect penis" is guiding Luit in the next example is evidenced by his visual monitoring behavior back and forth between his penis and Nikkie's location (and viewpoint). Without such monitoring behavior, "turning one's back" on someone, although it sounds like an inherently TARGET-centered behavior, could just be the AGENT's effort not to see the TARGET.

These examples show, as we pointed out in Section 1, that evidence which best helps us to make distinctions among representations underlying behavior emerges when the latter is adjusted to the TARGET's appraisal of the situation, in the face of variations in the physical configuration of TARGET and AGENT.

Acoustic concealment (A2). Here the AGENT acts quietly, such that the TARGET's attention is not attracted.

Example (No.13, Dunbar, gelada): Kummer noted "acoustic hiding" in captive gelada – the suppression of the loud vocalisations which when given by males when females groomed them can be heard from more than 100m away; Kummer observed that when a female was removed together with the subordinate male from his captive group and the two were placed in a side cage with auditory but not visual contact with the rest of the group, the male suppressed his loud call when the female finally succumbed and groomed with him. In the field we have also noted this. The animals do not give the shrill (in the case of the female) and loud (in the case of the male) post-copulation calls that are very characteristic of this species. Copulations in these circumstances are totally silent. Instances of acoustic hiding of either kind perhaps amount to 5 or 6 all told.

Similar cases have been reported in chimpanzees (No. 84, de Waal 1982; No. 81, de Waal 1986a). However, acoustic concealment is shown in contexts other than mating. Whitehead (No. 20) provided lengthy and detailed documentation of the way in which two individuals in one group of howling monkeys inhibited their roars until in a position to do so with maximal surprise to a second group, thus gaining preferential access to a resource.

The issue of mental representation here parallels the case of visual hiding; instead of evidence that the behavior is flexibly adjusted to what the TARGET can see, we must ask about its adjustment to what the TARGET can hear. However, because the latter does not depend on a line of sight, the requisite observations may be more difficult to obtain and are not clear in any of the above examples; indeed, it may be that there is little requirement for such sophisticated adjustment in natural situations.

Here is a challenge for future observation. Is there any evidence for an AGENT acting more or less quietly according to the likelihood that a TARGET can hear him? If Kummer's hamadryas female (No. 26) were making a noise in her surreptitious actions, would she pause or act more carefully if she noticed the leader watching her? Would she wait until he was "listening to something else" before continuing?

Inhibition of attending (A3). In this subclass, the AGENT avoids looking at a desirable object when such looking would lead one or more TARGETS to notice it.

Example (No. 2, Fossey, gorillas): S's group travelling slowly between feeding sites in a relatively straight line along a narrow trail. Four other animals behind S in a line. S looks up into *Hypericum* tree and spies a nearly obscured clump of *Loranthus* vine. Without looking at those behind her, she sits down by the side of the trail and begins to self-groom intently until the others have passed her and all are out of sight some 15 feet ahead. Only then did S stop "self-grooming" to rapidly climb into the tree, break off the vine clump and descend with it to the trail to hastily feed on it before running to catch up with the group.

Similar examples in chimpanzees were offered by Plooij (No. 24), Menzel (Nos. 89, 91, 93; 1974), van Lawick-Goodall (No. 86; 1971) and de Waal (No. 69; 1982). These include evidence, as convincing as Fossey's finding, that one individual is inhibiting attention to an object so as to avoid giving cues to other individuals. The one candidate monkey example provides no more than a hint that the same inhibition may occur, in the final "accidental" phase:

Example (No. 7, Ashe, mangabeys): The old female mangabey would often use distracting tactics to get at food when the male was sitting over it. She would pace around him, keeping her eye on the food and in coming closer would "accidentally" walk over a piece of food which would, of course, remain in her hand as she strolled off.

However, inhibition of attention may be more common when we are dealing with purely social interactions in monkeys, no object being involved:

Example (No. 64, de Waal 1986a, rhesus macaques): Subordinates sometimes ignore threats they must have seen. They sit frozen still, with a body position indicating readiness for flight, and look around, especially slightly upward, while avoiding turning the head in the dominant's direction (although they seem to monitor the dominant by means of peripheral vision).

Example (No. 6, Altmann, yellow baboons): Mothers with weanlings will often "ignore" (avoid visual contact with) their milk-begging offspring. Ignoring as a social strategy, used in many situations, is widespread and was described in my rhesus monograph. It may be the most common form of deceptive behavior.

Similar examples were given for chimpanzees (Nos. 78, 80, de Waal 1986a). If the AGENT is acting as psychologist, this is a particularly interesting class of behavior: To the extent that the AGENT's behavior is appropriately tailored to changing configurations of the two players, it implies that the AGENT represents *the power of the TARGET to use the AGENT's attention to guide his (the TARGET's) own attention*. The AGENT as psychologist is analysing the TARGET as a psychologist analysing the AGENT – what Dennett (1983) described as third-order intentionality, to be contrasted with the second-order intentionality apparent in the hamadryas example of *hiding from view*. Evidence for such tailoring would be that looking was inhibited only when the TARGET's position allowed him to see the AGENT, and only when the AGENT's direction of gaze was visible to the TARGET – for example, if either player turned his back on the other, we would want to know whether the AGENT was then prepared to look at the locus concerned. In general, this evidence is lacking. The fact that Fossey's gorilla S inhibited her attention only while the other gorillas were present is suggestive, but clearly we would need more detail to show that S was taking into account her companions' powers of attention, rather than their mere presence.

2.5.2. Distraction (B). The next four classes all involve behavior which functions to *distract* the TARGET's attention from some locus at which it is directed, toward a second locus. This is deception when there is nothing that would merit attention at the second location, whereas there would ordinarily be something when the nondeceptive behavioral counterpart was performed by the AGENT. The following classes distinguish the different ways in which the AGENT's behavior can function to shift the TARGET's attention to a second locus.

Distract by looking away (B1). The AGENT distracts the TARGET's attention from one locus by looking away toward another locus in such a way that the TARGET also looks there.

Example (No. 73, Byrne & Whiten 1985, chacma baboons): Subadult male ME attacks one of the young juveniles who screams repeatedly, pursuing ME while screaming (this is common when aid has just been successfully solicited). Adult male HL and several other adults run over the hill into view, giving aggressive pantgrunt calls; ME, seeing them coming, stands on hindlegs and stares into the distance across the valley. HL and the newcomers stop and look in this direction; they do not threaten or attack ME. No predator or baboon troop can be seen through 10 × 40 binoculars.

Byrne and Whiten reported a further case (No. 74) and similar examples were quoted for chimpanzees (No. 82, de Waal 1986; Nos. 22, 25, Plooij). Plooij confirmed that such behavior can function to deceive. In one instance, a young chimpanzee, Pom, became too familiar and started grooming him. Finding that staying motionless was no discouragement, "I tried another way to get rid of her. Having observed her mother deceive Flint, I did the same. While sitting, I suddenly looked in a particular direction, moving my head a little from left to right, acting as if there was something to be seen in the distance. It worked! Pom looked as well and looked back at me again. I continued acting and she walked in that direction" (No.23).

This class of behavior is functionally the reverse of *inhibition of attending* (A3), because it involves getting the TARGET to attend to some trivial locus when his attention is already on the crucial one, whereas in *inhibition of attending* the function is to prevent attention to a locus as yet unnoticed. But cases of both A3 and B1, where they become goal-directed, do share the necessity for the AGENT to be more of a psychologist, representing not just the TARGET's attention (as discussed above for A1 [hiding from view] and A2 [acoustic concealment]) but the ability of the TARGET to *use the AGENT's attention* to direct his own. Evidence for the latter includes looking away at a *plausible* focus of interest (e.g., distant and complex scenes rather than a nearby cliff face), and only looking away when this behavior can be seen by the TARGET (we might expect the AGENT to stop looking away if the TARGET turned his back). All the examples are consistent with the AGENT's making such discriminations, but none are conclusive and in the future more care should be taken to describe how looking away is adjusted to the behavior of the TARGET.

Distract by looking away with linked vocal signal (B2). The AGENT distracts the TARGET's attention from one locus by looking away at another locus and vocalizing in such a way that the TARGET looks there or at least loses the original focus of attention.

Example (No.27, Gautier, guenons): A threatens and chases B with aggressive calls. His/her calls evoke the same calls from C, D . . . cornered, B emits a social alarm call while looking at a hypothetical predator. The animals ascend the trees and the aggressive chase ends, at least for the present.

Very similar examples were given by de Waal (No.103, 1986, chimpanzees) and Seyfarth (No.88, in Dennett 1983, vervets).

Evidence for the AGENT representing the TARGET's capacity for changing his attentional focus may be harder to get in this subclass than the last, because a single alarm vocalization may be enough to achieve the effect (as opposed to needing more continuous modulation of visual attention) and because a broadcast signal does not need to take into account the nuances of attention that visual signals do. Perhaps, as suggested for looking away, there will be observable differences regarding whether the AGENT performs such behaviors in circumstances which are *plausible* to the TARGET (e.g., not giving a deceptive eagle alarm in the undergrowth; not giving a deceptive leopard alarm in the canopy).

Distract by leading away (B3). The distraction in this case is achieved by the AGENT's leading the TARGET away from the first locus to another one, allowing the AGENT to return to the first free of competition.

Example (No.87, van Lawick-Goodall 1971, chimpanzees): One day, when Figan was part of a large group and, in consequence, had not managed to get more than a couple of bananas for himself, he suddenly got up and walked away. The others trailed after him. Ten minutes later he returned, quite by himself – and, of course, got his share of bananas. We thought this was coincidence – indeed, it may have been on that first occasion. But after this the same thing happened over and over again – Figan led a group away and returned, later, for his bananas. (p. 96)

Other examples where repeated use of the "leading away" tactic makes clear its deceptive function were given by Menzel (Nos.95, 96, 1974, chimpanzees). In some examples it is not so clear that the leading away performed a deceptive function, as opposed to merely being followed by an opportunistic return (No.77, Byrne & Whiten 1985, chacma baboons; No.8, Ashe, mangabeys).

Turning to the issue of representation, this behavioral class is complicated in that we must consider the possibility of two sequential goals: to lead the TARGET away from the first locus, and to return to it unaccompanied – the basis of a simple plan. Each of these subgoals entails representing different aspects of the TARGET's psychology.

With respect to the first goal, the AGENT must represent the TARGET's attention (to the AGENT), assuming that the TARGET must attend to the AGENT as a prerequisite to being led away. But although this may suffice in some cases (the mere departure of the AGENT then being sufficient to cause the TARGET to follow), in others the TARGET may evaluate the desirability of following the AGENT. Evidence for the latter would be tailoring of the AGENT's actions to variations in the motivation of the TARGET; for example, waiting until the TARGET's interest in the first locus drops to a level where it can be counterbalanced by being led away. No such evidence is available yet.

Pursuing the second goal – returning unaccompanied – requires either that the TARGET should cease to attend to the AGENT, or that the TARGET should, although still paying attention to the AGENT, judge that it is no longer worthwhile to follow the AGENT. Representation of the TARGET's motivation to follow would be indicated by such evidence as waiting until the AGENT became interested or engaged in some new activity before returning to the first locus, and adjusting the form and timing of the return to any tendencies on the part of the TARGET to follow. Such records may already be available – it is certainly feasible to collect them – but do not appear to be in print yet.

Distract with intimate behavior (B4). Here the distraction is achieved by the AGENT's shifting the TARGET's attention to some part or extension of his own body, which is highlighted gesturally or posturally.

Example (No.62, Strum, in Jolly 1985, olive baboons): One of the female baboons at Gilgil grew particularly fond of meat, although the males do most hunting. A male, one who does not willingly share, caught an antelope. The female edged up to him and groomed him until he lolled back under her attentions. She then snatched the antelope carcass and ran. (p. 412)

Other examples involve grooming, facial expressions, embracing, presenting, and missile throwing (No.63, de Waal 1986a, rhesus macaques; No. 61, Winner, in Ettlinger 1983, chimpanzees; No.28, Gautier, guenons;

Nos.9, 10, Ashe, mangabeys; No.101, de Waal 1986a, chimpanzees; No.11, Dugmore, sakis).

Where such a tactic is seen to be goal-directed, the AGENT must be representing the TARGET's attention, which is to be distracted. However, unlike in the case of leading away, there is not the same opportunity to keep the deception hidden. In the example above, the resource just has to be grabbed while the AGENT's attention is distracted, the deception thus being made obvious and presumably difficult to repeat.

There is an alternative explanation for this class of behavior, one that does not impute a deceptive intention and will often be hard to refute: When the most preferred course of behavior is thwarted by the TARGET, execution switches to the next most preferred activity (grooming in the above example) which coincidentally distracts the TARGET and allows the AGENT to switch back to his most preferred activity. Evidence which would count against this "blind opportunism" interpretation would be behavioral characteristics which were tailored to achieving distraction; in the above example, not just *any* grooming, but specifically the kind (under the chin for example) that would encourage the TARGET to "loll back" and forget the meat. If the AGENT doesn't attempt a grab as soon as the TARGET's eyes move off the meat, but chooses a time when response is more sluggish, we have further cause to conclude that the AGENT is taking the TARGET's attention into account. A different approach to testing the opportunism hypothesis lies in computing the a priori probability of the AGENT's performing the distracting act at that precise moment by chance. This would actually be the beginning of an analysis of the statistical likelihood of the "unplanned" hypothesis. This statistical approach could be combined with discriminating those instances in which the behavior is appropriate to the goal of distraction, narrowing the a priori probability of, for example, grooming under the TARGET's chin at just that moment. Repeated occurrences of a particular type of tactic could make such an analysis fruitful. In the meantime, there is one report that demonstrates (limited) planning on the basis of both the tailoring and the statistical criteria: A chimpanzee was chasing another, who ran behind a tree. He threw a brick at her on one side, yet moved to catch her on the side to which her attention predictably shifted (No.101, de Waal 1986a).

2.5.3. Creating an image (C). Now we move on to cases where the AGENT's behavior functions to portray the AGENT in a way that, rather than merely affecting the TARGET's *attention* as in A and B above, causes the TARGET to misinterpret the behavior's significance for the TARGET in other ways. The two classes below are distinguished by the way the behavior acts to create this impression.

Present neutral image (C1). The deception involves behavior which is simply nonthreatening, just in the sense that it is of little or no significance to the TARGET.

Example (No.1, Fossey, gorillas): Majority of group day nesting within a 25 feet radius with low-ranking Q at the edge of the group. After intently gazing at P (a desirable infant) from the side-lines, Q stares in the opposite direction, circles and begins bending down branches for a nest. After momentarily sitting in the nest, Q, with gaze averted from P, gets up and moves a few feet closer to repeat the activity of "nest-building." After roughly 40 minutes and 6 "nests" later, Q was sitting next to dominant female E and gazing directly at infant P.

Example (No.65, de Waal 1982, chimpanzees): Yeroen hurts his hand during a fight with Nikkie. . . . Yeroen walks past the sitting Nikkie from a point in front of him to a point behind him and the whole time Yeroen is in Nikkie's field of vision he hobbles pitifully, but once he has passed Nikkie his behavior changes and he walks normally again. For nearly a week Yeroen's movement is affected in this way whenever he knows Nikkie can see him. (p. 47)

Example (No.99, de Waal 1986a, chimpanzees): The most dramatic instance of self-correction occurred when a male, who was sitting with his back to his challenger, showed a grin upon hearing hooting sounds. He quickly used his fingers, to push his lips back over his teeth again. The manipulation occurred three times before the grin ceased to appear. After that, the male turned around to bluff back at his rival. (p. 113)

On other occasions Yeroen was observed to be "feigning to be in a very good mood" (No.98, de Waal 1982). Note also the self-grooming done by the gorilla S in example No.2, already described.

Only one monkey example was given but it could represent a fairly common behavior, very similar to what is claimed for Yeroen's mood-feigning above: (No.5, Altmann, yellow baboons): "Occasionally I have seen an animal remain very calm as it got progressively closer to another individual, then suddenly launch an attack."

If it is appropriate to regard the AGENT as acting as a psychologist in any of these cases, then what distinguishes this class from those described above is that instead of the AGENT needing to represent the *attention* of the TARGET and what the TARGET is likely to be able to see, what is required is a representation of the TARGET's *interpretation* or *evaluation* of the AGENT's behavior. In other words, the AGENT as psychologist is here adjusting his future behavior to the current state of the TARGET's evaluation of the AGENT's behavior, rather than using a set of discrete rules of the sort: "if T does x, do y . . . if T does m, do n. . . ." In the case of Fossey's gorilla Q, information which might be integrated to compute this current state includes past and current signs of nervousness by the mother, E; Q's current credibility given her past use of this tactic; and reasons for E to be mistrustful, such as recent threats to the infant P, or the current approach of others sharing Q's intent. The structure of Q's behavior – that she built six nests, with gaze averted – indicates that her behavior may well have been sensitively adjusted to such an evaluation on the part of E. However, a sceptic must argue that she could have been following a simpler rule such as: "if E stands and picks up P, make a nest." Again we need more information; it is only where we can describe Q's acts as adjusted to the state of E's evaluation across variation in several different contextual variables such as those suggested above that we start to gain real power from the idea of the primate as psychologist.

Present affiliative image (C2). Here the image presented is not merely neutral but is interpretable as affiliative.

Example (No.4, Altmann, yellow baboons): Every day, one can see females approach mothers, pretend to be primarily interested in grooming the mother when what they are really after is an opportunity to sniff, touch or hold her infant. (As a result, mothers with young infants are often very well groomed.)

"But is the mother really deceived?" asks Altmann: "Surely the multiparous ones know exactly what's going on!" Altmann hits the nail on the head here. We must be particularly sceptical about whether such behavior is deceptive in the sense that the TARGET misinterprets it; rather, the affiliative act by the AGENT may represent a benefit to the TARGET which fully compensates for the resource the AGENT gains from the TARGET. This is nevertheless an interesting class of possible deception, for if *we* find it difficult to weigh these costs and benefits, so might the interactants! If the AGENT makes any psychological assessment, this will have to include a representation of the TARGET's assessment of likely benefit/cost ratios. This alone could place special demands on social intellect to avoid the mistrust apparent in the following case:

Example (No.68, de Waal 1982, chimpanzees): If Puist is unable to get a hold of her opponent during a fight, we may see her walk slowly up to her and then attack unexpectedly. She may also invite her opponent to reconciliation in the customary way. She holds out her hand and when the other hesitantly puts her hand in Puist's, she suddenly grabs hold of her. This has been seen repeatedly and creates the impression of a deliberate attempt to feign good intentions in order to square accounts. Whether we regard it as deceit or not, the result is that Puist is unpredictable. Low-ranking apes hesitate when she approaches: they mistrust her. (p. 66)

Further similar chimpanzee examples are given by de Waal (Nos. 102 and 104). The same provisos apply as in C1 (present neutral image) concerning the extra detail which would be required as evidence of the AGENT's representation of the TARGET's evaluation of the AGENT's behavior.

2.5.4. Manipulation of TARGET using social TOOL (D). So far we have been concerned with dyadic interaction. Although sometimes several individuals might be involved, the essential nature of the deception could be characterized using the AGENT and TARGET scheme. For example, when an AGENT conceals something from several other individuals, it makes sense to think of this as involving parallel dyadic AGENT–TARGET interactions. By contrast, in this form of deception and the next it is essential to describe the deception in at least triadic terms.

The function of the behavior in the first class is to manipulate one individual, the TOOL, so as to affect the TARGET to the AGENT's benefit. The following subclasses are differentiated on the basis of the way the deception functions within this social configuration.

Deceive TOOL about AGENT's involvement with TARGET (D1). The function of the AGENT's behavior is to mislead the TOOL about the significance of the involvement, or the behavioral interaction, between the AGENT and the TARGET (we specify "involvement" because in some cases the TARGET may see just a static tableau, rather than an active interaction between AGENT and TARGET).

Example (No.71, Byrne & Whiten 1985, chacma baboons): Adult female ML is feeding where a patch of turf has been loosened. Young juvenile PA tentatively approaches and although ML makes no threat, PA screams. As happens in the normal context, JG, the only adult male of the group, runs towards them and ML retreats, leaving PA to feed on the food source. Two minutes later this sequence is repeated. Five minutes later the same sequence recurs. JG again acts as if the vocalisation signified an attack by ML on PA, but this time he chases ML for 20 m, jumps upon her and lunges as though to bite her. (p. 670)

Byrne and Whiten describe two other examples (Nos. 70, 72). In a further case (No.29, Rasmussen, yellow baboons) the benefits accruing through such exploitation are clear:

Example: There were 13 adult males in the troop we studied, and coalitions of two to six mid-low ranking males frequently challenged the two highest ranking males for possession of oestrus females. The eighth-ranking male, KMO (who was fairly old, judging from tooth wear) was particularly adept at gaining courtships in this way. . . . (Commonly) when another male initiated the coalition, KMO joined in the background, then ran off with the female when the consorting male chased the initiator. If KMO was deliberately "using" the other coalition members in the above example, it may be because he'd learned through experience that he could often run off with an oestrus female when her consort was occupied chasing another adult male. . . . Using coalitions to obtain females, KMO ended up with a total number of days consorting potentially fertile females that was second only to the highest ranking male in the troop.

If the AGENT is acting as psychologist in any of these examples, he will be representing one individual's (the TOOL's) *evaluation of the significance of an interaction between two other individuals* – the TARGET and the AGENT. In the case of the juvenile baboon PA above, the fact that the scream was uttered in a situation in which the TOOL had no evidence of a genuine attack on PA indicated that the behavior was being adjusted according to the TOOL's (inferred) evaluation. In all three records PA only screamed when potential helpers were available but out of sight. The plausibility of the tableau presented to the TOOL was maintained by PA's desisting from feeding until the TARGET had been chased off.

Deceive TOOL 1 about TOOL 2's involvement with TARGET (D2). Here four individuals are involved, the function of the AGENT's behavior being to mislead the TOOL about the significance of the configuration or behavioral interaction existing between a second TOOL and the TARGET.

Example (No.21, Snowdon, patas): Frequently sub-adult males would carry an infant and then approach the adult male. They would start to tease the male, and then when he finally reacted, the infant would squeal, arousing all of the females in the group who would then chase the male around the compound for several minutes.

In this example there is little to warrant interpreting the AGENTS (sub-adults) as psychologists.

Deceive TARGET about AGENT's involvement with TOOL (D3). Here it is the TARGET who is deceived about the significance of the configuration or behavioral interaction caused by the AGENT's action on the TOOL.

Example (No.16, Dunbar, geladas): A yearling was geckering and mewing at its mother after failing to gain access to the female's nipples while she was feeding. So it then moved across to the harem male who was grooming with another female nearby and geckered and mewed at them. They ignored. So the infant hit out at the male's back, then pulled his cape. The male ignored. So after holding onto his cape for a few seconds, the infant pulled it again. This time the male turned round and hit out at the infant. The infant then ran across to its mother, who had looked up at the commotion. When the infant approached, the mother allowed it to go on the nipple at once, and then she moved off carrying the infant away from the male.

Dunbar describes three more variations on this theme (Nos.14, 15, 17).

The AGENT as psychologist has a task exactly equivalent in complexity to that involved in subclass D1 (representing the judgment by one individual of the significance of what is happening between two others); it is just that the roles of TOOL and TARGET are now reversed. This reversal represents an important distinction in classifying what is going on within the range of primate deception; we need not repeat the earlier discussion (2.5.4.) of grounds for establishing such representations. In the case of the gelada infants above, it would be important to note whether, after only a mild response by the male, the infant looks at the mother to check whether she appears anxious enough to treat the infant as if he needs support.

2.5.5. Deflection of TARGET to FALL GUY (E). The function of this class of behavior is to divert the TARGET posing the problem toward a third party, the FALL GUY. Unlike the TOOL in class D, whom the AGENT causes to take an active part in dealing with the TARGET problem, the FALL GUY is an essentially passive victim in the sequence.

Example (No.19, Sugiyama, Japanese macaques): No. 5-103 (5 year old just matured male and transferred from troop B) came and began to eat. No. 3-42 (24 year old 2nd ranking male) came near from behind him. When No. 5-103 noticed No. 3-42, there was only 3m between them; the former looked aside, vocally attacked a feeding female nearby and chased her 5m away. Seeing them, No. 3-42 also barked and chased to follow her for a few metres. During this time No. 5-103 ate wheat again for half a minute and then walked away. I observed the same type of behavior more than 50 times during the 27 year study.

Byrne and Whiten give similar examples where the problem thus dealt with is a threat or attack (Nos.75, 76; 1985). The tactic is also used when competing for grooming (No.18, Dunbar, geladas) and for food, as shown in the following example, which continues the incident described earlier (No.62, sect. 2.5.2) by Strum (No.83 in Jolly 1985, olive baboons): "Later the same male killed again. Again she groomed him. This time he kept a hand on the carcass. She left off grooming, and chased after his favorite female. He dithered, but at length went to his friend's defence. The first female promptly doubled back to snatch the antelope."

Such examples share with the other triadic ones the requirement that the AGENT acting as psychologist must represent the judgment made by one individual (in this case the TARGET) about what is taking place between two other individuals (in this case the AGENT and FALL GUY). The difference between this class and the last lies in the AGENT's need to select the individual who is to be the FALL GUY. Clearly, an individual must be chosen who has the right characteristics to engineer a deflection of the TARGET. In the Strum example, the choice of the male's "favorite" female is just this sort of evidence which – if there is more of it – we can use to assess the extent to which the AGENT's behavior is indeed being adjusted to the TARGET's evaluation of the social situation.

3. The questions

3.1. Are there phylogenetic differences in tactical deception?

In the above classification, any one indexed record may be limited to a single event, or, as in several contributions, it may be generic, mentioning that similar behavior has been seen repeatedly. We must therefore be cautious in any numerical comparisons. It seems safest to indicate simply the presence or absence of evidence for each type of deception (Table 1) and to put forward the following as working conclusions which may stimulate more confirming or disconfirming data.

3.1.1. Chimpanzees versus gorillas. Coming full circle from our opening remarks about the reputation of chimpanzees for deceit, we note that they alone are cited for nine of the thirteen classes of deception, compared with two for gorillas. At least three alternative hypotheses might account for this difference. One is that insofar as

Table 1. *Presence (*) and absence (–) of current candidate examples of tactical deception in primates (based on corpus in Whiten & Byrne 1986)*

Class of deception:	Conceal			Distract				Create image		Use tool			Deflect	Null	No.
Subclass	A1	A2	A3	B1	B2	B3	B4	C1	C2	D1	D2	D3	E		
Prosimian	—	—	—	—	—	—	—	—	—	—	—	—	—	*	7
Callitrichid	—	—	—	—	—	—	—	—	—	—	—	—	—	*	7
Cebid	—	*	—	—	—	—	*	—	—	—	—	—	—	—	3
Colobine	—	—	—	—	—	—	—	—	—	—	—	—	—	*	6
Cercopithecine	*	*	*	*	*	*	*	*	*	*	*	*	*	*	59
Hylobatid	—	—	—	—	—	—	—	—	—	—	—	—	—	*	5
Pongo	—	—	—	—	—	—	—	—	—	—	—	—	—	—	2
Gorilla	—	—	*	—	—	—	—	*	—	—	—	—	—	*	10
Pan	*	*	*	*	*	*	*	*	*	—	—	—	—	—	12

Note: An entry (*) in the penultimate column (Null) indicates at least one response stating that no tactical deception has been observed. The last column shows the number of workers to whom the questionnaire was circulated for each taxonomic group.

deceptive competence is concerned, chimpanzees are superior in social intellect. Another hypothesis is that in the stable intimacy of a gorilla family group there are few opportunities for individuals either to get away with deception or to profit from doing so, whereas the opposite is true in the fluidity of chimpanzee society. If so, the explanation of the observed chimpanzee-gorilla difference would be "ecological" rather than "mental" and we would really expect gorillas to be able to generate behavior in the other seven subclasses of deception. Of course, even if an intellectual difference exists it might have been caused, through natural selection, by a difference in social ecology! Another explanation for the apparent difference is that all the chimpanzee examples come from captive or provisioned animals, for whom particularly intensive and continuous records have been made. However, one cannot precisely quantify such bias by counting man-hours of observation per species, since in different studies the focus of attention varies, and it is this focus which governs the possibility of subtle behaviors being seen.

3.1.2. Chimpanzees and "triadic" deception. Despite the number of chimpanzee examples, there is none in the last four subclasses of deception described above, which involve inherently triadic manipulations. This is surprising because it is natural to think of multiple social interactions as the most cognitively demanding. For example, because of its relatively high frequency in chimpanzees, it has been suggested that triadic play facilitates the development of complex social skills (Smith 1982). Have chimpanzee watchers really not seen behavior of classes D and E? If they have not, perhaps we need to ask whether chimpanzees are *too* socially intelligent to be duped by such tableaux as those presented in the D and E records above.

3.1.3. Taxa exhibiting no tactical deception. There is no evidence for tactical deception in some taxa. One of the contributors recording a null finding offers an explanation apparently based on an "evolutionary threshold" (No. 47, Dugmore, lemur): "I would regard *catta* especially as not having the intelligence for such an action." However, we have only three (null) replies from those who study prosimians; we must accordingly remain agnostic on this issue until more data are in. Since we published our definition of tactical deception, examples which seem to fit in have been reported in stomatopods (Caldwell 1986) and birds (Munn 1986) and it would be surprising if equivalent behavior were beyond the capacity of a primate. Again, we must consider alternative ecological explanations. Callitrichids and hylobatids, for instance, are typically observed in monogamous family groupings which encourage a high degree of cooperation; hence deception, if it is indeed an available option, would seldom be beneficial.

It is always difficult to solicit negative findings, but we trust that displaying the current state of knowledge in Table 1 will encourage further responses that will clarify the picture. When a larger corpus of data is available, it will be important to distinguish between the following three explanations for any species differences in frequency of deception: (1) variation in observation time and quality between species; (2) ecological differences which affect the opportunity for benefits from deceit; and (3) genuine intellectual differences. [See also Houston & McNamara: "A Framework for the Functional Analysis of Behavior" *BBS* 11(1) 1987.]

3.2. *Is creative intelligence involved?*

In all our discussions so far, the social intelligence of primates has been limited to the orchestrating of behavior towards some social goal; the associated concept of representation has been used repeatedly to refer to the level at which the AGENT's analysis of the TARGET is being undertaken. Because of the paucity of relevant data in our current corpus, we have necessarily neglected the more specific issue of the extent to which the pursuit of these social goals is achieved by innovations in behavior (Kummer & Goodall 1985) which would warrant the term "creative intelligence" (Humphrey 1976).

What will we have to investigate in order to address this issue? Like workers in artificial intelligence we have allowed the term representation to include processes of perceptual classification (for example, representing the current attention of the TARGET), but psychologists often distinguish between perception and representation, the latter referring only to the coding of experience which is stored across time and used to guide behavior which may occur long after the original experience. When the social representations we have already discussed are preserved in memory in this way, we have the basis for a further sense in which the primate becomes a psychologist, for social representations could be used to construct a predictive model of the minds of potential TARGETS, which could subsequently be used to deal with a relatively novel problem presented by a TARGET.

For example, an AGENT's representation of the ability of others to use the AGENT's attention to direct their own could be used in generating behavior to solve a new social problem, either by withholding attention to things competed for (A3: concealment of attention) *or* by using a shift of attention to distract (B1: distract by looking away). This is what Plooij did in the example of *Looking away* (No. 23, Sect. 2.5.2), but do the primates themselves do it? To determine that, we need a situation where a primate generates deceptive behavior whose appropriateness hinges not merely on the TARGET's current behavior nor on the AGENT's previous reactions to such behavior, but rather on prediction about novel acts that the TARGET should perform according to a theory (held by the AGENT) about how some part of the TARGET's mind works. We are grateful to Plooij for the following example (No. 24), which appears to illustrate both novelty and the phenomenon of counterdeception:

> One chimp was alone in the feeding area and was going to be fed bananas. A metal box was opened from a distance. Just at the moment when the box was opened, another chimp approached at the border of the clearing. The first chimp quickly closed the metal box and walked away several metres, sat down and looked around as if nothing had happened. The second chimp left the feeding area again, but as soon as he was out of sight, he hid behind a tree and peered at the individual in the feeding area. As soon as that individual approached and opened the metal box again, the hiding individual approached, replaced the other and ate the bananas.

3.3. The study of deception and mind-reading: Are we proceeding in the right way?

Finally, before we design a new questionnaire with the expectation of collecting a larger corpus of upgraded quality, a number of methodological questions arise. As we had hoped, the collation of a body of disparate records has allowed a larger pattern to emerge, one that inspires more confidence than the anecdotes from a single study. Many suggestive records do not constitute one conclusive one, however. In the majority of cases there is clear evidence of deceit, but whether this is mediated by the AGENT's mental representation of the TARGET's psychology remains uncertain in most cases. For further progress, the quality of each individual record must be improved.

Before we suggest how this can be done, we must ask whether anecdotal data, even of high quality, are adequate to the task. Woodruff and Premack (1979; and see Premack & Woodruff 1978, for an analysis of related issues) imply that they are not, arguing that an experimental approach is necessary to test hypotheses about the psychological causes of deceptive actions. Unfortunately, "their scrupulous efforts to force their chimps into non-anecdotal, repeatable behavior that manifests the intelligence they believe them to have engenders the frustrating side effect of providing prolonged training histories for the behaviorists to point to in developing their rival conditioning hypotheses as putative explanations" (Dennett 1983, p. 348). Conversely, Silverman (1986), working with a pig-tailed macaque, found that where a certain level of social insight was not shown, there always remained a likelihood that the experimental set-up did not offer an adequate context for the demonstration of the monkey's natural competence. Perhaps wary of such problems, de Waal (1986a) has suggested a freer experimental approach, one that amounts to setting up facilitating contexts to encourage spontaneous intentional deception: For example, "let an ape be alone in a room, monitored by a video camera, where he or she is tempted to do something which he or she knows is not allowed by the experimenter. The forbidden act should be such that evidence for it may afterwards be detected. The prediction is that the ape will attempt to remove the evidence before the experimenter arrives" (p. 117).

Such experiments may indeed provide much needed control over the otherwise elusive occurrence of deceit (Cheney & Seyfarth 1985b), but they should not be seen as necessarily confined to the laboratory. The neatest cases of primate field experimentation involve deceiving the animals by playbacks of taped calls (Cheney & Seyfarth 1980), and this paradigm can be adapted for the study of deception itself (Seyfarth & Cheney 1988).

If science had to rely exclusively on experimental manipulation, astronomy would be in bad way; so too would most of field research in the behavioral sciences. What is essential is to make predictions which can be tested not only by further experiments but also by further observations. It has been argued that the most sophisticated instances of primate deception to date have been collected from free ranging animals (Smith 1986). The important thing here is to know exactly what to attend to when the crucial but perhaps rare events of interest occur – and this is true even when they can to some extent be "set up," as advocated by Dennett (1983) and de Waal (1986a). All the material on which the present target article is based, though mostly extracted from written records, is basically retrospective; what is needed now is a set of guidelines to our contributors which spells out exactly what to record in the future. In presenting the taxonomy of deception above, we have already addressed the question of what will constitute adequate evidence that the AGENT has representations of the psychological states of others; we also pointed to grounds for skepticism in treating some of the putative examples as deception. Commentary will be welcome on these issues, and in general on our framework for the scientific study of primates as psychologists. This should enable the very best guidelines to be agreed upon, so that future theory can be based on more solid evidence than is available now. To set this process in motion, we suggest the following guidelines and questions.

3.3.1. The role of "anecdotes." We have tried to show above that even unique anecdotal reports are valuable at this stage of our enquiry. At the very least, they may indicate the tip of an iceberg which merits deeper investigation; for this reason it will be important to collect and publish further isolated cases, particularly where these expand the phenomena catalogued. We have drawn attention in the text to the details which can make a single observation valuable. In such a case, the description of the context and historical antecedents of the event are likely to be crucial.

But can anecdotes ever be more than a jumping off point for more systematic work? We propose that the answer must be "no," although this does depend on what is meant by an anecdote. We suggest that no *single* observation can be regarded as definitive evidence in support of a hypothesis (e.g., that a certain species is capable of intentional deception). There are two major reasons for skepticism. One is that in many cases the relevant evidence involves very fine distinctions in behavior, such as exactly where two or three animals are looking from moment to moment, and the reliability of such observations can become established only when they are repeated. Second, a single instance may in many cases simply represent a coincidence.

All this leads us to emphasize the value of multiple records. Quite apart from the question of reliability, and although we have noted reasons why deception may occur only rarely, surely only those classes of deception that occur more than once will ultimately be of interest! Already in the corpus outlined above, several contributors are credited with clusters of similar records; it was such repetitions in our own observations that led us to attempt the present exercise (see Byrne & Whiten 1985; Table 1). In addition, when we have clear records of the same method of deception from independent observers of the same or even different species, we can be confident that a real pattern of behavior exists. Such concordances already exist in the corpus.

Even if we encourage the contribution of multiple records in the future, however, problems remain. One is *reporter bias*. The approach we have adopted for the present exercise tends to select just positive cases. [See also Rosenthal & Rubin: "Interpersonal Expectancy Effects" *BBS* 1(3) 1978.] Although the act described as deceptive may appear to be nicely adapted to such a function, the null hypothesis that it occurred at that instant for other reasons has to be addressed. What is the "control" frequency of such acts? Perhaps the gorilla who built six nests usually builds that many. Perhaps the baboon who groomed the male with the meat groomed him nearly all the time anyway. Reporter bias is the failure to record such negative occurrences as a baseline against which the hypothesis of deceptive functioning can be evaluated. In the case of the baboon groomer, we suggested that an approach to this issue would be to calculate the probability that, in any one minute, she would be grooming this particular male's chin; if the probability is 1/1,000, then this is the probability of making a "false positive" error in rejecting the null hypothesis that the female just happened to groom the male in this way at such an opportune time. This approach could be used as a general technique for handling the problem of reporter bias. It does of course place greater demands on observers, who must make many more observations than just the deceptive episodes. In short, in terms of our definition of deception, observers must record both the honest and deceptive behavioral counterparts.

Given these provisos, we can ask for evidence bearing on various hypotheses: that the act was deceptive; that it involved the AGENT representing the viewpoint and/or beliefs of others; and that it originated through a specific mechanism, for example, trial-and-error learning.

3.3.2. Evidence for deception. In addition to a bald description of a putative instance of deception, what is the basis for believing that the putative DUPE was indeed deceived and, in particular, that the AGENT's behavior was not accidental but was functionally or intentionally deceptive? As we proposed above, such questions can be approached quantitatively. For example, if the frequency of response by the putative DUPE is significantly less when the chimpanzee AGENT covers his penis with his hand than when he doesn't (No.66, sect. 2.5.1), then there is evidence of deception (in this case through concealment).

3.3.3. Evidence that the AGENT has representions of psychological states. There is probably no substitute here for the observer reading the discussion of examples in the taxonomy above. Then what we need to ask is: Did the AGENT achieve deception by tailoring his behavior to the social context in a way that requires the AGENT to represent another individual's psychological state? Clearly, observers attuned to the distinctions involved *can* ensure that the relevant events are recorded (e.g., Kummer 1982 and his "hiding from view" record above; the 1985 Byrne & Whiten records of "deceive tool about AGENT's involvement"; and de Waal's 1986a record of "distract with intimate behavior"). It is essential that the observer decide in advance what needs to be recorded as occurring (and as *not* occurring). Retrospective reconstruction may suffer bias.

3.3.4. Evidence for the origins of the deceptive act. In our first questionnaire we asked for "any evidence specifying the causation or ontogeny of the behavior: evidence that it is, for example, intelligent, insightful, innovative, the product of trial-and-error learning or imitation, or that any of these can be ruled out." To many, such distinctions constitute the most important issues concerning deception (Mitchell 1986) and it is accordingly interesting that even our most assiduous respondents could contribute next to nothing on this question. Is the question then worth repeating? Does it need to be made more explicit? If we have a complete record of the behavioral history of an individual, we should be able to record the occurrence of (for example) imitation or innovative behavior without any experimental intervention. Hence we presume that this gap in our knowledge reflects only practical difficulties to date – particularly since the present corpus is based on such a retrospective exercise. We anticipate that systematic prospective studies will be fruitful.

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Darwin, deceit, and metacommunication

Stuart A. Altmann
Department of Biology, University of Chicago, Chicago, Ill. 60637

What is the nature of the deceptions that primates use, and what is the nature of primates that enables them to deceive and be deceived? Whiten & Byrne (W&B) have focused on the second of these two questions, seeking evidence that the agent of a deception has "representations of the psychological states of others." The first question is no less intriguing and is more readily operationalized.

We recognize deception in animals by observing their behavior, not by observing their mental processes. However, although we can describe the deceptive behavior of the actor in a particular instance, no refinement of that description and no extension of the process of description to other instances will suffice to elucidate the nature of deception. The reason for this is that no defining motor pattern of behavior is involved. Deception is not a form of behavior, it is a type of relationship.

The essence of tactical deception is that it involves a false metasignal that is responded to as if it were true. Let me explain. Consider first a nondeceptive form of metacommunication (Bateson 1955; 1956). One primate lunges at another, jaws open. How should the target individual respond? That depends on other, ancillary behaviors of the actor that precede or accompany this lunge. If those ancillary acts include gambolling and a "play-face," then the lunge will be reacted to as play. The gambolling and the play-face are cues to the target animal about the set of contingencies that will prevail in the subsequent interaction: that the mouth-on-flesh that follows the lunge will not be forceful enough to draw blood or even hurt – a nip and not a bite. Now, the play behavior per se is, like any other interaction, a sequence of communicative acts; at each step in the interaction, the behavior of one individual is responded to by another. Thus, gambolling and the play-face are signals about other signals – they are metasignals. Such metasignals communicate the context, the mood or, less metaphorically, the signal-response contingencies of subsequent interactions. [See also Smith: "Does Play Matter" *BBS* 5(1) 1982.]

In this example, no deception is involved. The open-mouthed lunge that accompanied the gambol was indeed a prelude to a nip and not a bite. Play-fighting is very similar to physical combat. Play metamessages enable the participants to discriminate between these two classes of messages. Suppose, however, that one individual gambolled up to another and, when at close range, launched an aggressive attack. Its victim, taking its cue from the gambol, would be unprepared; it would neither have time to flee nor to get into position to defend itself. We would say that the victim was deceived. The gambolling approach was a metasignal that said, in essence, "My behavior will be playful and therefore harmless." The subsequent attack reveals that this is false.

Two ways of producing a false metasignal are available: Either change the behavior or change the metasignal. Switching from a nonchalant or even playful approach into an attack approach, as described above, illustrates the first. W & B's first example of tactical deception illustrates the second. A baboon being chased aggressively by another suddenly adopts the alert scanning posture normally given upon the approach of a predator or neighboring group, thereby switching the context from you-against-me to us-against-them.

Teasing shows how complex deception can get. Teasing involves rapid alternation between two types of metamessages, one of which says, "This is just play," the other, "This is really serious." At each step, the metasignal of the moment reveals that the preceding metasignal was false, that is, deceptive. Although such complex forms of deception may be restricted to humans, the many examples of tactical deception compiled by W & B show how widespread the manipulation of metasignals is among primates.

As for Darwin and metacommunication, he was prescient, as usual. His descriptions of animal behavior contain many examples of metasignals, perhaps none so cogent as the following (Darwin 1896, p. 63): "When my terrier bites my hand in play, often snarling at the same time, if he bites and I say *gently, gently*, he goes on biting, but answers me by a few wags of the tail, which seems to say, 'Never mind, it is all fun. . . .' [D]ogs do thus express, and may wish to express, to other dogs and to man, that they are in a friendly state of mind."

Learning how to deceive

John D. Baldwin

Department of Sociology, University of California at Santa Barbara, Santa Barbara, Calif. 93106

Throughout their target article Whiten & Byrne (W&B) show a clear preference for cognitive models of causation; however, only in their last paragraph (3.3.4) do they deal directly with the question of causation, asking whether primatologists have any evidence that deception results from insight, innovation, "trial-and-error learning, or imitation." They point out that the question of causation may be one of "the most important issues concerning deception," but they note that their "respondents could contribute next to nothing on this question."

First, let me make it clear that I have no commitment to any given theory of the causation of deception. Different types of deceptive acts may be influenced by various combinations of evolutionary and ecological causes, operant conditioning and observational learning (which W&B call "trial-and-error learning" and "imitation"), and cognitive or insightful processes. Given the preliminary nature of the research on deception, it may be premature to prejudge the relative importance of the numerous relevant causes. Although W&B clearly favor cognitive theories, it might be wiser to use Chamberlin's (1965) method of multiple working hypotheses – eschewing favorite hypotheses and seeking data on all relevant hypotheses so as to evaluate each one fairly with the minimum of bias.

W&B show a willingness to incorporate evolutionary and ecological hypotheses (sect. 1 and 3.1) into their theory, but they assiduously avoid conditioning and learning, flippantly dismissing them as "mindless behaviorism" (sect. 1 para. 6; sect. 2.42, para. 3). There is not a single serious attempt to evaluate the merits of learning theory for the study of deceptive behavior. In rejecting "mindless behaviorism," they have dismissed the huge literature on comparative psychology, conditioning, and learning, with data on detour learning, problem solving, generalization, learning sets, creativity, and so forth – which may be relevant to deception.

Since W&B are willing to deal with evolutionary and ecological causes, I will focus on their failure to draw upon learning theory. Lloyd Morgan's canon (Morgan 1894) cautions us about interpreting behavior in terms of "a higher psychical faculty if it can be interpreted as the outcome of the exercise of one that stands lower in the psychological scale." If learning theory is adequate for explaining certain types of deceptive behavior, it may be unwise for W&B to generate only cognitive theories for the behavior. Since there is considerable evidence for the importance of both operant conditioning and observational learning in primate behavior (Baldwin & Baldwin 1981), some forms of deception may be based on learning.

Operant conditioning could begin when one individual's behavior accidentally deceives another; W&B do admit that some acts of deception may be accidental or coincidental (e.g., sect. 3.3.1). Any accidental deceptive act that was reinforced – by access to food, water, sexual interaction, grooming, and so forth, or by escape from an aversive experience – would become more likely to be emitted in the future. The phenomenon of generalization helps explain how a deceptive act learned in one situation might be emitted under different conditions later. After several repetitions, more sophisticated deceptions might emerge as the behavior was shaped into more complex and subtle forms. Since such learning could begin in infancy – for example, during social play – an individual might learn relatively complex deceptive skills by adulthood.

Although there is not enough space here to construct operant hypotheses for every behavioral class W&B describe, testable and falsifiable operant hypotheses can be generated for all those classes. For example, concealment (A1, A2), not looking (A3), distracting by looking away or leading away (B1, B2, B3), and other deceptions often involve avoidance, response inhibition, or slowed movements; all these patterns are common results of punishment and negative reinforcement. If an animal has an aversive experience after revealing its presence or showing that it possesses a prized piece of food, the showy behavior is likely to become inhibited. Strong punishment might lead to rapid learning of quieter, more surreptitious responses. There is, of course, mediation by the central nervous system (as in the learning of all species), but describing this mediation as "mental representations" tells us little about the exact nature of the mediational processes. Instead of speculating about the mental representations the animal might use, it may be more fruitful (for analyzing the learning process and predicting future responses) to describe the types of behaviors that were punished, the contingencies of punishment, the context in which punishment occurred, and the social cues that become discriminative stimuli for future responses.

Observational learning requires more cognitive mediation than does operant conditioning (Bandura 1986). There is a substantial literature showing that many primates can learn from observing others (Baldwin & Baldwin 1981), although the nature of their cognitive representations is difficult to infer. It is possible that many if not all of the forms of deception described by W&B could be learned by observation; if the imitation was adequately skillful, it might be rewarded and reinforced in the observer's behavior repertoire.

In order to evaluate the learning hypotheses, scientists will need to collect data on histories of reinforcement, punishment, observational learning, and related context events. Although W&B fear that such data may produce the "frustrating side effect" of supporting "conditioning hypotheses" (sect. 3.3, para. 2), these data are crucial for evaluating their cognitive hypotheses: If it can be shown that the principles of operant conditioning and observational learning are *not* adequate to explain deception, then evolutionary and cognitive hypotheses become more credible. Since W&B are clearly concerned with evaluating the role of cognition in deception, they may make more progress by collecting serious data on learning than by dismissing it as "mindless behaviorism."

Thoughts about thoughts

Jonathan Bennett
Philosophy Department, Syracuse University, Syracuse, N.Y. 13244

Do any nonhuman animals have thoughts? If so, do any have thoughts about thoughts? At first sight it looks promising to try to get at the second question through cases of deception: we may find that one animal (Agent) is motivated by a desire to produce an erroneous or ignorant state of mind in another animal (call her Patience), which implies that Agent mentally represents Patience's state of mind to himself. Before getting into the details, let me lay out the groundwork in my own way.

In all the cases we have to consider, the upshot of Agent's conduct that is relevant to his desires is some behavior on the part of Patience. We aren't going to have evidence that he sought to alter her beliefs out of basic malice (or goodwill), wanting her to get a false (or true) belief just for its own sake. The behavior of Patience's that ministers to Agent's wants may be negative – it may consist in her *not* interfereing, or scratching him, or the like – but that is behavior too, and I shall speak of it in the language of "doing." In all our cases, then, Agent does A, Patience does P, which is advantageous to Agent, and we are satisfied that this is not a mere lucky coincidence.

Two questions: (1) Did Agent do A intentionally, acting under the guidance of some thought of what the upshot would be? If so, then: (2) was Agent's intention *just* that Patience should do P, or did he reckon on affecting her conduct by affecting her mental state? *We* may be sure that the only route from his conduct to hers is through her mental state, but the question is: Was *he* relying on that route's being followed?

Let us start with question (1). When we say that Agent did A intending to bring about result R, or because he thought that doing A would bring about R, this diagnosis is always *threatened from below* by the possibility that Agent did A as an instance of a drill, a pattern of stimulus and response: Agent acts in circumstances of physical kind K_C, and A is of a physical kind K_A, and Agent has found that whenever he is in K_C circumstances if he performs a K_A action R happens. If that is the case, Agent may have the thoughtlessly mechanical habit of performing a K_A in K_C circumstances whenever he wants R. How can this challenge from below be fended off?

One might answer: "Well, if Agent's action A and circumstances C do not belong to any kinds K and C such that he has found in the past that in C circumstances K actions lead to R, his doing A on this occasion can't be something he does as a matter of a drill that has been inculcated in him by his past experience." A few decades ago, that answer was given by psychologists who thought they had a viable concept of animal "insight" that could be explained in terms of radically unprecedented behavior. But that was all a muddle. If the connection between A and R is not *somehow* attested to in Agent's past experience, his doing A in order to get R on the present occasion becomes not insightful but merely lucky or else miraculous. For a postmorten on the "insight" muddle see Bennett (1964).

The right way to meet the challenge from below is not to find behavior that doesn't instantiate a pattern, but rather to find behavior that falls into a *teleological* pattern and into no *one* stimulus–response pattern: that is, a kind of result that Agent often brings about by movements of many different kinds, on the basis of many physically different clues that the result is achievable. This approach takes us away from "when Agent gets sensory input from a K_C environment he makes movements of physical kind K_A" toward something more like "when Agent has evidence that R can be achieved he does whatever will produce R." Of course it's much more complicated than that, but that outlines what is chiefly needed.

So the conclusion that Agent is acting intentionally – that is, behaving as he does because of what he thinks and wants – does not conflict with the need for pattern, regularity, repetition, so long as the patterns are not stimulus–response ones but rather are teleological in the way I have explained.

Now, suppose we are satisfied that much of Agent's behavior is intentional, including some in which he intends to modify the behavior of Patience. We want to know whether his belief that by doing A he will get Patience to do P is ever based on his belief that by doing A he will affect her mental state in a certain way.

The evidence that Agent is a "psychologist," as Whiten & Byrne (W&B) put it, goes like this: Agent believes something of the form "If I do A, Patience will do P," and we want to know why he connects his doing A with her doing P. If we can't explain this better, that is, more economically, than by crediting him with believing (1) that if he does A she will go into mental state M, and (2) that if she goes into mental state M she will do P, then we have a case for attributing those two beliefs to Agent and thus crediting him with thoughts about Patience's mental state.

To be fully entitled to attribute beliefs (1) and (2), we would need evidence that Agent has had opportunities to learn that those two are true. That is a complex matter I don't fully understand; to sort it out, we would need to understand how Agent's experience of his own mind relates to his beliefs about other minds. I shall restrict myself to the more immediate question of challenges from below – that is, of what would undermine the attribution to Agent of beliefs (1) and (2) even if there were no problems about learning.

The immediate threat is that Agent can be understood to have connected his doing A with Patience's doing P in some manner that doesn't run through Patience's psyche. That will be the case if A is of some physical kind K_A, and P is of a physical kind K_P, such that Agent's experience has accustomed him to its being the case that when he does something of kind K_A Patience does something of kind K_P. If K_P really is a physical kind, and doesn't have to be marked out in terms of psychological underlay ("movement that indicates her lack of interest," "movement that she wouldn't make if she were afraid"), Patience's mind is banished from Agent's scenario and the challenge from below has succeeded.

From this I conclude that most of the anecdotes W&B have collected are at best weak evidence that Agent is a psychologist.

The "hiding from view" cases are impressive only to the extent that in them Agent undergoes some quite complex maneuvering to keep something out of Patience's view. That is indeed evidence of "intentionalness," acting toward a foreseen outcome. However, it doesn't constitute evidence that Agent has thoughts about Patience's mental state unless there is pressure to suppose that the outcome, as represented in Agent's mind, is some state of Patience's mind. That pressure is weak. It seems possible, even plausible, to suppose that many animals at various levels have a *physicalistic* notion of line of sight, based on proximity and absence of intervening objects. Agent's grasp of the advantages of keeping something out of Patience's line of sight probably doesn't require him to operate as a psychologist any more than does his operating to keep downwind of his prey.

Those auditory examples in which the deceptive behavior consists in keeping quiet are even weaker as evidence of thoughts about mental states. Agent needs only to connect his silence with Patient's noninterference, and that he can presumably do by simple induction. *Keeping quiet* is an intrinsic, physical kind of behavior; it lacks the complexity of some of the visual examples, and is therefore less good as evidence that these cases involve intentionalness at all, let alone intentions to produce false beliefs. *Not interfering* is not intrinsic, because it means "not behaving in a manner that stops me from getting what I want"; but that doesn't help much. It doesn't even seem to involve Agent's thinking about Patience's thoughts, and it's not especially impressive in any other way. Plenty of fairly low-level nonhuman behavior can't be understood unless the animal can recognize external events as threatening, unwelcome, interfering, and the like. A sense of how external events relate – whether as conducive or threatening – to one's own desires is required for any kind of cognitive mentality.

In those cases, then, all Agent needs to have learned is that in certain familiar kinds of situations his silence is a means to Patience's noninterference; and there is really nothing left of the case for thinking that Agent is a psychologist in these situations.

Similarly with distraction by looking away: Agent needs only to know that he and his kind tend to look in directions in which others look, and don't continue with attacks when they are looking off in another direction. That challenge from below presupposes that Agent has a grasp of "looking in direction D" as a physical kind of behavior, marked off by posture, direction in which eyes are pointing, eyes open, and so forth, and not in terms of anything mentalistic. This – which could also be used to amplify the line-of-sight notion mentioned above – seems to be a modest assumption that is well supported by the data. (What it may imply is: Agent knows that in his community when one looks in a particular direction, so do others who see him do so; this knowledge is a conjunction of two bits of information: one about what happens when *he* looks in a given direction, and the other about what happens when *others* do so. If his thought about where others are looking is essentially a thought about posture and such, then we mustn't assume that he can simply generalize from the consequences of their looking in a given direction to the consequences of his doing so. Whether that is so depends on how Agent's experience of his own body relates to his perceptions of the bodies of others.)

Those remarks apply, mutatis mutandis, to the "inhibition of attending" cases as well. W&B themselves notice the structural similarity between these two kinds of cases.

The reported cases of distraction by leading away don't create any case for regarding Agent as a psychologist, so far as I can see. Even if the leading away is deliberate, and is intended to get Patience out of the way, it could be based on a grasp of "Where I go [smacking my lips, etc.], she goes," with no thought about Patience's state of mind. W&B point to the possibility that Agent is sensitive to Patience's level of attention: If she is not attending to him, she won't be drawn away by him; if she continues attending to him, she will return when he does. There could be (though I gather that there isn't yet) evidence that Agent's behavior in this general category reflects a sensitivity to those differences in Patience. That might add to the plausibility of a "thought about thought" diagnosis, but it might not. It would fail if the relevant notion of *attending* could be well understood in physicalistic terms, along the lines of my suggested account of "looking in direction D."

Similar remarks apply to the cases of distraction through intimate behavior, and I think they can be extended to the various kinds of deception W&B present in sections 2.5.3–2.5.5.

Having described Whiten & Byrne's problem situation in my own way, and expressed doubts about how far they have got with it, I want to add that I admire their grasp of what their problems are and of what would solve them. This is a useful and interesting paper, and I am glad to have a chance to try to push it further in its right direction.

Metaphor, cognitive belief, and science

Irwin S. Bernstein

Department of Psychology, University of Georgia, Athens, Ga. 30602

In 1880, W. Lauder Lindsay published *Mind in the Lower Animals.* He had collected written reports about the behavior of animals by people he considered reliable, from publications he considered dedicated to accurate reporting. He did not comment on single anecdotes but rather searched for repeated patterns of behavior. Behavior was classified according to its function or consequences and he carefully reported the behavior and the consequence. He concluded that animals committed suicide and engaged in criminal activities which could be independently verified by the objections and outrage of their peers. He assumed that the consequences of an animal's behavior were as obvious to the animal at the time the behavior was performed as they were to the observer, working from hindsight.

The next twenty years could be described as the era of anecdotal evidence during which rich interpretation based on anthropomorphizing dominated the literature. Lloyd Morgan's canon and the "law of parsimony" prevailed in the early twentieth century. Thorndike's cats, confined to a puzzle box, could do little but thrash about until the escape treadle was accidently stepped on. The "law of effect" was inevitable. Gestalt psychologists tried to return attention to context, the "umwelt," but many extreme behaviorists resisted. A small number (often referred to as "Skinnerians," but more extreme than Skinner) persist today. A total denial of interest in cognitive processes is no more constructive than the "good old days" of anthropomorphic mentalizing.

If some laboratory experiments fail and some natural observations are flights of fancy, we are not doomed to ignorance. It is indeed disturbing to find Whiten & Byrne (W&B) dismissing studies in which an independent variable is manipulated. To suggest that deliberate manipulations make results suspect is to dismiss most of science without examination; "artificial" seems to be used in a pejorative sense. One cannot do science with independent variables held constant or allowed to vary without measurement. Astronomy did not progress by only observing dependent variables. It was the measurement and correlation of both that led to progress. Relying on anecdotes, no matter how numerous, fails to specify any value of the independent variable, or the intentional state of an animal. The plural of "anecdote" is not "data." Premack and Woodruff (1978) and Menzel (1974) have demonstrated that deception and deceit can be subjected to controlled study. If only "natural" behavior is acceptable, then even de Waal's (1982; 1986) work would have to be rejected, since the chimpanzees at Arnheim were as captive as Hediger's zoo animals (sect. 2.1). Rejecting available data and dealing only with replies to a questionnaire abandons good data in favor of a methodology fraught with peril.

If I send out a questionnaire asking people to report any dream which was followed by an accident to the person in the dream, I will receive many replies. Respondents will be self-selected; doubters would probably respond only to scold me. What is the null hypothesis? How often should I expect these dreams to occur by chance? What are the appropriate controls? Do the answers to these questions change if the questionnaire is about animal deception?

Questionnaires are suitable instruments for studying beliefs, values, and attitudes, but they are a poor way to collect objective data on observable phenomena. Students of the history of psychology remember the period of introspectionism. A person has access to only one mind and may scrutinize its operations carefully; questionnaires often only solicit information on such operations.

In the absence of data many hypotheses are tenable. If my definitions are based on function and if I claim that these ends could be attained by animals intentionally working to achieve them, then the existence of a consequence is no evidence for the hypothesis. Logicians would call this the "error of affirming the consequent." Many means may achieve the same ends.

Turning the ignition key starts millions of engines every day, but proves nothing about people's knowledge of internal combustion engines. I adjust the thermostat in my office correctly but cannot tell you where the furnace is located or what fuel is burned.

A male monkey killing an infant may improve his own reproductive success, but no good sociobiologist claims that this male "strategy" is anything more than metaphor, or that a male monkey knows that infants also have fathers. Natural selection acts on functional consequences. Individuals respond to proximal stimuli. Intentions may be in the present but the burden of proof is on the one who hypothesizes their existence. Behavioral deception need be no more intentional than morphological deception.

Ethologists describing fixed action patterns did describe functional consequences. The broken wing display of a ground nesting bird when a fox approaches certainly functions to distract the fox and protect the nest. The bird does not have to read the fox's mind or intend to deceive the fox into thinking that she is injured and therefore easy prey. Noting that the consequence of behavior elegantly serves a function is not proof of planning or intentionality. Any opportunistic behavior can be seen as conscious planning. Bad "psychologizing," like bad sociobiology, hinders rather than furthers our understanding.

Why would a chimpanzee (sect. 3.2) hide behind a tree until a subordinate reopened the banana box and then displace him, rather than simply opening the banana box that he suspected was available? Why call every example of redirection and displacement evidence of intentional deception? An instrumental act that works can be learned, or established by natural selection.

Animals respond to stimuli. A failure to respond can mean that the observer failed to identify correctly the stimuli responsible for the act, or failed to specify the context in which the stimuli must occur to be effective. At any moment, individuals are *not* doing thousands of things. When your expectations are not fulfilled, it does not mean that the response you expected was deliberately suppressed. A "not response" is extremely difficult to measure.

The use of terminology like "mind reading," "fall guy," "dupe," and "creating an image" brings to mind rich connotations that far exceed what is necessary to deal with the phenomena under consideration. Operational definitions will not rid us of this excess baggage. I note that the second use of "mind reading" in section 2.4 occurs without cautionary quotation marks. Will we soon be writing of animal telepathy?

What kind of statistical testing would allow one to conclude that nine reports for chimpanzees and two for gorillas, based on an unspecified data set and unspecified hours of observation, suggest a taxonomic difference? Intuition and subjective processes seem to be assuming precedence as "evidence." Similarly consensus is not "evidence" of a hypothesis.

What I find particularly distressing is that W&B acknowledge many of the issues I have raised, but fail to address them. They seem to feel that acknowledging potential criticisms defuses them. Instead of calling for carefully designed and controlled research they appear to reject such research in favor of "observations."

It is just as wrong to say that all behavior is adaptive as to say that all behavior is intentional or that all outcomes were deliberately sought. Likewise it would be just as foolhardy to say that no behavior is adaptive, or subject to natural selection, as it would be to say that no behavior was due to intentionality and an awareness of consequences. What we need are not "suggestions," but tests of hypotheses with carefully defined terms and clear measures of independent and dependent variables. A little less inspired speculation and a little more scientific evidence. It is amazing how data can restrict the range of speculation.

Anecdotes and critical anthropomorphism

Gordon M. Burghardt

Departments of Psychology and Zoology, Graduate Program in Ethology, University of Tennessee, Knoxville, Tenn. 37996

Whiten & Byrne (W&B) are courageous in their espousal of the careful anecdote as an important, even essential, aspect of research into animal behavior. They have carefully mined the literature and arranged their gold in a complex typology across more than a dozen deceptive tactics. Here I will call attention to some relevant precedents. Are we in danger of reentering past deadends?

W&B cite 56 references. Of these, 40 (71%) are from the 80's, 10 (18%) are from the 70's, 5 (9%) date from the 50's and 60's while only one is older – Köhler's 1925 classic book on ape mentality. Certainly W&B are aware of the importance placed on anecdotes for hundreds of years; Romanes (1883) was more careful than many of his predecessors. Lloyd Morgan (1894), who with his "canon" supposedly brought down the anecdotal method, actually called for more careful, critical use of anecdotes and his own books abound in them. Yet, as with the present authors, he called for experimental data to decide critical issues, as does Griffin (e.g., 1978) in his calls for more open-minded consideration of mental awareness in animals from bees to chimps.

C. O. Whitman (1899) was unrelenting in his criticism of anecdotes per se and viewed them as shortcuts used to forego careful painstaking research. Margaret F. Washburn (1908) was more sympathetic and gave a most succinct analysis of anecdotes in her mentalistic but clear-headed treatise. She listed five major objections to the anecdotal method: "It is safe to say that this method of collecting information always labors under at least one, and frequently under several, of the following disadvantages" (1908, p. 5): Do these objections apply to the evidence presented by W&B or have they been effectively superceded?

1. "The observer is not scientifically trained to distinguish what he sees from what he infers." *Comment* – This problem seems minimal in the reports here, although it is not clear that respect for a clean distinction between description and interpretation is always maintained.

2. "He is not intimately acquainted with the habits of the species to which the animal belongs." *Comment* – Since the anecdotes here are by contemporary leaders in the study of primate behavior, let's dismiss this objection.

3. "He is not acquainted with the past experience of the individual animal concerned." *Comment* – This may apply to some but certainly not all of the present anecdotal evidence. The question really is *how much* of the past experience is known to the observer and *how much* is it necessary to know?

4. "He has a personal affection for the animal concerned, and a desire to show its superior intelligence." *Comment* – Perhaps "intelligence" is not the best word for the present cases but it is rightfully noted that many ethologists, myself included, get attached to individuals as well as species and often want to show what they can do. Certainly this can lead to a lowering of critical lenses, especially when a race is on to show who has uncovered what mentalistic (read humanlike) trait in a nonhuman first.

5. "He has the desire, common to all humanity, to tell a good story." *Comment* – All the submitters of anecdotes were human.

From this we can see that even when anecdotes are submitted for our scrutiny by trained observers intimately knowledgeable about a species and even the individuals involved, two of Washburn's five disadvantages are still dangers, although not necessarily applicable in every instance.

Washburn (1908, p. 8) also quotes Thorndike to address the "reporter bias" problem that W&B do consider:

> Dogs get lost hundreds of times and no one ever notices it or sends an account of it to a scientific magazine. But let one find his way from Brooklyn to Yonkers and the fact immediately becomes a circulating anecdote. Thousands of cats on thousands of occasions sit helplessly yowling, and no one takes thought of it or writes to his friend, the professor; but let one cat claw at the knob of a door supposedly as a signal to be let out, and straightway this cat becomes the representative of the cat-mind in all the books.

W&B similarly relied on anecdotes from farflung correspondents simply because the phenomena are rare, both in captivity and in nature, and not sufficiently repeatable or predictable for careful study. Phenomena such as infanticide also went unrecognized for years because of their rarity in the life of an individual, although infanticide turns out to have a profound influence on social dynamics, genetic structure, and so on. As Struhsaker and Leland (1985) have shown, infanticide is also hard to observe and its occurrence may necessitate considerable inference (e.g., circumstantial evidence), but it is at least in principle an objective act.

W&B are aware of this problem and acknowledge that the anecdotal method can only be a "jumping off point for more systematic work." This is an attitude similar to that behind "critical anthropomorphism" (Burghardt 1985) in the analysis of mental processes in animals. But does W&B's call for multiple records really counter the problems inherent in interpreting any "*single* observation"? How much problematic data adds up to one conclusive bit of evidence? Are we ultimately limited by authority and subjective analogical inference? Discouragingly, W&B end on a plaintive note and do not suggest any way to study the phenomena in a predictive or more publicly verifiable manner. (Videotapes, even if gathered, do not solve the problem of selectivity.)

What can we do? Harvey Carr's (1927) suggestion that any index of mind in animals be based on scientific knowledge of comparable phenomena in humans still has not been tried (Burghardt 1985). In this light, the complete absence of any consideration by W&B of deception in humans, especially in infants and toddlers, is remarkable. This is both a problem and a sign of the parochialism of too much primatology today; in this case human primates are ignored as well as nonprimates. My twin daughters engaged in behavior readily classified in several of W&B's subclasses in their second year, and I know much more about them and their past than do the authors of the subjects in the examples cited here. Should we encourage psychologists to flood the literature with anecdotes about their kids? Hardly. But to ignore the chance to join efforts with the largest group of scientists studying any primate is, to be blunt, foolish.

Classification of deceptive behavior according to levels of cognitive complexity

Suzanne Chevalier-Skolnikoff*

Human Interaction Laboratory, Department of Psychiatry, University of California at San Francisco, San Francisco, Calif. 94143

I would like to congratulate the authors on an unusually stimulating and thought-provoking paper. Besides their careful analysis of the material, Whiten & Byrne's (W&B's) inquiring tone is refreshing and conducive to further inquiry. Their collation of a large catalogue of deceptive behavioral incidents is also a significant contribution; previous studies on deception have been limited to very small samples, which has hampered research on deception.

I will direct my commentary to W&B's question about whether the causation and origin of deceptive behavior is worth pursuing. My response is an emphatic "yes," although I admit that researching this area will not be easy.

First I will attempt a tentative functional and causal classification of the examples of deceptive behavior that W&B have presented in their target article. Then I will suggest some approaches for investigating this problem, and finally I will propose a cognitive taxonomy of deception.

In the following classification I will try to formulate a taxonomy of deception that reflects mental complexity and distinguishes different psychological demands on the AGENT, who is the most significant actor, since he is the deceiver. To do this I will use a framework adapted from Piaget's (1952) model of human sensorimotor development as a guideline for analysis. This model consists of six stages that emerge sequentially during the first two years of life in human infants. They make up a hierarchy of qualitatively different developmental levels reflecting increasingly complex cognitive functioning. The model provides a multilevel framework of which the last four stages (stages 3–6) seem to be applicable to analyzing the ontogenetic and evolutionary development of deception. These four levels are:

Stage 3: *Secondary circular reaction.* Attempts to reproduce environmental events first discovered by chance, using single behaviors directed toward a single object or person (i.e., through operant conditioning).

Stage 4: *Coordinations.* Establishing relationships between two objects or persons, showing intentionality, and attributing cause of change to others.

Stage 5: *Tertiary circular reaction.* Repeating behavior with variation to explore the potentials of objects and persons through experimentation. Learning through experimentation to use one object, or person, to effect a change in another, often to solve a problem.

Stage 6: *Insight.* Solving problems correctly on the first try, without experimentation, through mental representation, forming mental combinations or innovations in which the interconnections are based on the properties of the variables themselves, rather than the frequency with which they follow one another or cooccur. This is the stage of truly "creative intelligence."

I have categorized the deceptive incidents cited by W&B according to the behavioral features evident from the contextual information presented in the examples that are characteristic of the different levels of this series (Parker 1977; and see Chevalier-Skolnikoff 1982, p. 309; 1983, pp. 550–51, for categorizing narrative data into sensorimotor stages by means of behavioral features). One cannot be certain that this classification of anecdotal material is correct, however, since a specific functional behavior can often be achieved ontogenetically and determined cognitively by different means (Riley & Trabasso 1974; Steinberg & Powell 1983). For instance, a specific deceptive maneuver could first be performed by chance and then incorporated into the AGENT's behavioral repertoire through operant conditioning (a stage 3 mechanism), or it could be learned through deliberate trial-and-error experimentation aimed at achieving the goal of deceiving (a stage 5 mechanism), or it might be achieved through insightful (stage 6) problem solving. Even though the behavior may be functionally and descriptively the same in the three cases, in terms of causal determinants it would be different in each case. Furthermore, the behavior may represent different levels of understanding depending on its conception. For instance, "deception" might only be understood empirically in the first (stage 3) case, whereas only in the

*Mailing Address: Suzanne Chevalier-Skolnikoff, 205 Edgewood, San Francisco, Calif. 94117.

Table 1 (Chevalier-Skolnikoff). *A functional and cognitive classification of examples of deceptive behavior cited in Whiten & Byrne*

Functional categories	Tentative assignment of cognitive stages: 3/4	5	6	ambiguous
A1	**3**		**26***, 66*, 67*	
A2	**13**, **20**, 81			
A3	93, 89, 86, 91, **7**, **64**, **6**, 78, 80		69, 24a*, 24b*	2
B1		**74**, 82a, **25**	82b, **73**, 22	
B2			**27**, 103, **88**	
B3			87, 95, 96, 77	
B4	**9**		101	**63**, **62**, **61**, **28**, 10, 11
C1	**5**	1	65, 99, 98	
C2	68a		68b, 102, 104	**4**
D1		**29**		**72**, **70**, **71**
D2		**21**		
D3	**17**	**14**, **15**, **16**		
E				**18**, **19**, **75**, **76**, **83**

*These incidents also incorporate an understanding of object permanence.
Note: Incidents performed by monkeys are in boldface.

third (stage 6) case might the AGENT understand his actions in terms of representation, and only in this case would the AGENT truly understand what the TARGET thinks or understands. Only in the third case would the AGENT be a "mind-reader" and a "psychologist." For this reason the following classification is provisional (see Table 1):

A1: *Hiding from view.* The examples in this category seem to be determined by two different levels of cognitive functioning. For instance, example No. 3, Altmann's yellow baboon keeping food out of sight by turning its back, may merely represent voluntarily avoiding an interaction – a stage 4 mechanism based on intentionality. Does the baboon also understand through mental representation that the TARGET cannot see the food? There is little evidence to support or refute this possibility. Examples No. 26, Kummer's hamadryas baboon carefully moving behind a rock, and No. 67, de Waal's male chimpanzee turning his back to the alpha male as he monitored the condition of his penis until it was no longer erect, are much more convincingly determined by an insightful understanding of another's view, as well as the concept of the hidden object (a concept I will not be able to explore fully here).

A2: *Acoustic concealment.* Most of these examples seem to include intentionality as animals inhibit behavior that is likely to elicit undesirable results, a stage 4 mechanism. It cannot be ruled out that the behavior is insightfully based, but there is no clear evidence that it is. Furthermore, no "dishonest" messages were sent in these incidents. Example No. 20 (Whiten & Byrne 1986), Whitehead's howlers not vocalizing as they move toward another group, may not even incorporate inhibition – the monkeys may merely have stopped vocalizing as they traveled.

A3: *Inhibition of attending.* Many of these examples may also be based merely on stage 4 inhibition (e.g., Nos. 6, 80, 78, and even 86, Lawick-Goodall's chimpanzee, Figan, moving away from the bananas to the other side of the tent [Whiten & Byrne 1986]). Furthermore, some of these examples (e.g., No. 6, Altmann's yellow baboon mothers ignoring their infants' weaning tantrums) are not deceptive because no "dishonest" signals were sent. Other examples, for instance No. 24a and b (Whiten & Byrne 1986) in which Plooij's chimpanzee closed the banana box and walked away when a second chimpanzee approached – and the second, manifesting a counterdeceptive strategy, left and hid behind a tree, watching the first return to the box to eat the bananas – seem to include insightful (stage 6) problem solving as well as an understanding of object permanence.

B1: *Distraction by looking away.* Two of these examples (Nos. 74 and 25) involve "redirected aggression," an ethological concept. In these incidents subordinates are being theatened by more dominant animals and they turn aggressively on others. Since redirected aggression involves the use of one individual to effect a change in the behavior of another, these behaviors are classified as stage 5. Other examples (Nos. 73 and 22) appear to be based on insight, as animals in similarly awkward situations seem to solve the problem insightfully by sending "dishonest" messages about imaginary threats. However, we know little about the neurophysiology or the evolution of redirected aggression. To what extent might this behavior be canalized by natural selection? (It is prominent in some species and not in others.) Is it possible that the threats at "nothing at all" represent some kind of overflow activity? If this were the case, could such a mechanism be an evolutionary precursor to voluntary deception?

B2: *Distraction by looking away with linked vocalization,* and B3: *Distraction by leading away.* These are both represented by examples that do seem to involve stage 6 insight, as, for instance, in incident No. 87, when Lawick-Goodall's chimpanzee Figan led the other chimpanzees away from the banana supply and then later returned by himself to eat more of them.

B4: *Distraction with intimate behavior* is represented by a variety of incidents that apparently involve different levels of cognitive functioning. Some of these examples are very difficult to categorize cognitively according to the available information. For instance, No. 62, Strum's female olive baboon who edged up to the male possessor of an antelope carcass and groomed him until he relaxed and lolled back, and then snatched the carcass and ran: Was the female just getting as close to the meat as she could and then did she suddenly see that she had a chance to snatch the carcass, or was the whole event insightfully premeditated?

C1 and C2: *Presenting neutral and affiliative images* are also comprised of incidents that incorporate diverse levels of cognitive functioning.

D1, D2, and D3: *Manipulation of target using social tool* is

Table 2 (Chevalier-Skolnikoff). *A cognitive classification of examples of deceptive behavior cited by Whiten & Byrne*

Behavioral categories	Tentative assignment of cognitive stages				
	3/4	4	5	6	ambiguous
Emotional display	**17**				
Inhibition (conditioned response)	81, **64**, **9**, 93, 89, 91				
Inhibition (voluntary)		**3**, **13**, 86, **7**, **6**, 78, 80, **5**, 68a			
Displacement activity			82a, 1		**2**
Redirected agression			**74**, **25**, **14** **15**, **16**		**19**, **75**, **76**, **18**, **83**
Solicited threat			**29**		**75**, **76**, **18**, **83**
Social buffer			**21**		
Inhibition (insightful)				67, 69	
Sending false messages to attain goal				**26**, 66, **24a** **24b**, 82b, **73**, **22**, **27**, 103, **88**, 87, 95, 96, **77**, 101, 65, 99, 68b, 98, 102, **104**	4, **62**, **63**, **61**, **28**, **10** **11**, **71**, **72**, **70**

Note: Incidents performed by monkeys are in boldface.

especially interesting because, as W&B have pointed out, these triadic interactions superficially appear to be cognitively the most complex form of deception. However, they probably incorporate only an intermediate, stage 5 level of cognitive complexity as the AGENT uses another animal as a social "tool" to effect a change in the behavior of another in order to solve a problem – all behavioral features of the fifth stage. If these tactics are learned through experimentation or by imitative matching (there is not enough space to explore fully the role of imitation in this commentary), they are determined by stage 5 mechanisms. If they are acquired through insightful problem solving or through delayed imitation, they involve stage 6 functioning.

So, how is one to determine whether this tentative cognitive classification is correct, and how the ambiguous incidents are to be classified? I believe there are four ways to evaluate the cognitive basis of deceptive behavior more clearly, in addition to using some of the methods proposed by W&B in sections 3.3 and 3.3.1. The first way is to do ontogenetic studies on large numbers of animals observed intensively, evaluating when and during which sensorimotor stages the various types of deceptive behavior appear. I have attempted to do this on a relatively small sample of primates and found that true deception presumably based on insight first appeared in apes after they had entered the sixth sensorimotor stage. Consequently, the behavior appeared to incorporate stage 6 behavioral features and also emerged after subjects had demonstrated stage 6 functioning in other domains (Chevalier-Skolnikoff 1986). A second way is quantitative, to examine frequencies of different types of deceptive behavior manifested by subjects of various species at different ages. In the absence of ontogenetic studies, frequency data can help to establish which species show the behavior most often and at what ages. This information is useful in making interpretations, for an ambiguous behavior is most likely to be determined by the cognitive mechanisms most often used by the subject's species and age group. The third method is to examine counter deception, as W&B have done. Counter-deceptive tactics provide strong evidence that one has not overinterpreted insightful deception in that population. The fourth avenue is to perform experiments like those of Woodruff and Premack (1979) on samples of appropriate ages, in order to understand the ontogency of deception, and on different species to study the evolutionary aspect.

Ontogenetic studies from a cognitive perspective might reveal a developmental sequence of deceptive behavior similar to that presented in Table 2.

Deception and explanatory economy

Arthur C. Danto
Department of Philosophy, Columbia University, New York, N.Y. 10025

The subadult baboon, ME, can be supposed, by feint, to have deceived the posse of adult male baboons, bent upon a disciplinary intervention, only if ME himself was not deceived (sect. 2.5.2, No. 73). For if ME, believing that he detected interlopers – whether alien baboons or baboon predators – stands on his hind legs and peers into the threatening distance, then his would-be disciplinarians are not duped but simply misled; baboons are wired to respond to such signals by emulating his posture and peering in the same general direction as he. That no interlopers are revealed through the field observer's 10×40 binoculars leaves quite undecided whether ME was not deceived by signs that in the event were false, but quite as insistent as true ones would have been – or whether ME pretended such a response, the effect on his elders and betters being the same. Who knows what scents or rustles might not have signaled alarums to baboon sensibility? How do we know in such a case that we are not being deceived, pretense being as indiscriminable from false perception as true interloper signals are from false ones? ME's human observers would not be deceiving us, but instead simply leading us astray by interpreting as pretense what might have been an epistemologically unfortunate episode in the life of ME. The world of the field observer and of the baboon is a perceptually treacherous place.

Our authors (Whiten & Bynre, W&B) seek to brake a slide into mindless behaviorism by saying that a psychological representation is more "economical" than a merely behavioral one. But behavior underdetermines which of a pair of competing psychological descriptions holds in the case of ME, as indeed in most of the examples W&B have laid before us. ME behaves "outwardly" the same whether he falsely believes there are interlopers or merely pretends to believe that in order to distract his menacing peers. The issue is not between psychological versus behavioral representations, but between psychological representations which behavior cannot discriminate. In particular, behavior alone will not justify an explanation in terms of deliberate ruse if the same behavior is compatible with the ascription of a false belief. We are ascribing beliefs to the animal either way, though beliefs of radically different orders. One might regard a group of baboons as a system of signal-emitting instruments in which the general welfare is a function of the reliability of the individual instruments. The signal emitted by ME is spontaneously responded to by them as a clear signal, even if ME is responding to environmental signs he has no way of knowing to be false. The striking question is whether he, or any baboon, can *play* the part of an instrument, and nothing in the scenario described would enable us to know this. In order to pretend to be perceiving something untoward in the bush, ME's posture would mimic what it would be if he were merely responding to signs of danger, and his mimetic action would have to be explained through a practical syllogism of some complexity; namely, that his aggressors will read the signal as clear, that they will give greater weight to the threat of invasion than to a mere bit of misbehavior on the part of a junior, and that their attention span is such that they will forget what brought them in the first place to the hillcrest, emitting pantgrunt calls of monitory ferociousness, thus leaving ME in peace. ME would have to have internalized the signal system of his species, and then, rising above it, used it manipulatively.

When, some decades back, philosophers of knowledge pondered the relationship of sense-datum language to the language of physical objects, there was a question of whether we can translate such terms as "apple" into expressions using solely the lexical resources of a sense-datum vocabulary. It was a standard position among optimists to say that the translation could be effected in principle, but that reasons of economy mandated continued use of the philosophically less preferred idiom. I did not believe that the sole difference between the two languages turned on parsimony, nor do I believe that behaviorism would yield the preferred idiom except for reasons of parsimony. The issue is not economy but truth. The world of sense-data is a world vastly different from the one of physical objects which may cause us to have the sense-data that serve in turn as evidence for physical objects and for the causal relationships between ourselves and them. A baboon endowed with representational powers is a creature vastly different from one that merely behaves, and the world it inhabits is in several dimensions richer than one that has no such creatures in it. Methodological scruples are not metaphysically innocent if we allow them to define reality for us.

Granting representational states on the part of baboons, we have the problem of discriminating, on the basis of a piece of behavior consistent with any of a large set of such states, each of which is inconsistent with the other, which of them explains the behavior. This is a field example of the "other minds problem," a vexation which can arise at any point when we have recourse to explanatory variables otherwise hidden from us, but in practice there must be ways of discovering whether someone is having an experience or only pretending to have one. Obviously, the chimpanzees are capable of singling out a defective instrument in their midst, as in the interesting case of Puist, who has proved treacherous (sect. 2.5.3, No. 68). But when the system of representations becomes as intricate as the one we are obliged to ascribe to ME on the hypothesis that he is feigning, verification becomes correspondingly intricate. We would have to decide whether ME can have that system of representations, and this is obviously difficult to do when our laboratory is the world and contingencies leak in at every point.

Perhaps we can say this: If feigning occurs, it must occur against a presumed background of honest conduct, in the explanation of which honest beliefs must be ascribed to the agents. We must represent the animals as representing one another's conduct in the normal case more or less as follows, if the case is an honest piece of alerting: A baboon stops, rises, and peers into a distance rendered threatening to his peers by virtue of his action. His peers read his behavior thus: He believes something suspicious is approaching. This gives them a reason to look as well. In the case of deception, the first peerer would have to pervert the normal case: he would believe that they believe that he believes something suspicious is approaching, and so on. He can cause them to misrepresent only against an implied background of representations, which themselves are representations of his representations – beliefs about the beliefs of others. Only if each member of the group has internalized the standard representations of the other members can one member use his knowledge of such representations as an instrument of duplicity. The mind of the individual would be, as it were, the state writ small. Primates could be psychologists only if sociologists as well.

What would force such an explanatory scheme upon us? We have two alternatives: either the baboons compose a system of signalers, which allows for false signals but not deliberate deception, or they compose an interrepresentational community that knows itself to be such, allowing members to appropriate this knowledge to their own ends. One explanatory scheme may be more economical than another, but we ought never to be guided by this. The question is which is true – but we might postpone having to deal with the question if ME were merely deceived by appearances.

Consider the feints dogs make in playing at fighting, where the trick is to get past an opponent's guard. This form of guile is clearly adaptive, since the dogs are acquiring prowess that will enhance their survival in Purina-free worlds. It is difficult to see that deception among primates could be adaptive in this way, in part because it seems sufficiently infrequent that we have to solicit anecdotes to see whether it occurs at all. We observe jousting in puppies all the time – we don't have to canvass dog psychologists for data. Obviously, if it does occur among primates, it gives its users advantages; however, one would then expect it to form a constant feature of common life, as primates acquire the skills they need to transact a competitive social reality. Yet this does not seem to fit the distribution of data; much as I should celebrate the discovery that primates are natural psychologists, I await a stronger case than I find here for believing that these various, entertaining descriptions are not but artifacts of the wish, on the part of the authors, that their subjects be colleagues.

Why creative intelligence is hard to find

Daniel Dennett
Center for Cognitive Studies, Tufts University, Medford, Mass. 02155

The concluding question and answer of Whiten & Byrne's (W&B's) valuable survey is worth repeating, if only to save W&B from a likely misreading: "But can anecdotes ever be more than a jumping off point for more systematic work? We propose that the answer must be 'no'. . . ." I have been dismayed to learn that my own limited defense (Dennett 1983) of the tactic of provoking "anecdotes" – generating single instances of otherwise highly improbable behaviors under controlled circumstances – has been misinterpreted by some enthusiasts as giving them license to replace tedious experimentation with the gathering of anecdotes. But as W&B stress, anecdotes are a prelude, not a substitute, for systematic observation and controlled experiments.

They describe in outline the further courses of experimental work that would shed light on the phenomena of tactical deception, and suggest that the postponed question of whether these behaviors arise as a result of "creative intelligence" may be settled by the outcome of such research. This research should certainly be pursued, and if properly conducted its results are bound to shed light on these issues, but it is worth noting in advance that no matter how clean the data are, and indeed no matter how *uniform* they are, there is a systematic instability in the phenomenon of creatively intelligent tactical deception (if it exists!) that will tend to frustrate efforts of interpretation.

To see this, consider the range of possibilities available in the generic case, stripped to its essentials. Suppose AGENT intelligently creates a deceptive tactic that is devastatingly effective on a first trial against TARGET. Will it tend to be repeated in similar circumstances? Yes; *ex hypothesi* it was intelligently created rather than a result of blind luck or sheer coincidence, so AGENT can be supposed to recognize and appreciate the effect achieved. But then there are two possible outcomes to such repetition: Either it will provoke countermeasures from TARGET (who is no dummy, and can be fooled once or twice, but will eventually catch on), or it won't (TARGET is not so smart after all). If it doesn't, then the exploitative behavior will become (and be seen to be) stereotyped, and ipso facto will be interpretable not as a sign of creative intelligence but as a useful habit whose very cleverness is diminished by our lower regard for the creative intelligence of the TARGET. If, on the other hand, TARGET attempts countermeasures, then either they will work or they won't. If they don't work, TARGET will be seen once again to be an unworthy opponent, and the deceptive behavior a mere good habit. If the countermeasures tend to work, then either AGENT notices that they do, and thereupon revises his schemes, or not. If not, then AGENT's intelligence will be put into question, whereas if AGENT does come up with a suitable revision then he will tend *not* to repeat the behavior with which we began, but rather some relatively novel successor behavior.

In other words, if both AGENT and TARGET are capable of creative intelligence, then what must ensue is either an escalating arms race of ploy and counterploy or a semistable equilibrium in which the frequency of deceptive tactics is close to "chance" (a tactic will work against a wily TARGET only if infrequently used, something a wily AGENT will understand). Escalations, however, are bound to be short-lived phenomena, punctuating relatively static periods. (If AGENTS and TARGETS are so smart, one might ask, why haven't they already discovered – and exhausted – the opportunities for escalation?) So the conditions one would predict in any community in which there is genuine creative intelligence are conditions systematically difficult to distinguish from "mere chance" fluctuations from a norm of trustworthiness. Any regularly repeated exploitative behaviors are in themselves grounds for diminishing our esteem for the creative intelligence of either AGENT or TARGET or both.

Our estimation of AGENT's cleverness is in part a function of our estimation of TARGET's cleverness. The more stupid TARGET appears, the less we will be impressed by AGENT's success. The smarter TARGET is with countermeasures, the less success AGENT will have. If AGENT persists in spite of failure, AGENT's intelligence is rendered suspect, while if AGENT largely abandons the tactic, we will not have any clear way of determining whether AGENT's initial success was dumb luck, unrecognized by AGENT, or a tactic wisely perceived by AGENT to have outlived its usefulness.

This can be made to appear paradoxical – a proof that there couldn't be any such thing as genuine creative intelligence. Once or twice is dumb luck; many times is boring habit; in between there is no stable rate that counts clearly and unequivocally as creative intelligence. If we set the threshold in this fashion we can guarantee that nothing could count as genuine creative intelligence. Using just the same move, evolutionists could prove that no history of natural selection *could* count as an unproblematic instance of adaptation; the first innovation counts as luck, while its mere preservation unenhanced counts as uncreative.

Clearly, we must adjust our presuppositions if we want to make use of such concepts as creative intelligence or adaptation. The stripped-down paradigmatic case exposes the evidential problem by leaving out all the idiosyncratic but telling details that are apt to convince us (one way or the other) in particular cases. If we see AGENT adjusting his ploys to the particularities of TARGET's counterploys, or even just reserving his ploys for those occasions in which he can detect evidence that TARGET can be caught off guard, our conviction that AGENT's infrequent attempts at deception are intelligently guided will be enhanced. But this seeing is itself a product of interpretation, and can only be supported by longitudinal observation of variegated, not repeated, behavior. There could be no surefire signs of creative intelligence observable in isolated, individual bits of behavior.

What we will always have to rely on to persuade us of the intelligence of the members of a species is the *continued* variation, enhancement, and adjustment of ploys in the face of counterploys. So it is not repeated behaviors but changing behaviors that are the sign of creative intelligence, and this can be observed only in the long term. Moreover, the long-term history of any genuinely intelligent agent will show this proclivity to make novel and appropriate advances mixed with a smattering of false starts, unlucky breaks and "bad ideas." So we should set aside the illusory hope of finding "conclusive" empirical evidence of creative intelligence, evidence that can withstand all skeptical attempts at a "demoting" reinterpretation. When we recognize that the concept of intelligence is not a neutral or entirely objective one, but rather an evaluative one, this should not surprise us.

Emotional control

Frans B. M. de Waal
Wisconsin Regional Primate Research Center, University of Wisconsin, Madison, Wis. 53715-1299

Whiten & Byrne (W&B) wonder whether chimpanzees attract scientists who are keen to find striking instances of intelligence, or whether the species itself is exceptional. The literature on cross-species communication, not treated in W&B's article, appears to confirm the overrepresentation of pongids in anecdotes of deception. Many of these reports antedate the current interest in cognitive explanations of behavior (e.g., Kellogg & Kellogg 1933; Hebb 1946; Hediger 1955).

Are pongids really special? Perhaps I can shed some light on this issue as I have extensively worked (and still do) with captive members of both the genera *Macaca* and *Pan*. As my descriptions of deception among chimpanzees attest, I have no problem attributing intentions, even devious ones, to animals (de Waal 1982). Yet, the same observer with the same attitude, looking hard for similar instances in macaques, has discovered very little. Admittedly, macaques do engage in deceptive practices such as ignoring threat signals, faking a scream normally given in response to an attack, or hiding themselves from view. Their deceptive behavior is infrequent and simple, however, compared to the pervasive distortion of information among chimpanzees. It would seem premature to decide whether this is a difference in degree or in kind between "the" monkey and "the" ape, but I am convinced that the difference cannot be entirely attributed to observer bias.

When, after six years of exposure to chimpanzees, I took up watching rhesus monkeys, my first impression was that they hardly take the time to review a situation before responding. Most of their reactions are immediate, with no room for secondary impulses. This does not mean that reflection is absent in this species, but time is an important factor in the "formulation" of intelligent responses, and chimpanzees definitely take more time than macaques. So do sloths, one might counter, but one can sometimes see a chimpanzee being pulled between conflicting impulses until a solution is found. Hence, the delay in response appears to be due to mental processing, not to a temperamental or physical inability to act swiftly, which chimpanzees can and will do when necessary.

Let me give an example of externally visible decision making. Walnut, the dominant male of the chimpanzee colony at the Fieldstation of the Yerkes Primate Center, was consorting with an oestrus female. An adolescent male, Kipper, was following both of them and would take any opportunity to approach the female, who had shown sexual interest in him on several occasions. Walnut's constant vigilance, maintained for days on end, did not leave much time for feeding. At one point, with the female asleep, he headed towards the food trough (at a distance of approximately 20 m) only to turn around midway when he saw Kipper sneaking towards her. This sequence was repeated several times, Walnut being torn between hunger and sexual possessiveness. Suddenly, the normally dignified alpha male made a wild dash to the trough, crammed as many chow pellets as he could in his mouth, hands, and feet, and hurried back just in time to separate Kipper from the female.

If the external signs of a dilemma are suppressed, its solution will come more as a surprise, and may be more effective. If Walnut had remained calm, not showing any inclination to leave till the last second, Kipper might not even have had the time to decide what to do. Self-control is such a basic prerequisite for successful social maneuvering that it needs to be part of cognitive ethology if we wish to make real progress. That is, we need to study the normal emotional life of animals to understand the modification of its expression for the sake of social manipulation.

Whereas animal awareness, mental processes, insightful problem solving, and intentionality are now openly discussed, references to feelings are conspicuously lacking in most of the literature on cognition. The mind is usually compared to a digital computer, that is, to a machine without fears, hopes, and changing adrenaline levels. In reality, emotions color perception at every level, and it is virtually impossible to draw a line between the rational and the emotional components of decision making.

Here we are especially interested in the extent to which animals withhold information about their intentions and emotions. Such withholding would be easy if they were cold machines, but many animals – and chimpanzees foremost among them – have dramatic displays of emotion that need to be suppressed for effective deceit. We may speculate that the capacity to do so evolved in a social environment in which everyone's nerves were constantly tested by bluff and provocation, while reactions were closely monitored. The term "poker face" refers to precisely such an environment. The hiding of primary impulses allows players time to think before they act, and confuses competitors to the point where they may dig their own graves. The technique is also common in the political arena. For example, the Russian veteran diplomat Andrei Gromyko was nicknamed "old stone face" for his ability to conceal his mood behind an impenetrable mask during negotiations. Similarly, Freeman (1983, p. 217) discussed the value of displays of "engaging affability" among Samoans under pressure: "when a chief is being criticized by others [i.e. by other chiefs], however severely, it is usual for him to respond, even when deeply angered, by intoning at regular intervals the words Malie! Malie! (How agreeable! How agreeable!)."

Perhaps self-possession is not the male handicap (i.e. in the form of an inability to express feelings) that some contemporary feminists consider it to be; there are probably good strategical reasons for competitors, male or female, to hide emotions from others, perhaps even from themselves (cf. Trivers 1985, on self-deception). If displays of vulnerability have adverse effects, they will be eliminated through a combination of learning and the innate ability to control the musculature of the face and other communicative body regions. The behavior of adult male chimpanzees faced with intimidation displays of rivals is strikingly similar to the above human examples. Signs of nervousness are hidden, a good mood is projected, irrelevant objects are picked up and inspected, and all this while tensions are mounting and serious escalation is in the air (de Waal 1986a). Observations of solitary games in which juvenile bonobos and gorillas make strange facial expressions suggest that voluntary muscular control in this body region is not limited to our species (Chevalier-Skolnikoff 1982; de Waal 1986b).

As this timely target article by Whiten & Byrne demonstrates, we are still a long way from conclusive verification of intelligent deception among nonhuman primates. The present collection and classification of anecdotes encourages us to design new experiments and carry out more systematic observations. The point I am making is that the emotional life of animals, and the extent to which it can be held in check, need to be an integrated part of the research.

How to break moulds

R. I. M. Dunbar
Department of Anthropology, University College London, London WC1E 6BT, England

There are just three points I wish to make in response to Whiten & Byrne's (W&B's) target article. All of them are, in a sense, contextual rather than critical, since I feel that W&B have done an excellent job, both in overcoming the natural reticence of field ethologists by persuading them to come clean on their observations of deceptive behaviour and in their analysis and classification of these examples.

The first point I wish to emphasize is that we continue to have some serious mould-breaking to do. Many of those who are interested in behaviour, from ethologists and psychologists to philosophers and social scientists, are still reluctant to abandon the behaviourist framework in which we have worked for more than half a century. They are not wrong to insist on applying Lloyd Morgan's canon rigidly; the kinds of problems they happen to be interested in often do not require a more sophisticated level of analysis than that of behaviourism.

However, in ethology, there has been a shift of interest from the simple interactions that occur between individual organisms to the relationships that animals use those interactions to mediate and serve. One of the problems faced by an animal when trying to negotiate and organise its relationships with fellow group members in a complex social system is that it cannot always do what it wants to do. Its ideal relationships are not always attainable because of the conflicting interests of other individuals. In such a situation, it is not enough for us simply to describe what an animal does – we need to be able to go beyond the world at face value to try to understand what the animal is attempting to do. This inevitably means trying to see the world from inside the animal's head.

Now, when behaviourists object to an anthropomorphic approach, what they generally have in mind is claims about how animals feel (or what philosophers now tend to refer to as the "raw feels" of experience). This is not what social ethologists are interested in however; we need to be able to get at the animal's interpretation of those raw feels – in other words, the "raw thinks" that the animal interprets "raw feels" in terms of.

Behaviourists may continue to remain skeptical of these demands, insisting that the behaviour we observe does not require a cognitive (or mentalistic) interpretation. In this context, the issue of deception seems to me to be a crucial test case, because genuine deception surely rests on the cognitive manipulation of abstract information about another individual's understanding of actions or events. Both sides can, I think, agree on this, so the only issue of any consequence remains the purely empirical one of whether or not any nonhuman organism actually shows deceptive behaviour in this strong sense. Even though profound skeptics may still remain doubtful, the material presented by W&B clearly goes a long way toward establishing the basis of a case that might yet convince even B.F. Skinner. [See special issue on the work of Skinner, *BBS* 7(4) 1983.]

The second point I wish to draw attention to arises out of this apparent conflict between traditional behaviourism and cognitive ethology. At several points in their discussion of specific examples, W&B point out that simpler behaviouristic explanations cannot be discounted on the basis of the evidence in hand. Herein, it seems to me, lies the very root of the problem. An interaction between two animals is not an isolated event – it is part of a developing relationship that has a long history and takes place within a much wider social context. Whereas one can often give a simple mechanistic interpretation of a specific interaction, it is less easy to sustain such a view once context and history are taken into account. I would suggest that the way forward lies less in trying to engineer tests that would satisfy experimental psychologists than in trying to find ways of exploiting the contextual nature of interactions. I suspect that the recent advances in discourse analysis (e.g. Gumperz 1982) might be a profitable avenue to explore.

My last point concerns the nature of hypothesis testing. Too often, scientists assume that the scientific method is synonymous with an experimental approach. It is not. The scientific method demands only that hypotheses be formulated and tested in a rigorous way. The difficulties of controlling for extraneous variables have, by historical accident, necessitated a heavy dependence on experimental manipulation. But times have progressed and we can now handle these problem variables statistically in ways that allow us to test hypotheses in a nonexperimental form. Although I do not wish to decry the value of carefully structured experiments (and especially the kinds of experiments carried out in the field by Cheney and Seyfarth 1985a), I think it is important to avoid the naive mistake of assuming that an experimental test of hypothesis is necessarily more rigorous than an observational one. There is no intrinsic reason why this should be so, for the rigour of an experiment is only as great as the experimenter's ability to identify and exclude confounding variables. An experiment can often be less rigorous than a well-constructed analysis of observational data precisely because it disrupts the natural flow of interactions: experimental manipulation by definition introduces new and uncontrolled extraneous contextual variables into a social situation.

Toward a taxonomy of mind in primates

Gordon G. Gallup, Jr.
Department of Psychology, State University of New York at Albany, Albany, N.Y. 12222

Whiten & Byrne (W&B) have provided an interesting critique and taxonomy of apparent instances of intentional deception in primates. It is clear from their target article, however, that deception is not the issue. The question posed by tactical deception in primates is whether they are capable of functioning as "natural psychologists."

W&B have chosen not to discuss my position on this topic (e.g., Gallup 1982; 1983; 1985). In my view, deception is simply one of a variety of introspectively based social strategies that can be used to infer whether an organism can use its experience to infer the experience of others. When it comes to mental processes in species other than our own it is no longer so much a question of making inferences, but one of making inferences about inferences.

I have argued, as has Humphrey (1982), that the ability to impute mental states to others presupposes self-awareness; that is, you have to be capable of reflecting on your own experience to be in a position to use it to model the probable experience of others. Accordingly, in addition to the reasons given by W&B, there is a further reason why gorillas may fail to show more convincing evidence of tactical deception. Gorillas may be incapable of becoming the object of their own attention. Indeed, based on their inability to recognize themselves in mirrors, I predicted the gorilla data years ago (Gallup 1982). To my mind, the question is not so much one of trying to reconcile the absence of deception in gorillas with its presence in chimpanzees, but to account for the puzzling performance of baboons. The absence of any evidence of deception in orangutans, who, like chimpanzees, can also correctly decipher mirrored information about themselves, is not surprsing in so much as they lead a rather solitary existence in the first place. In fact, I have even conjectured (Gallup 1983) that the reason orangutans are so reclusive may be because they have learned that other orangutans cannot be trusted!

A rather curious feature emerges from the data presented by W&B in Table 1. The number of people queried about cercopithecinae exceeds the total for all other subclasses combined. Since most of W&B's examples of deception given involve chimpanzees and baboons, it would be interesting to know how many people working with baboons were contacted. The reason this may be important is that the data for all other categories in Table 1 are not given in terms of frequencies, but only in terms of whether there was *any* evidence of deception. Since apparent instances of tactical deception may be merely coincidental, it follows that the likelihood of finding a spurious instance would increase with the number of observations made. For reasons that have been detailed elsewhere (Gallup 1985) it would also be helpful, if not essential, to document the existence of interspecific as well as intraspecific instances of tactical deception.

There are a variety of other ways in which questions like these can be answered using more rigorous and more readily replicable laboratory procedures. For example, I have suggested (Gallup 1985) that one way of dealing with an organism's capacity to function as a natural psychologist would be to give it systematic experience with different visual observations (e.g., blindfolds, opaque goggles, etc.). Having done that, one could assess its reaction to other organisms that have been comparably obstructed; for instance, how would a subordinate male baboon react to a limited quantity of highly prized food in the presence of a familiar dominant male as a function of whether the dominant animal was wearing a blindfold? This kind of strategy can be adapted to a number of situations and modalities. W&B pose the question "Is there any evidence for an AGENT acting more or less quietly according to the likelihood that a TARGET can hear him?" An obvious way to answer this question would be to teach a baboon to vocalize to get an experimenter's attention and then give it experience with auditory obstructions (e.g., ear plugs, head phones, etc.). Would it as a consequence respond differentially by adjusting the intensity of its vocal output as a function of whether the experimenter's auditory capacity had been obstructed or not? My guess is that it would not.

Whereas deception is an interesting topic in its own right, when it comes to evidence of "mind reading" it alone will not suffice. Evidence of mind requires convergent validation with data that cut across a variety of other categories which presuppose the capacity to impute mental states to others, such as attribution, empathy, sympathy, gratitude, grudging, and sorrow.

Subjective reality

Donald R. Griffin

Rockefeller University, New York, N. Y. 10021-6399; Department of Biology, Princeton University, Princeton, N. J. 08544

Whiten & Byrne's (W&B's) stimulating target article reviews progress in the objective analysis of significant though limited evidence for intentional deception in nonhuman primates. It also goes on to present some helpful and realistic recommendation for gathering improved data in the future. The collection and comparative analysis of what are often disparaged as anecdotes is a reasonable first step, and one that should lead to collecting new and better data. To further the principal objective of establishing guidelines for future research, I suggest more emphasis on open-minded readiness to recognize and appreciate the unexpected. Most of the behavioral patterns discussed in W&B's paper probably surprised the scientists who first observed them. Since there is no reason to suppose that we already know everything of interest about deceptive behavior, there is a danger that unduly rigid guidelines might interfere with new discoveries.

W&B have largely freed themselves from the behavioristic inhibitions that impeded the investigation of animal minds for so many decades; but a few vestiges of these constraints still imply that it is scientifically unsound to speculate about possible subjective thoughts or feelings in nonhuman animals. For example, quotation marks still serve as a sort of security blanket when the authors make wholly reasonable use of words such as purpose, believe, intend, listen to, or unplanned. And the very denial that they are guilty of "an anthropomorphic approach," reinforces by implication the belief that subjective mental experiences are confined to our species. Since that belief is being challenged, it seems counterproductive to continue such genuflections when questioning the orthodoxy that they express.

Surely one of the most important questions about deceptive behavior is whether nonhuman animals consciously intend to deceive, but we feel more comfortable with general, noncommital terms such as representation or psychological state when we may actually be dealing with specific, conscious thoughts. For example, the optical image of the outside world on a monkey's retina is an internal representation; it is of minimal interest, however, because the important question is whether the animal experiences conscious, subjective imagery of present, past, or probable future events. Reluctance to use mental terms, or to consider their possible appropriateness, may well be an obsolete vestige of mindless behaviorism. Of course it will be a long time before we can hope for completely satisfactory evidence for or against conscious thinking in other species; but progress toward this distant objective will not be facilitated by reluctance to acknowledge that it *is* one of the most important objectives of research on tactical deception.

The distant blast of Lloyd Morgan's Canon

Cecilia Heyes

Department of Experimental Psychology, University of Cambridge, Cambridge CB2 3EB, England

Why do we want to know whether certain animals are "natural psychologists"; whether they have beliefs and desires, hunches and hankerings or, indeed, beliefs about hunches and hankerings for desires? I say "we" because, not only do I share Whiten & Byrne's (W&B's) curiosity, but it unites us with a growing number of psychologists, naturalists, and philosophers who are willing to argue into the academic small hours over the conditions for, and the significance of, the attribution of intentional states to animals. Dennett is a prominent member of this group [see multiple book review of Dennett's *The Intentional Stance*, *BBS* 11:3 1988.], and W&B seem to have taken several of their leads from him. Owing to the clarity of his thinking and the permeability of his prose, Dennett is an obvious choice of philosophical guide for any psychologist, as long as we are wary.

Dennett's ultimate destination – the reason *he* wants to know whether animals are intentional systems – is rather different from that of most other people. Consequently, if we take advantage of the path Dennett has beaten through the conceptual undergrowth we may well end up, neat and clean after a relatively easy journey, in a place we never wanted to be. On the other hand, if we follow Dennett just so far, and then try to strike out on our own, we may well find ourselves stranded in dense jungle far from our chosen destination. I suspect that W&B are approaching this impasse, and I would like both to outline the grounds for my suspicion and to offer them a sketch map of what I see as an escape route.

To appreciate what Dennett hopes will be gained by asking whether animals are intentional systems it is necessary to survey his principal contribution to this debate (Dennett 1983) in conjunction with several others (Dennett 1978; 1984; 1987). This broadsheet makes it clear that in a legitimate intentional characterization of an animal's behavior Dennett sees not an explanation of that behavior, nor any justification for the conclusion that the animal has a conscious mental life something like ours, but a manageable practice problem for AI modellers. At once optimistic about the ultimate utility of AI, and depressed by its recent retreat into toy worlds and the modelling of isolated faculties, Dennett urges AI specialists to develop their skills by modelling *whole* systems that are simpler than people, that is, the competence of animals (Dennett 1978).

So much for Dennett's destination. How does he clear a path to it? Although he may not have fully demonstrated the advantage of intentional over functional descriptions of animal competences (Heyes 1986) he has avoided being ensnared by a couple of major objections to the attribution of intentional states to animals: that they are (1) not observable and (2) not explanatory. Dennett secures the basic observability of intentional states by regarding them as postulates of an empirical, "folk" theory of mind, and he enhances their empirical discriminability using the "rationality assumption" which transforms, by idealization, folk psychology into the intentional stance (Stich 1981). The idea that reference to mental states is not explanatory originates in the long-standing claim that mental terms are irreducible to mechanistic terms (i.e., some common denominator of scientific explanation; Churchland 1979). Dennett deals with this objection by embracing it. He regards the attribution of mental states as an instrument of investigation, not as an explanatory terminus. Thus, Dennett beats a path to the legitimate attribution of mental states to animals by idealizing and instrumentalizing our everyday conception of mental states.

Despite expressing the wish to find out "just where it gets us to consider primates as 'natural psychologists,'" W&B hardly address this question. For several reasons, I take them to be assuming that if a primate can properly be said to be a natural psychologist, then its deceptive behaviour will have been explained, and we will have a better idea of what it is like to be a baboon (say): First, it is usual for specialists in any given field of science to expect their own activities to terminate in explanation; they seldom regard themselves, as Dennett would have them regarded, as data processors for another speciality (in this case, AI). Second, in section 2.4.2, W&B seem to be arguing that if animals behave in an intentionally describable fashion then those animals are *really* representing mental states. Finally, I take the what-is-it-like-to-be-an-X question (Nagel 1974) to be so compelling that all those interested in animal behaviour are guilty until proven innocent of being under its influence.

If W&B do expect intentional characterizations to explain animals' behavior and to give us a glimpse of their phenomenal worlds, then their destination is a long way from Dennett's, and it is natural that they have rejected his instrumentalism. However, in so doing they have forfeited a means of counteracting objections to the attribution of intentional states to animals from the standpoint of the unity of science. What will they say when someone asks: Why foist intentional states onto animals when it is so difficult to find a place in science for those which obstinately adhere to people?

Although they reject his instrumentalism, W&B make extensive use of Dennett's other machete – the rationality assumption. Although this does allow them to probe the content of mental states empirically, the rationality assumption, as part of an idealization of folk psychology, leads W&B down a path from which the conclusion that certain primates have mental lives *like ours* is thoroughly inaccessible. If the behavioral criteria for the attribution of mental states are different, then why should the phenomenal experience be the same?

Like most escape routes, the one I offer W&B would not take them to their original destination, but it would allow them both to take advantage of Dennett's insight, and to contribute as explorers, rather than Sherpas, to the resolution of a pressing issue. In my view, speculation and observation dedicated to finding out whether animals are intentional systems is valuable in so far as it has the potential to challenge the long-standing assumption that human behavior with certain distinctive operating characteristics also has distinctive origins. "Folk" assume that behavior which is distinguishable by its manifest flexibility in relation to environmental change is also distinctive in terms of its causes; that is, that it is *mentally* generated. Dennett has highlighted the possibility of testing this assumption by *first* identifying behaviours that have the manifest properties of mentality and *then* investigating the mechanisms that generate them. This way we can find out whether folk have cleverly and unwittingly picked out a natural kind in contemporary behavioural science.

Attempts, like W&B's, to identify animal behaviour with the manifest properties of mentality are an invaluable part of the first stage of this enquiry. This is because when animals are used, the second stage – investigation of the origins of behaviour – is facilitated by both the relative simplicity of the behaviour, and the range of invasive techniques that may be applied. However, W&B's data set will be spoiled for the present purpose if they allow the question of origins to interfere with the intial assignment of behaviour to intentional and nonintentional categories. For the most part, they succeed admirably in avoiding such interference, but in section 3.3 the distant blast of Lloyd Morgan's canon, the heavy artillery for obfuscating the distinction between causes and operating characteristics, can be heard. If we are to discover whether the behaviour that folk would class as intentional, on the basis of its manifest characteristics, has unique origins, then we sabotage the whole enterprise in allowing intentional status to hinge on the putative origins of the behavior in "prolonged training histories," "creative intelligence," instinct, or whatever.

A practical consequence of the recommended disregard for origins is that W&B need not be reluctant to propose, or to canvass the results of, laboratory studies. Like the behaviour of laboratory animals, that of free-living animals may be the product of "prolonged training histories" (just because the trials were not imposed and documented by an experimenter, it doesn't necessarily mean that they didn't happen); but if the behaviour of either is found to be *both* a product of conditioning *and* manifestly intentional we should be pleased, because it indicates that studies like W&B's have the potential to challenge received wisdom, and to extend, rather than reapply, our understanding of the mental.

Lies, damned lies and anecdotal evidence

Nicholas Humphrey
Ducklake House, Ashwell, Hertfordshire SG7 5LL, England

George Eliot, in *The Mill on the Floss*, sets a trap for those of us who smell deception everywhere. She represents Mr. Tulliver seeking advice from Mr. Riley about where to send his son to school. Riley knows of a particular establishment, and recommends it so enthusiastically but so implausibly that it seems obvious that he himself stands to gain from the boy's placement. In comes the author with a put-down to her "too sagacious readers":

> There is nothing more widely misleading than sagacity if it happens to get on a wrong scent, and sagacity persuaded that men usually act and speak from distinct motives, with a consciously proposed end in view, is certain to waste its energies on imaginary game. Plotting covetousness and deliberate contrivance in order to compass a selfish end, are nowhere abundant but in the world of the dramatist; they demand too intense a mental action for many of our fellow-parishioners to be guilty of them. (1860, chap. 3)

Whiten & Byrne (W&B) are surely aware of the dangers of attributing too intense a mental action to an animal. Yet, in discussing the remarkable evidence that they've amassed, they continually favour the dramatic interpretation over the more prosaic one. In the light of their own caveat about the *human* capacity for manipulating evidence, I think they ought to be more cautious.

I will focus on those cases where I happen to have some knowledge both of the animal and of the observer in the field. Dian Fossey was not, I believe, someone whose reports should be taken entirely at face value. This passage, from *Gorillas in the Mist* (Fossey 1983, p. 201), illustrates the problem: "On perceiving the softness, tranquility, and trust conveyed by Macho's eyes, I was overwhelmed by the extraordinary depth of our rapport. The poignancy of her gift will never diminish." In short, Fossey identified so closely with the animals (and was so jealous of their reputation vis-à-vis that of other apes) that she saw human qualities everywhere.

How then would she respond when asked to provide anecdotal evidence that gorillas are capable of showing a "higher mental faculty" (at least as capable as chimpanzees!)? At the very least she might have let imagination run away with her. Take the first case W&B mention (A3-sect. 2.5.1), where a gorilla is reported as dallying behind her companions after "she looks up into Hypericum tree and *spies* a nearly obscured clump of Loranthus vine" (my italics). It sounds like a neutral enough, nonsubjective observation. But it is not. Did Fossey *see* the gorilla "spying" the vine? How could she have recognised that she had "spied" it, except on the basis of her *subsequent behaviour interpreted as an act of tactical deception.* But if there was no evidence that she had seen the vine *before* she stopped and groomed, the deception-interpretation falls apart.

Or take the second case where a subordinate gorilla is reported as approaching a baby by building a series of fake nests (C1-sect. 2.5.3). I happen to know that this tactic was one that Fossey herself frequently adopted when *she* wanted to approach the centre of a group. Having seen her building nests so that *she* should deceive the gorillas, I cannot but be skeptical when she offers an exactly parallel example as a case of deception of one gorilla by another.

None the less, suppose the behavioural acts were as she states. The fact remains that classical ethology could, in this case, provide a perfectly adequate "lower-level" explanation, namely, that the animal, being caught between approach and avoidance, was in a "conflict situation" and responded with the "displacement activity" of nest building. I admit I do not like this kind of explanation any more than W&B do; but precisely because the dramatist within me would prefer to think the gorilla was showing deliberate cunning, the scientist needs double convincing that the simpler explanation will not do.

Is that a tall order? It need not be – as my own observation of Fossey's nest building confirms. Suppose someone were to suggest that Fossey herself was simply showing displacement behaviour. I would answer with considerations such as these: (1.) I never saw her nest building in any other conflict situation, (2.) I noticed that, while she built the nest, she also made fake belch vocalisations, as if to reinforce the "neutral image," (3.) I have independent evidence that Fossey herself understood the nature of deception, and in particular (*vide* the above example) that she could attribute deceptiveness to other creatures.

Beliefs about beliefs (e.g., belief that someone else is being deceitful) are not easy to pin down. Yet isn't it possible that the best evidence we could have for an animal being *capable* of deception would be evidence that the animal could *recognise* deception practiced by another? Admittedly, there are as yet no established criteria for distinguishing, at a behavioural level, the "moral outrage" that might be shown by an animal who recognises he's been duped from the "mere rage" he might show at losing out. But the ground has already been laid theoretically for making such a distinction (e.g., by Trivers 1981), and it ought (if it is real) to be observable in practice. Maybe the famous observation by Tinklepaugh (1928) of the rage shown by monkeys when they were cheated by a human experimenter needs looking at again.

You can't hide your lying eyes (or can you?)

W. C. McGrew

Department of Psychology, University of Stirling, Stirling FK9 4LA, Scotland

Whiten & Byrne (W&B) are to be congratulated for coming to grips with a slippery subject. The hands-on metaphor is apt. They have provided a practically useful taxonomy, from the down-to-earth definition of tactical deception, through the clear setting out of the players, to the 13-category typology. At last we can get down to gathering data which can be systematically compared. If this scheme did no more than this, it would be useful enough. In fact, by going on to tackle the "psychology" of the phenomenon, especially in terms of what more we need to know in most cases, W&B have made a uniquely helpful advance.

W&B emphasize the "social" model for explaining the evolution of intelligence, but they make no mention of the alternative "environmental" model, as proposed by Parker and Gibson (1979), Milton (1981), and so on. For social creatures, such as most primates, the former model is attractive; but it is less convincing for other taxa. For example, are pack-living wolves likely to be that much more intelligent than family-living coyotes, or pride-living lions than solitary leopards, to give two congeneric examples? In addition, how much empirical evidence is there that being caught out at (say) "crying wolf" actually results in being denied help by one's associates?

In stressing the importance of manipulation W&B may be exceeding reasonable evolutionary expectations. (See Hinde 1981 for another response to Dawkins and Krebs's 1978 thesis on manipulative communication.) After all, there may be equally good reasons to cooperate (that is, to engage in transactions in which both parties accrue net gains) in communication as in any other social interaction.

A traditional distinction not made explicitly by W&B concerns deception by acts of *omission* (inhibition of existing behavioral patterns) versus acts of *commission* (new use of behavioral patterns which goes beyond mere disinhibition). Perhaps ignoring another is the most simple example of the former, whereas distracting another is a good example of the latter. Omission seems to be less intellectually demanding than commission, but the two may be hard to disentangle. Consider the pet pig-tailed macaque described by Bertrand (1976) which was toilet-trained successfully by her owners, but which used the acquired signal of impending urination deceptively to extricate herself from tiresome situations!

It may be, as W&B state, that morphological deception occurs mostly between species, whereas behavioral deception occurs chiefly between the sexes but within a species. However, one of the most thorough empirical studies of behavioral deception remains that of Byers and Byers (1983) on pronghorn mothers concealing the location of their cached fawns from predatory coyotes. Similarly, though tactical deception may occur within groups (and the key variable is less likely to be species than degree of association), some cases at least of strangers fooling one another may qualify as tactical (Sargeant & Eberhardt 1975).

Mindless behaviorism, bodiless cognitivism, or primatology?

E. W. Menzel, Jr.

Department of Psychology, State University of New York at Stony Brook, Stony Brook, N.Y. 11794

Whiten & Byrne (W&B) present an interesting and useful survey of deceptive behavior in nonhuman primates, but I have several bones to pick.

1. Once W&B got me to worrying about whether the seemingly special abilities of chimpanzees are all attributable to the pecadillos of chimpanzee researchers, I didn't know where to stop. Soon I was stewing about analogous problems in marmosets and macaques. Next it was bats, chickens, sea slugs, and dung beetles. And then Noah's ark opened. I was saved from the deluge only by a cold beer and a dog-eared copy of the *Journal of Irreproducible Results.*

2. The term "mindless behaviorist" is an even worse ad hominem. It should be banned from print in scientific publica-

tions. Its direct alternative is, moreover, "bodiless cognitivist" – and I would certainly not want to qualify for that label either.

3. Menzel's (1974 central problem was formulated as a problem in "(zoological) geometry or mechanics," not cognitive psychology. In other words: Given that group of young chimpanzees in that big enclosure out there, how do they vary their locations with respect to one another and the environment? Where will the group go next; how do individuals scatter around their common center; who or what controls the group's travels; how is this control possible?

The deviousness and detouring in the movements of an animal that is headed toward a hidden pile of food when it is accompanied by a companion that characteristically takes all of the food, as opposed to the straightforwardness and directness of its movements when this companion is absent and it is accompanied by an animal that begs for a small share, is not a poetic metaphor. Nor is the difference difficult to demonstrate in chimpanzees, unless one chops the original problem down to fit into a T-maze. If my 1974 description sounds anecdotal, I recommend the procedures and "baseline" data that are reported in the remainder of the same paper also be inspected.

The question is: How devious can a given animal get, what sorts of detours can it negotiate, and to what degree do animals act together or split according to the personal payoff for so doing? But spatiotemporal deviousness and detouring are continua, not all-or-none categories. Cooperation and competition are two sides of the same coin of coordination, regulation, and control. How a normal living being moves from one moment to the next is never (in my opinion) accidental, trivial, or "uninteresting." I do not hesitate to translate pictorial and graphical representations into words if this will enhance communication; the word "deception" seems as accurate as any, but to assume that the "real problem" is deceit as a student of humans would define it, and that spatiotemporal analyses are only a means for getting at deceit, is putting the cart before the horse. Whether or not a given verbal label is warranted rests more on an understanding of human verbal habits than on an analysis of animal movements, and no two people read between the lines in the same way. Regardless of how many categories of deception one comes up with, I am not confident that they will describe or explain the total variance of animal movements any more effectively than would a straightforward locational analysis.

As Hull (if not every behaviorist) said, "However striking the analogy, it must never be forgotten that molar behavior theory is not molar physics" (1952, p. 243). But it is not human linguistics either. If cognitive ethology ever actually comes to treat animal movements as a secondary or coincidental rather than intrinsically interesting subject matter it will, in my opinion, become a contradiction in terms.

4. I doubt that the deception is rare or difficult to analyze in feral nonhuman primates (see Menzel 1966). An equally plausible position is that: (a) until recently, nobody explicitly looked for it; or (b) to see it, one must first of all identify the animals' Archimedean fixed points.

The latter task can certainly be greatly facilitated by simple experimental stratagems, such as introducing special "goal objects." Experiments are, however, an extension or refinement of common sense, not a substitute for it. (By the way, does anyone have any experimental evidence for deceit in any human president, king, or dictator? All I have ever seen here is anecdotes.) Thus, for example, the ultimate "fixed point" for a neo-Darwinian analysis would be animals' maximization of their inclusive genetic fitness (e.g., Dawkins & Krebs 1978; Trivers 1985); and whatever behavioral strategies and goals one can get at by means of short-term experiments or observations must obviously be viewed in that larger context. [See Maynard Smith: "Game Theory and the Evolution of Behavior" *BBS* 7(1) 1984.]

5. According to my dictionary, "deception usually refers to the act, and deceit, to the habit of mind" (*Webster's New Universal Unabridged Dictionary*, 2d Edition). Deception may or may not imply intent, and may be defined from the point of view of any participant in an interaction, including an external observer; deceit by definition implies intent on the part of the actor. It seems to me that W&B might be mixing up the two concepts. But in any event deception seems overcognitivized, and many problems remain unsettled. Thus: Who shall be tried for deceit? (If ants so qualify, what about a very deceptive ethological model of an ant?) Is the accused to be presumed innocent until proved guilty, or vice versa? (Compare and contrast Dawkins and Krebs and Trivers with human linguists.) Who qualifies as witnesses? How much circumstantial evidence does it take to make an airtight case? Should witnesses be asked for their on-the-spot judgments about guilt, or are all decisions to be relegated to a judge and jury who are not personally familiar with the accused (or, indeed, with the species to which it belongs)? Or, in general, what is the "law of the land"?

6. To sum up: W&B offer us a choice between "animals as mindless" or "animals as psychologists," but in so doing they obscure a third alternative, which still sounds fine to me. It is, of course, animals as animals – using Darwin's rather than Descartes' understanding of the terms "animal," "human," "mechanical," and "mental."

A prime goal of taxonomy is, in Plato's words, to "carve Nature at its joints." But where is the zoological joint between behavior and cognition? In my opinion, it lies in an "illusion" of human semantics, not in animal behavior. "If behavioral science ever develops the tools for analyzing organismic 'events as a whole' in a really adequate fashion, the old controversies between laboratory versus field, experimentation versus observation and various 'schools' of behavior theory will truly fade into oblivion" (Menzel 1974, p. 149; see also Menzel 1987).

Ontogeny, biography, and evidence for tactical deception

Robert W. Mitchell
Yerkes Regional Primate Research Center, Emory University, Atlanta, Ga. 30322

I am sympathetic to Whiten & Byrne's (W&B's) concerns and believe their taxonomy is useful in stimulating and categorizing observations of complex interactions. However, I fear that their inferences from types of act-context co-occurrences to mental causes are disturbingly reminiscent of, and lead to the same problems as, those of Romanes (1883/1977; see Mitchell 1986). Because an act based on mental representations about the mind of another organism is often perceptually similar to an act based on less complex representations, evidence other than the act (and its associated contexts) must be used to differentiate types of mentality (Mitchell 1986; 1987a). Adequate evidence of the type of mental representation a deceiver has would include knowledge of what the deceiver can know. Thus, the interpretation of deception must be based on our theory of the psychology of members of the deceiver's species, and perhaps of that individual (Mitchell 1986; de Waal 1986a). Such a theory requires that one observe organisms ontogenetically (Mitchell 1986).

Ontogenetic observation was, in fact, Morgan's (1894) remedy for Romanes's anecdotalism. Although W&B write that "historical antecedents" of the deception must be known to make inferences to mental representations, they attempt to make *these inferences without such ontogenetic information* (see, e.g., discussion about or following No. 26 under A1 [hiding from view]; No. 26 under A3 [inhibition of attending]: No. 16 under D3 [deceive TARGET about AGENT's involvement with TOOL]; and No. 83 under E [deflection of TARGET to FALL GUY]. Grounding observations of deception in knowledge of the species-typical behavioral ontogeny and psychological development of deceivers and deceived, and even in their individual biographies, transforms anecdotes into psychologically meaningful data (see chapters by Chevalier-Skolnikoff, Miles, Morris, Vasek, and de Waal in Mitchell & Thompson 1986a).

W&B's definition requires a catalogue of all contexts associated with a particular "honest" act from the "normal repertoire" of an individual before any example can be categorized as tactical deception (cf. Thompson 1986). However, many of the examples provided *cannot* fulfill this requirement, because the act used in deception does not occur in the "normal repertoire," only in the deceptive context. Such is the case for the act of concealing one's erect penis (A1, Nos. 66 and 67); the act of keeping one's eye gaze away from food (A1, No. 3); suppressing vocalization (A2, No. 13); and inhibiting one's attention (A3, No. 2). With these behaviors it is not, as the definition requires, that the "honest" act occurs in a "different" context. Instead, the honest act is different from the act used to deceive. Deception is indicated either because of the *absence* of the normal act-context co-occurrence (Nos. 2, 3, 13), or because of the *addition* of another (deceptive) act which counteracts an already present "normal" act (Nos. 66, 67).

W&B's definition is also based on the erroneous assumption that the normal, highest-frequency act-context co-occurrence in an organism's repertoire is the "honest" version of that organism's act. Contrary to this assumption, we know that an act may be more frequently deceptive than "honest" when the victim's failure to respond to it has serious consequences for the victim (Mitchell & Thompson 1986b, p. 359). Also contrary to this assumption, W&B include as tactical deception the false alarm calling of Munn's birds, which call false alarms more frequently than they do true ones (Munn 1986a, p. 171). Greater deceptiveness can occur here because the victims may lose their lives by not responding.

The widespread assumption, apparently accepted by W&B, that deception between familiar organisms is likely to be rare requires empirical verification, because in numerous cases deception is certainly continually repeated between the same two individuals even after its discovery (see, e.g., Werth & Flaherty 1986). Anyone who has played a game of object-keepaway with a dog or person knows that familiarity with a player's deception need not make it rare; indeed, it may become more frequent than its honest counterpart (see Mitchell 1987b).

The inherent problems with W&B's definition and their inclusion of instances which clearly do not fulfill it yet seem to be intuitively acceptable instances of deception suggests that W&B had other criteria in mind when they accepted these instances as tactical deceptions. Presumably they were interested in intentional deception as characterized by Russow (1986, p. 48), in which an agent's act is deceptive if the agent intends to produce a false belief in another organism. W&B repeatedly imply that the only other interpretation of an apparently deceptive act is that it is random. Thus, they do not articulate an alternative (and more likely) characterization: that the act is intended to produce a *response* by the victim without the deceiver's having any ideas about the victim's beliefs (see the distinction between planned deception and learned deception in Mitchell [1986, p. 25–28]). The idea quoted from Premack and Woodruff (1978) – that such a "behavioristic" theory about the victim requires more intelligence for a deceiver than does a mentalistic theory – is belied by our knowledge that children are "behaviorists" before they become mentalists (see Vasek 1986, p. 280). The behaviorist alternative assumes less complex representations (and is thus more economical) and seems likely to account for many of W&B's examples (especially types D and E).

Finally, I disagree with, or do not understand, some of W&B's ideas. (1) I do not understand the contrast between the AGENT's "adjusting his future behavior to the current state of the TARGET's evaluation of the AGENT's behavior" and the AGENT's "using a set of discrete rules" of the "if . . . , then . . ." sort; the latter seems necessary for the former. (2) I doubt that "only those classes of deception that occur more than once will ultimately be of interest," especially if one is interested in creative intelligence; a truly creative deceiver might invent novel deceptions to avoid being caught. (3) I do not understand how anyone is deceived in types D and E, though I do see how someone is manipulated. (4) I do not understand why the first chimp's later act in No. 24 is novel (unless by "novel" is meant "different from previous acts"), nor why the second chimp need be predicting anything about the other's psychological processes. Might not the second be thinking that something is odd about the first's actions and be waiting to see what it does?

Which are more easily deceived, friends or strangers?

Duane Quiatt

Department of Anthropology, University of Colorado at Denver, Denver, Colo. 80202

A question I wish to raise concerns the extent to which tactical deception depends on familiarity between deceiver and deceived. There seem to be no reports of intergroup deception in nature in Whiten & Byrne's (W&B's) corpus of anecdotes. Why? Is this an artifact of the way we observe primates or does it have more to do with the character of their behavior? Implicit in the notion of tactical deception is behavioral flexibility, largely though not entirely absent from interspecific mimicry or camouflage (the anglerfish at least does not change its bait). If effective deployment of signals, whether for clarification or obfuscation, is enhanced by feedback, as I assume it must be, then its seems likely that the better an animal knows its intended victim the better the feedback, the more accurate the cognitive representation of that individual's psychological state, and the more effective the deception. It is also true, however, that increased understanding of one another should entail advantages for specific individuals, both in deception and in defense against deception, assuming mutual familiarity and symmetry in communication. Here one can get stuck in a kind of revolving door and there is some danger of emerging with dizzy revelations about the role of deception in the evolution of ethics and language through selection for finer and finer discrimination between cheating and fair play in a system of reciprocal altruism. That would seem to me to conflate concepts appropriate to natural selection with those appropriate to learning in development. My question is instead designed to direct attention to what constitutes mutual understanding, how much of it may be necessary, and perhaps how much is too much.

Putting oneself in the shoes of another human being is something most of us have been encouraged to do – usually with the idea that it will make a better person of us, increase our understanding of the other fellow, and improve our disposition toward cooperation and mutual support. By definition it involves taking or sharing the perspective of some specific other individual, an ability which George Herbert Mead associated with competence in language (Mead 1934, cited by Vasek 1986). Vasek goes a step further, arguing that "Perspective taking and language skills are integral parts of *lying* successfully." For intentional deception to be successful, the person framing the deception "must . . . coordinate perspectives by taking the other into account . . . [and], because other people do not simply accept verbalizations without putting them in some frame of reference, the deceiver must recognize that others attempt to coordinate perspectives with him or her; that is, the people to be deceived try to relate to or understand the apparent truth, and if they cannot believe the message they may detect the deception" (Vasek 1986, p. 284, emphasis added).

It is perspective taking, not language skills per se, that is the *sine qua non* of lying. Deception at some level of definition (e.g., tact, diplomacy, commercial advertisement, putting a best foot forward and a good face on the matter) is pretty thoroughly embedded in ordinary discourse and interaction. At these levels – and perhaps still more evidently in the conventions of children's play or adult games and sport – nonverbal deception is at least as important as and more acceptable than lying. Success in gambling games such as bridge, poker, chess, or pocket billiards requires knowledge of winning strategies including those that involve deceiving other players as to experience, ability, present playing condition, goals, and game strategies. More than that, if a deceiver is to coordinate perspectives he must have sufficient understanding of the specific game context and of individual players to know what kinds and levels of deceptive behavior will be not only tolerated if discovered but – no less important and more to the point of the present discussion – anticipated as a part of normal play.

This suggests to me that deception may require more than perspective taking (necessary if one is to represent in one's own mind the psychological perspective of a potential victim) and the coordination of that perspective with a misrepresentation of one's own. It may require something in the way of mutual alignment of perspectives, a point well made by Mawby and Mitchell (1986), who in a study of feints and ruses in athletic games emphasize the significance of competition between individuals of comparable levels of athletic skill and, presumably, experience at deceiving and being deceived. A professional player may discover more difficulty, and greater risk of injuring himself, in attempting to "fake out" a less-than-competent opponent than a player at roughly his own level of ability (Mawby & Mitchell 1986, p. 321).

All of this suggests a general conclusion, in two parts: that deception is unlikely to occur where perspectives cannot be brought into partial overlap and that deception is not possible where overlap is total (Quiatt 1987). In *Homo sapiens*, language and translation afford overlap of individual perspectives on a broad basis – in most respects, given the rapidity of modern translation and cultural transmission of behavior, species-wide. At the same time, there are marked differences in environmental influences even within human families (Plomin & Daniels 1987). For human beings, environmental variance, insuring against perfect overlap of individual perspectives, is mediated by language. So is their capacity for self-deception; any language competent human being is probably able to represent to himself not one or two but any number of perspectives on the world. So, as far as human deception is concerned, the conclusion just noted seems trivial, but I wonder whether it does not have a useful application to intraspecific deception in nonhuman primates.

It is unlikely that two monkeys of the same species can see things from exactly the same perspective, any more than two human beings can; but the view from a laboratory cage is nevertheless a narrow one. Animals that share close confinement cannot distance themselves physically from one another, and its seems reasonable to conclude (in the absence of any information as to nonhuman primate fantasy life) that in such straitened environmental circumstances there cannot be much psychological distance either. At the other extreme are monkeys in different groups in nature, though they meet occasionally in contests for resources: One can only speculate as to how much they might have in common or whether a member of one group could coordinate perspectives with a target individual in another in order to deceive him. Clearly, however, the situation is different from that in human society, where language and language-based human cultures provide far more comprehensive frames for the coordination of individual perspectives.

Somewhere in the middle, between hard confinement and the free habitats of primate groups in nature, are the relatively open worlds of groups in which behavior is still constrained and individual perspectives are coordinated to some degree by human management – by restriction of range (through fencing or other means, as in island colonies) and, usually, the imposition of provisioned diet. Relatively few of the data collected by W&B derive from such groups, and again I wonder why. In any event W&B are, it seems to me, on the right track. It is through close examination of the circumstances of deception, including spatial and social constraints, dependence on common resources, and history of relations between deceiver and deceived, that "evidence for the origins of the deceptive act" (section 3.3.4) is likely to be obtained.

Only external representations are needed

Howard Rachlin
Psychology Department, State University of New York at Stony Brook, Stony Brook, N.Y. 11794-2500

> "Representation . . . a showing, exhibiting, manifesting. . . ." (*Webster's New Twentieth Century Dictionary of the English Language*).

Person A infers from the behavior of monkey B that monkey B infers from the behavior of monkey C that monkey C believes X. In principle, there is nothing wrong with this set of nested inferences just as it stands. Whiten & Byrne (W&B) correctly cite Wittgenstein (1953) and claim functional ("*economical*") justification for such statements. But what is the point – where is the economy – of adding to the chain of inferences the concept of a *representation* of monkey C's belief in the head of monkey C and a *representation* of monkey B's inference in the head of monkey B? [See also multiple book review of Sperbes & Wilson's *Relevance: BBS* 10(4) 1987.]

As you sit and watch a movie in which at a given instant, say, an actor is laughing, you see, in some sense, patches of color on a screen; in some other sense, a person; in some third sense, that person's emotions and beliefs. The second and third perceptions (and probably the first as well) involve inferences about events taking place at times other than that instant. But just as it is not necessary to suppose that an actor behind the screen underlies your perception of a person, so it is not necessary to suppose that a representation in the person's head underlies your perception of that person's emotions and beliefs. Representations, in these cases, are there on the screen, showing, exhibiting, manifesting something. Correspondingly, monkey C's beliefs and what monkey B infers about them are represented in monkey C's and monkey B's behavior (now and in the past) – not in invisible, inaccessible recesses of their heads.

There would be no harm in inventing unshown, unexhibited, unmanifested representations as a sort of game – given that anybody will actually bother to play it; where are the flow diagrams, where are the computer programs that this conception demands? – except that it distracts psychologists from more fruitful pursuits, more useful inferences, about how the animal as a whole has interacted with its environment (including other animals) in the past and how that relates to what is happening now.

The most disappointing part of this article is W&B's quotation from the philosopher Daniel Dennett (1983; sect. 3.3) to the effect that carefully designed experiments will provide histories of reinforcement by which behaviorists might explain what is observed. Psychologists interested in the minds of animals are therefore advised to abandon the experimental method (lest the animal mind be explained by behaviorists). Of course if there were any value in cognitive (as opposed to behavioral) explanations of the mind, W&B would not need to worry about what a behaviorist might say.

On anecdotal evidence, which W&B use to promote and demote various species of animals on some vaguely specified scale of intentionality, they might have quoted another observation of Wittgenstein (1953, sect. 199), that it is impossible to obey a rule only once; and they might have asked Thorndike's (1911/1965) question about all the dogs who don't find their way home.

Tactical deception: A likely kind of primate action

Vernon Reynolds

Department of Biological Anthropology, Oxford University, Oxford OX2 6QS, England

Whiten & Byrne (W&B) have done an excellent job of compiling reports on tactical deception in primates. It is interesting to note the lack of examples from prosimians and certain other primate groups, and the heavy concentration among Cercopithecidae and *Pan*. It seems likely that more work will reveal deception in more species, but this must remain open until we have more data. Perhaps what will eventually emerge is a scale of primate social intelligence. Man will certainly be at the top as the most deceptive primate of all, a point made by Colter Rule (1967), who regarded speech as man's way into ever more devious actions: "We have for so long thought of speech serving as a communications vehicle that we have difficulty seeing that it also served the function of furthering covert behavior" (p. 161).

It seems that tactical deception is most evident (so far) in those species that we already know to have complex social systems. Given what we know about the complexity of social interactions within these systems, the existence of tactical deception is not in itself surprising, though no doubt we shall be in for many surprises as the various forms it can take are revealed. Baboons that are capable of the complex mate acquisition strategies described by Smuts (1985), for example, are indicating the existence of qualities of mind that incorporate tactical deception along the way.

There are problems of evidential support. W&B ask for better, more detailed descriptions. Doubtless these will be forthcoming. However, I doubt that they will falsify the hypothesis that primates can mentally work out and then put into practice deceptive actions. Better descriptions will, I expect, render the current probability a certainty and give us a lot of detail about the structure of primate social thinking and how it differs from species to species.

This is a fascinating prospect. As Harré and Reynolds have pointed out, there are two interlocked problems here: the analysis of primate intentions and the analysis of primate intensions (Harré & Reynolds 1984, p. 249–50). W&B have embarked on the systematic study of the former; the latter remains to be attempted.

There remain many curious problems, of which I will mention three. First, why is so much deception focussed around the rather boring business of food getting? Would one not expect much more around sex? In humans, this is surely the case. What happens in circumstances where food is very plentiful?

Second, a priori one would expect less deception (or none) between close kin, and more as genetic relatedness declines. Can this be tested?

Third, are there any observable giveaways in deceiving primates? One thinks of the lie detector, which rests on increased skin-surface moisture and/or increased heart rate during human deception. Do monkeys show their two-facedness in ways that can be detected? And do they get better at deception over time, as we humans do?

There seems to be plenty of scope for future research, and we are grateful to Whiten & Byrne for opening the door.

Deception: A need for theory and ethology

Carolyn A. Ristau

Rockefeller University, New York, N. Y. 10021

It is indeed an excellent idea to garner first hand reports of deception from a large number of experienced field workers studying a diverse array of primates. The monograph by Whiten and Byrne (1986) is a still more useful reference, because it preserves all the entries and full descriptions of each. Without such a collection, the reports are often never published, being passed orally along research grapevines, altered with each retelling, or, at best, scattered widely in the literature as footnotes or embedded in other information. Much potentially important information is lost to the thinking and observational processes of other scientists. Possible patterns among species remain unseen; and an impetus to a different focus of observation and the design of fruitful new experiments is missing.

Since the days of Romanes (1883; 1884), one-time occurrences have been pejoratively termed "anecdotes"; they have thereby, as noted, been lost from the scientific literature. Yet, as Dennett (1983) and others have emphasized, the mark of intelligence will most probably be found in the unique event; oft-repeated acts by an organism demonstrate learning, an important ability, but not necessarily as revealing of significant intelligence.

One hopes the publication of Whiten & Byrne's (W&B's) collection will foster a slowly growing trend to publish careful reports from other species on a broad array of problems. Sensible statistical information to accompany such reports would likewise be of value. Included could be observation time and types of ecological and social situations encountered.

What are other reasonable next steps? A serious *grappling with theoretical issues* is needed. A categorization (preferably hierarchical) is needed of the various kinds of deceptions ultimately organized along more fundamental dimensions than the functional classification used by W&B. Others have begun this difficult struggle; some are listed by the authors (e.g., Miles, Mitchell, Russow, and de Waal in Mitchell & Thompson 1986a). See also Ristau (1988; submitted). W&B state that it would be "rash given only the data currently available" to attempt a more complete taxonomy of deception, reflecting the psychological demands on the AGENT, "the true mental complexity involved." Perhaps, but it's worth more attempts and is likely to help delineate both the possible high level cognitive interpretations and more rudimentary mechanisms. Researchers with extensive direct laboratory or field knowledge of their subjects could make useful contributions to this effort. Developmental psychologists concerned with the ontogenetic development of deceptive abilities in human children could also be helpful in creating a psychological taxonomy. Also useful would be close, detailed observation of the process whereby an organism becomes more proficient at a specific deception and the kinds of errors made in the course of building the expertise.

Woodruff and Premack (1979) have done this in their well-known experimental study of chimpanzee deception. Initially, a chimpanzee knew the location of hidden food, but only the human had access to it. The "good guy" human, after finding the food, shared it with the apes, while the "bad guy" kept the food to himself. Occasionally, roles were reversed and the human knew the location of hidden food whereas the chimpanzees did not. After repeated trials, the four chimpanzees grew more proficient in deceiving and avoiding being deceived, but to different extents. The point I wish to make is that through careful attention to behavioral detail and the use of videotaped records of the apes' behavior, Woodruff and Premack and their assistants noted the following progression in the development of deceptive skills:

1. Emotional responses occurred, specifically anxious behaviors which caused confusion as to the significance of other cues.

2. Inhibition of correct orientational cues occurred, first the voluntary (e.g., approaching) and then the involuntary (e.g., eye movements).

3. Finally, redirection occurred via voluntary behavior such as pointing, which emerged spontaneously during the experiment. Possibly this order is a general one, applicable across species, tasks, and even ontogeny.

Such careful attention to detail amounts to an ethological approach. [See also Gardner & Gardner "An Ethological Alternative to the Law of Effect" *BBS* 11(3) 1988.] An ethological perspective might also suggest that we reconsider the literature on "displacement" activities – behaviors performed by organisms when thwarted or frightened, and so on. We may find that the classes of displacement activities, such as preening or feeding, are the very behaviors preferentially chosen by animals engaged in some categories of deception – distraction, for instance. This is not to deny the possibility of a high level interpretation of the deception, but rather to look to existing biases in behavioral repertoires as a path to the voluntary and strategic use of those behaviors. The human smile provides an example. First largely reflexive, a human gains considerable voluntary control over its use. An ethological perspective will also reveal that each species has its own set of attention-grabbing behaviors; some are found across many species. Do they use such behaviors selectively in deceptive acts? Direction of gaze is a particularly potent behavior – certainly used extensively in the primate deceptive acts and powerful in nondeceptive acts such as the human traffic jams caused by "rubber necking."

W&B recognize a need for experimentation and offer suggestions for some of the examples of deception. Experiments are essential to help determine the extent and *limitations* of the abilities described as deceptive. These needs become still stronger as the study of deception is extended to other species. Is the AGENT able to "take the role of the other," recognizing that concealment entails more than burying one's head in the sand (i.e., "TARGET-centered representation" [A1, sect. 2.5.1])?

A final word: The hopes expressed by W&B are rather grand – to "sketch the features of other individuals' psychological states that an individual with deceptive intent must be able to represent." Attempts to produce a categorization of deceptive acts with ethological, psychological, and philosophical underpinnings – or all of the above – may well aid in the realization of that hope.

Deception and adaptation: Multidisciplinary perspectives on presenting a neutral image

Thomas R. Shultz[a] and Peter J. LaFrenière[b]

[a]*Department of Psychology, McGill University, Montréal, Québec, Canada H3A 1B1 and* [b]*Ecole de psycho-éducation, Université de Montréal, Montréal, Québec, Canada H2C 1A6*

Viewing animals other than humans as capable of thinking and feeling about their social world has long been suppressed by such pejorative labels as *anthropomorphic, mentalistic,* and *unscientific.* We applaud the efforts of Whiten & Byrne (W&B) to break out of this constrictive zeitgeist by collecting and classifying anecdotal accounts of tactical deception in primates. We will respond to W&B's question about whether they are proceeding in the right way from our own perspective of developmental psychology.

It seems inevitable that studying deception will bring together scientists working in disciplines that have grown too insular for healthy expansion and growth. For example, paralleling the recent surge of interest in tactical deception in primates is an emerging literature in developmental psychology on the human child's knowledge about the appearance/reality distinction (Flavell 1986) and their developing *theory of mind* (Astington et al., in press). The historical contexts for these two parallel strands are quite different. Ethologists are coming to realize that behavior can be more effectively explained in intentional (cognitive) terms. Developmental psychology has long been cognitive, but has recently directed attention to the child's cognitions about mental life. It is extremely likely that cognitive ethologists and developmental psychologists have much to learn from each other.

The ethologists could learn from the developmental literature that the largely second-order intentional phenomena discussed by W&B do not emerge full-blown in the human child, but develop in apparently orderly fashion out of earlier precursors. These abilities follow the appearance of representational capacity, which appears at about 18 months (Piaget 1962; Leslie, in press), and the emergence of first-order intentional phenomena, which appear at about 3–4 years (Shultz 1980; Wellman & Johnson 1979). One sees in the ethological literature a strong focus on phylogenetic comparisons, but little appreciation of ontogenetic development. Even if some primate species fail to show second- or higher-order intentionality, or even if they succeed, it would be interesting to identify the earlier stages. Moreover, the techniques and concepts used in studying second-order intentionality in children, involving false beliefs (Wimmer & Perner 1983) as well as deception (LaFrenière 1988; Harris et al. 1986; Shultz & Cloghesy 1981), may provide some inspiration to ethologists. It need not be assumed that research techniques used with children are exclusively verbal, and thus inappropriate for nonhuman species. Poulin-Dubois and Shultz (in press), for example, demonstrate the appearance of the concept of *agency* in infants by the end of the first year using a measure of habituation of visual attention (*agency* being a precursor of first-order awareness of intentional states).

From W&B, developmental psychologists studying early theories of mind could learn about the feasibility of collecting anecdotal material on second-order phenomena and classifying it according to putative social functions. From other cognitive ethologists (Dawkins & Krebs 1978; Humphrey 1976), they could relearn the importance of studying cognitive skills in a social context; that manipulation and deception may be among the more adaptive aspects of higher intelligence. We find the evolutionary arguments for considering social context considerably more convincing than many of the less rational appeals that are common in the developmental literature.

W&B and others (Dawkins & Krebs 1978) make the point that deception must be rare to be effective. But if it is so rare, how can it also confer adaptational advantage? The answer is presumably that deception must occur in critical situations, for example, mating. But the evidence and argumentation for this still needs considerable work. The reason that deception must be rare to be effective is presumably that animals learn to modify their reaction after being duped several times. This point is certainly open to empirical investigation – whether in the laboratory, using variants of the paradigm designed by Woodruff and Premack (1979), or other ingenious, and perhaps more naturalistic, experiments.

On this and other issues it may be possible to implement variants of the *Sherlock Holmes method* described by Dennett (1983). This involves making assumptions about the animal's intentional states and using these assumptions to create a situation that is likely to provoke a telltale response on the part of the animal – a response that makes sense only if the animal actually possesses the assumed intentional states. As Seidenberg (1983) has correctly noted, such provocations are more than techniques for collecting anecdotes; with appropriate controls, they constitute experiments. They are considerably more natural and potentially more telling than the laborious laboratory training procedures criticized by Dennett (1983) and by W&B.

The challenge, of course, is to integrate laboratory and field research, recognizing the unique value of each. While laboratory research can inform us about the capacity of various animals for learning, problem solving, and even tactical deception, naturalistic observation and experiments embedded in the natural environment à la Tinbergen (1965) or more recently Seyfarth, Cheney, and Marler (1980) can provide compelling evidence about the adaptive function of behavior.

We hope that the study of adaptive deception will not be put aside indefinitely. What evidence can be cited from primatology for or against the hypothesis that creative intelligence evolved as a response to selection pressures created and sustained by the challenge and necessity of life in social groups? Although this issue is alluded to in W&B's first paragraph, their discussion is entirely too muted for an interdisciplinary, commentary-type journal such as *BBS*. Their functional taxonomy allows one to classify a deceptive act according to the immediate consequences of the agent's behavior. However, these functions have not been explicitly linked to inclusive fitness. In this respect, we wonder whether Whiten & Byrne are guilty of presenting an overly neutral image of an exciting and controversial position.

Family life and opportunities for deception

Peter K. Smith
Department of Psychology, University of Sheffield, Sheffield S10 2TN, England

Whiten & Byrne's (W&B's) target article makes an excellent start in delineating the kinds of tactical deception which occur in primate species. To the examples in their category B4 ("distract with intimate behavior"), they could add the use of play as a distractor. Breuggeman (1978) documented how adult play in *Macaca mulatta* often involves some form of social manipulation; a mother might play with an infant to distract it from persistent suckling or with an older sibling to keep it away from an infant. Also, a sibling might play with another sibling to keep it from the mother, or from a third sibling. In addition there are some indications from the primate literature that rough-and-tumble play can be "used" to inflict hurt or damage, which is not the normal outcome of such play bouts. Of course, it is always difficult to make judgments of intent in individual instances, and even in human children there is continuing debate on whether, or how often, rough-and-tumble play bouts (and associated play signals) are used in a deceptive way (Humphreys & Smith 1984; Neill 1985).

The evolutionary speculations summarised in W&B's Table 1 are obviously *very* preliminary. The differences reported may simply arise because some species have been observed much more intensively. From Table 1, there is actually a close rank-order correlation between the number of stars a species is awarded, and the number of workers to whom the questionnaire was circulated for that taxonomic group. W&B invoke the nature of family groups as one explanation for supposed phylogenetic differences. They suggest that there may be less tactical deception in gorillas because of "the stable intimacy of a gorilla family group" and they invoke a similar argument for typically monogamous callitrichids and hylobatids. This argument is suspect. Family groups can provide an excellent base for tactical deception. Breuggeman's examples of deceptive play occurred in this context, and Trivers's (1974) theory of parent–offspring conflict (and associated sibling rivalry) gives a clear adaptive rationale as to why deception might be selected for.

Of course, much parent–offspring conflict may not involve tactical deception by W&B's definition. Suckling has been a paradigm example of parent–offspring conflict. If an infant simply whines or suckles more than the mother wishes, this is conflict but not deception. However, if the infant whines or suckles when its nutrient needs are apparently satisfied, this would be deception and indeed tactical deception if the different nutrient state is taken to define a different context for the action. As a related example, Altmann (1980) describes "tantrum" behavior in infant baboons, often precipitaed by the mother making the nipple inaccessible. The tantrum resembles a "fear paralysis," but is usually ignored by the mother. It would be interesting to consider whether such tantrums qualify as tactical deception (they certainly appear to be deceptive and are usually recognised as such by the mother).

So far as humans are concerned, deceitful signals from children to parents, although poorly documented by child psychologists, do seem to emerge in the second year of life together with other aspects of symbolic function such as symbolic play, language, and humour (McGhee 1979). Dunn and Kendrick (1982) have indicated that children can be very sophisticated in situations of sibling rivalry by 2 years of age; the authors argue that very young children have a "fairly sophisticated model of others and of themselves as psychological beings" (p. 186). A stable, intimate family context does not, in itself, discourage deception; rather the opposite. However, it may be that, since family members know each other well, occurrences of deception tend to be more subtle (except, perhaps, in infants) and therefore less obvious to an external observer.

Social strategies and primate psychology

Shirley C. Strum
Department of Anthropology, University of California, San Diego, La Jolla, Calif. 92093

> So then Sheila says to Betty that Arnold told her what Harry was up to but Betty told me she already heard it from Blanche, don't you know. . .

I applaud both the rationale and the results of Whiten & Byrne's (W&B's) endeavor. Restoring abilities (mental and other) to actors is not an isolated reaction to "mindless" behaviorism among some psychologists and ethologists but a convergence with trends in anthropology, sociology and in interpretations of what science is and how it is done (see references in Latour & Strum 1986; Latour 1987; Strum & Latour, in press). The authors' approach does what it claims to do: by classifying, formalizing, and presenting the anecdotal data on tactical deception in nonhuman primates W&B suggest heuristic patterns, stimulate the categorization and analysis of further existing data, and encourage the collection of more crucial data.

My real question concerns the focus on deception itself. As W&B point out, deception is a form of "manipulation." They are right to look to a "flexibility of response" such as that shown in "tactical deception" (Byrne & Whiten 1985) as a way to test the hypothesis that primates are "natural psychologists." The admissible evidence for this is doubly narrowed, however, because tactical deception is both a subclass of "social manipulation" (Western & Strum 1983) and is self-limiting (the constraints imposed by the community of familiar individuals). If the inquiry is extended to all types of "tactical" social manipulation – what I have elsewhere called "social strategies of competition and defense" (Strum 1979; 1981a; 1982) – then we greatly enlarge the data and perhaps resolve some dilemmas presented by the tactical deception corpus.

It may be argued that deception involves a higher order of intentionality, in representing the beliefs and desires of others (see Dennett 1983 and multiple book review of Dennett's *The Intentional Stance*, *BBS* 11(3) 1988), than "honest" acts do. Is this really the case? Clever (in the sense of skillful, ingenious), devious (in the sense of circuitous), tactically complex honest acts should provide evidence for the proposition that individuals have representations of each others' psychological states. They should also provide a way to evaluate whether deception is truly an advance in psychologizing. The appeal of "deception" as a topic may have a hidden agenda (see Latour & Strum 1986) rather than being just an innocent scientific quest.

Nothing is lost and much is gained when we ask "did the AGENT achieve" his goal (rather than whether he was engaging in deception) "by tailoring his behavior to the social context in a way that requires the AGENT to represent another individual's psychological state" (sect. 3.3.3). The important questions, particularly for phylogenetic concerns are: Who needs *social manipulation* and why, under what conditions and with what restrictions? The traditional answer is rather circumspect: Since society is entered into – mindlessly – rather than created actively, only a few areas are open to social manipulation. A different answer (Strum & Latour 1987) suggests that actors (or "agents" define what society is for themselves and for others. Actors have much greater scope for manipulation when negotiating and constructing, that is, "performing" a society; they also need to know the same things that scientists do in investigating such societies (including the difference between beliefs and behavior). This scenario for the evolution of the "performative" social bond highlights the different avenues that are possible, contrasting tactical, social strategies – which require cognitive assessment skills – with genetically encoded social manipulation (see Figure 2 in Strum & Latour).

A limited set of factors may predict the occurrence and evolution of tactical sophistication among primates. These include the size and heterogeneity of the group, its social stability, the value and type of cooperation, and specific migratory patterns of individuals. Under some conditions there may even be significant sex differences in the necessity for and constraints on social manipulation (Western & Strum 1983). If so, the growing momentum for the "primate intelligence . . . as an adaptation for handling the complexities of the social environment" position (sect. 2.4.1) may be overstated. Few primate species live in groups that satisfy the conditions implied by W&B's "complexities of the social environment" in anything but a limited sense.

W&B examine the distribution of data in Table 1 and ask whether it is fact or artefact. Let's consider the baboon, chimp, and gorilla comparison more closely. (Baboons rather than cercopithecines in general are considered because comparable evidence is present from at least one population; see Strum 1987 and references therein.) I suggest that the differences are real and significant within the performative framework just described. The degree and type of social complexity and the opportunity and necessity for specific kinds of social maneuvers differ among the species. For chimpanzee groups, provisioning or captivity create a baboon type of performative overlay on the natural chimp system. The deception data is thus a hodgepodge – some "natural" to the chimp, some facultative chimp-baboon behavior. Yet because chimps are not *evolutionarily* baboons, they lack some "natural" baboon types of behavior. It is unlikely that chimps are too smart to be duped by W&B's classes D and E of deception, since humans aren't!

I would also recommend a new methodology, one that might provide the variety of crucial data highlighted by W&B (sect 3.3–3.3.4). The "extended case history" would follow one animal from a well studied population (or as many animals as observers) all day, every day, for a minimum of three months (or a year or even more). A decade ago I did a pilot one-month study and was particularly struck by two things. First, interactions build up not just over hours or days but over weeks (perhaps even months or years). This is critical to our concern with cognitive representations, tactics, and plans. Sampled data allow us to make inferences, but piecing together the story leaves a lot of room for accusations that we are "upgrading" behavior, individual skills, and species characteristics. Second, the moment of learning and insight passes quickly and any method not geared to the rare event easily misses it. Of course we claim that learning has occurred by comparing past and present behavior, but wouldn't it be nice to capture the moment, and more than once (see Strum 1975; 1981). Case histories make anecdotes into more rigorously convincing demonstrations of cognitive achievements. Case histories of diverse social strategies, not only tactical deception, should add significantly to our understanding of primates as natural psychologists.

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Misdescription and misuse of anecdotes and mental state concepts

Roger K. Thomas
Department of Psychology, University of Georgia, Athens, Ga. 30602

"Deception" is a valid way to conceptualize some acts of human behavior, and Whiten & Byrne (W&B) do appreciate the difficulty of determining its validity for nonhuman primates. However, despite W&B's caveats, anecdotal data are not acceptable in behavioral science.

Anecdotes have two major flaws. The first is inherent; one can never be sure that sufficient relevant information has been observed. The first example of deception in the target article can be used to illustrate this. It was reported that a baboon being "chased aggressively" adopted "the alert posture and horizon-watching normally shown when an important entity like a . . . predator has been spotted" and that the baboon doing the chasing "stopped to look for the focus of interest." W&B asserted that "in this case no such entity existed" and concluded that the baboon had deceptively distracted its chaser.

Despite the assertion that no entity existed for the "deceiver" :o see, there is no way to be certain. The baboon may have seen something the observer missed or it may have mistakenly "seen" something (e.g., a rock formation mistaken for a predator). If the baboon saw or even imagined it had seen something, then the act was not one of deception.

There is another possible explanation for the "deceived" baboon's behavior. What if the baboons were playing "follow the leader"? If so, then "chased aggressively" is an incorrect inference and the following baboon's stopping "to look for the focus of interest" was merely part of the game. The objection may be raised that experienced observers can distinguish a "chase" from a "follow." Perhaps so, but scientific evidence must be justified by more than an observer's confidence, especially when isolated instances of behavior are involved.

A second flaw often seen in anecdotal and other observational reports is the inclusion of biasing and unjustified inferences. Consider two more examples. First, de Waal (No. 66; A1, sect. 2.5.1) reported that a couple of chimpanzees were "courting each other surreptitiously" and "restlessly looking around to see if any other males were watching." Having inferred these, how could de Waal avoid "observing" that an ensuing act would be one of deception! Second, Altmann reported (No. 4; C2, sect. 2.5.3; emphasis added):

> Every day, one can see females approach mothers, *pretend to be primarily interested* in grooming the mother when *what they are really after* is an opportunity to sniff, touch, or hold her infant. . . . "But is the mother *really deceived?*" asks Altmann: "Surely the multiparous ones *know exactly what's going on!*"

Eliminate the inferences from this alleged case of double deception, and the observation is "Female approaches and grooms mother; female sniffs, touches, or holds infant." Surely such anecdotes cannot be accepted as evidence in a behavioral science.

The problem of including biasing and unjustified inferences in one's observations is remediable. However, the elimination of inferences from *description* would require considerable reorientation to proper descriptive language. For example, words which may seem descriptive, such as "chase" or "groom," imply the doer's intention.

Observers vary in the use of unfounded inferences, and many are to be commended for the relative purity of their descriptions. Nevertheless, even the most careful observers tend to use mental state concepts inappropriately in their descriptions or explanations.

For example, recurring as a causal agent among the cited examples of deception is the concept of "aggression." Aggression, like deception, is a mental state concept. There is no such *thing* as aggression in the sense of having an isomorphic physical correspondent. Aggression is defined ultimately in terms of some set (which, itself, must be defined) of behavioral hypotheticals. One such hypothetical might be, "If A runs behind B and bites B, then A is aggressive." (Note the use of "runs behind" instead of "chases," which begs the question of whether the behavior is aggressive.) Assuming that an acceptable set of behavioral hypotheticals to define aggression has been determined, a fundamental question is whether aggression or any mental state can function as a causal agent.

Fodor (1981) and Churchland (1984) discussed several philosophical positions pertaining to the roles of mental states. The two extreme positions are represented, perhaps, by the "radical behaviorists," who disavow completely the need to postulate mental states, and by the "functionalists," who allow that mental states may function in an explanatory account as causes of other mental states. According to the functionalists, aggression could be a cause of deception.

However, even if one accepts the functionalist position (which, in principle, I do) two major problems remain. First, there is the problem of determining an acceptable set of behavioral hypotheticals to define each mental state. Second, there is the problem of determining appropriate functional relationships among mental states or among mental states and behavioral outputs. Churchland chose "pain" (and Fodor "headache") as exemplars of mental states. Churchland's account of "pain" can be used to illustrate some unresolved issues pertaining to the second problem.

According to Churchland (1984, p. 36), pain causes both behavioral outputs ("wincing, blanching, and nursing of the traumatized area") and other mental states ("distress, annoyance, and practical reasoning aimed at relief"). But, as Lorden and I noted (Thomas & Lorden, in preparation), the relationship between pain and other mental states is unclear ("What is psychological well-being? Can we know if primates have it?").

For example, (a) it is reasonable to think of "pain" *directly* causing "distress," "annoyance," and "practical reasoning" but not vice versa; and (b) it is reasonable to think of pain being directly reducible and localizable to physical substrates but not the others. The point is that there may be fundamental differences among mental states, and the significance of these differences must be evaluated before mental state concepts can be used defensibly in functional relationships.

I realize that adherence to the views expressed here would postpone if not preclude the study of deception in primates or, for that matter, the study or use of most mental state concepts in field research. That might not be a bad thing, because I fear that the current use of mental state concepts in such research is, in many cases, delusional.

Deception and descriptive mentalism

Nicholas S. Thompson

Departments of Psychology and Biology, Clark University, Worcester, Mass. 01610

Between the extremes of "mindless behaviorism" as Whiten & Byrne (W&B) so aptly describe it and causal mentalism (which seems to lie at the core of their project) is a desirable middle ground originally charted by E. C. Tolman (1951), Albert Hofstadter (1941), and Gird Sommerhoff (1950). This middle ground I have called descriptive mentalism (Thompson 1987b). Those who occupy this middle ground can have the advantage of causal mentalism and its economy of expression, but without its worst disadvantage, namely, that it leads the unwary to make vacuous explanations (Lipton & Thompson, in press). Mental states, whether they are mental representations or drives or thoughts or whatever, cannot cause behaviors because behaviors are constituents of mental states. To say that an animal eats because it is hungry is like saying that an object is a table leg because it is part of a table. The "causality" is semantic, not physical. Just as tableness is in the relationship of the boards to one another and to the activities of humans, so the essence of particular mental states is in the activities of humans or animals in relation to their environments. Mental states are instances of natural design (Thompson 1986a; 1987a; 1987b). They are higher-order patterns which require for their recognition intimate knowledge of an organism's relations with its social and physical environment.

If one grants that mental predicates refer to complex patterns in the behavior of organisms, one can readily see why anecdotalism of the sort put forth in this article is a dubious endeavor. Deception is design to defeat design (Thompson 1986). To establish a behavior as an instance of deception one must specify thoroughly the background of behavioral order against which the deceptive behavior is anomalous. Thus, operationally speaking, the anecdote necessary to reveal deception has to be a much longer one than any that are told here. In fact, nothing short of a comprehensive, systematic, and standardized review of the relevant parts of the species' ethograms will do.

Like many explanatory mentalists, W&B confuse mental states with brain states, defining a mental representation as a neurally coded counterpart of some aspect of the world. A mental state is no more in the brain case then tableness is in the boards that make the table. Surely no one really takes this sort of definition seriously. Are the authors ever planning to look in the brain to see whether their mental representation is there? How would they recognize it if they found it? Wouldn't they first have to identify the "aspect of the world" that is the brain state's counterpart. And having done that difficult bit of behavioral description, what would be the point of naming it after its origins in the brain?

The identification of mental states with brain states seems to be part of an attempt to sidestep the difficult task of giving a comprehensive account of design in behavior. Like all instances of natural design, a mental representation is impossible to specify at the level of a particular behavior under a particular circumstance, because it is a higher-order pattern of behaviors and circumstances. W&B thus rightly look for mental representations at a different level from that of behavioral particulars. Unfortunately, they go downward, to the biological level, rather than upward, to the level of higher-order patterns. The locus for a concept like mental representation (or any mental term, for that matter) is in long-term, higher-order patterning in the behavior.

In short, the comparative study of deception as proposed here will not succeed until it has been preceded by a study of the general patterns of behavioral design against which deceptions are played out.

Authors' Response

Toward the next generation in data quality: A new survey of primate tactical deception

R. W. Byrne and A. Whiten
Psychological Laboratory, University of St. Andrews, St. Andrews, Fife KY16 9JU, Scotland

We see a major function of the Open Peer Commentary process as leading to a refinement of theoretical perspectives and an enhancement of future data collection procedures. To this end, we use this response to work toward a new questionnaire about primate tactical deception, one that has benefited from the constructive criticisms of many commentators.

1. Mindful behaviorism

When we suggested that those few scientists who have until now published data on primate (chiefly chimpanzee) tactical deception may have shared a lack of sympathy with "mindless" behaviorism, we seem to have given the wrong impression to some commentators. **Baldwin** and **Humphrey** believe that we continually favor dramatic, cognitive explanations over more prosaic ones; **Menzel** wants the term mindless behaviorism banned altogether; **Mitchell** urges us to attempt in every case to explain tactical deception as first-order intentionality, and **Baldwin** accuses us of dismissing the literature on conditioning (which we take to be the same point); **Griffin**, however, believes we do not go far enough in throwing off behavioristic inhibitions, and **Thompson** agrees that our term is an apt one.

"Behaviorism" has meant various things, so we should make our position clear. If it means that comparative psychology should rest firmly on the analysis of behavior, we agree and are behaviorists. If it means the experimental analysis of learning, then again we support the enterprise as far as it goes, although we share **Dunbar**'s concern that behaviorism has not grappled (and perhaps cannot) with the large portion of learning that occurs in a social context. Throughout the target article we implicitly assumed the presence of what seems dictated by conventional wisdom, namely, first-order (or even zero-order) intentionality, acquired by conditioning; only in a very few cases did we claim there was second-order intentionality. We have spelled out conditioning explanations for some tactical deceptions elsewhere (Byrne & Whiten 1987), but *BBS* space limits did not allow these relatively familiar arguments to be detailed in addition to providing what we felt to be more useful, given the records collected already: the implications of getting good evidence for second-order intentionality. Whether tactical deception is the best place to look for such evidence is disputed among the commentators: **Gallup** argues that it alone can never be evidence of "mind reading," but **Dunbar** cites it as a crucial test case. We discuss deception's relation to other forms of manipulation below.

If behaviorism instead means that comparative psychology is not allowed to talk about "mind" or "representations," we dub it "mindless" and we do indeed reject it – in favor of mindful behaviorism. Thus we would want to go beyond the description of behavior that **Menzel** advocates, to ask about the representational structures (and we would not care if they were localized in the big toe, cf. **Thompson**) which subserve behavior; we cannot understand or sympathize with **Rachlin**'s attack on the whole idea of representation, when it is now so conventional and fruitful in the neurosciences. **Thomas**'s rejection of all terms he calls "mentalistic," including most of the words found useful by our informants, is likewise unhelpful. He is unable to follow his own prescription, and suggests that several baboons aggressively chasing another were "playing follow the leader." Replacing the mentalistic terms, "follow" with "run after" and "leader" with "animal in front," does not help!

Rather than trying to enforce bans on particular terms or types of explanation, we would commend **Bennett**'s clear dissection of how a putative "mind reading" explanation can be challenged or supported. It is behavioral evidence which can settle the matter.

2. Science and story-telling

The suspicion was voiced by **Dennett** that we would be misread as having condemned experimentation in favor of anecdotes: He was right! Thus **Rachlin** claims we advise readers to abandon experiments, and **Bernstein** says we dismiss experiments as worthless. Neither is so, as is made clear by **Dennett**'s preemptive defense. Perhaps we can return the favor by pointing out that the "frustrating side effect" (**Baldwin**) of prolonged experimental training schedules is that of making it difficult to distinguish between first- and second-order intentionality and is thus frustrating for any *experimenter*. This point was made originally by Dennett (1983), who suggests a kind of experiment which would *not* provide such a reinforcement history; this "Sherlock Holmes" approach is advocated by **Shultz & LaFrenière.** Our own support for intelligent experimentation has not varied since the target article was written, and we appreciate **Menzel**'s notion that "experiments are an extension or refinement of common sense, not a substitute for it." Several commentators have suggested interesting ideas for future experiments (e.g. **Gallup**'s plans for blindfolded primates, and **Shultz & LaFrenière**'s mention of some techniques that have been successful with children).

Our belief that well-constructed analyses of observational data *can* be as rigorous as experiments receives cogent support (**Dunbar**). **Burghardt**'s balanced historical survey puts the whole debate in perspective – including the nice point that Lloyd Morgan called for careful, critical use of anecdotes and himself used them freely! **Burghardt** considers the biggest dangers in such an exercise to be (1) the observer's desire to tell a good story and (2) personal affection for the animals, with a consequent natural desire to view them as "superior." It is the latter criticism that **Humphrey** makes of the late Dian Fossey, urging that both her records (see target article) can be discounted. Concerning one he argues that she can have had no evidence that a "deliberately ignored" item had indeed been seen; however, hanging around in its vicinity and then unhesitatingly rushing toward the item when the conditions change is surely such evidence. In the other, he raises the possibility of nests being built as a displacement activity; we too consider this explanation (Byrne & Whiten 1987) and come to the same conclusions as to how it should be tested. But the fact that Fossey herself used nest-building to manipulate gorillas into a sense of security is not relevant, unless we can know that she did *not* learn this trick first from observing a gorilla! We agree with **Ristau** that so-called displacement activities should be given very careful scrutiny.

Many commentators either agreed with and supported our view or urged independently from their own perspective that the background history and context (the "baseline data" of **Menzel**) of an observational record are crucial for its proper interpretation (**Baldwin, Bernstein, Menzel, Mitchell, Thompson**). It must be remembered, however, that *all* of our informants were highly familiar with the repertoire of normal behavior (the "ethogram") of the species they reported on; were this not the case, much more explicit background data would have been necessary even for the limited interpretations we ourselves were prepared to make.

Several commentators reiterated our points about the need for "control" data, null hypotheses and multiple records of the same behavioral pattern from different populations (e.g., **Bernstein, Rachlin**). **Thomas**'s argument that one can never be sure that everything important is noticed in an anecdote, so anecdotes should be ignored, is really an attack on the whole of observational science, especially ethology; it is equally true (and unhelpful) to say that one can never be sure that there has been a control for every important variable in an experiment. "Good" experiments control the right variables, and "good" observational records include the right data. **Menzel**'s thoughts on "who shall be tried for deceit" are also worth considering carefully.

Commentators correctly suggested that a corpus of records of deception can be used to test hypotheses other than those directly concerned with mental representation. **Reynolds** points out that one should expect a decrease in the benefits of deceiving as the degree of kin relatedness increases between actor and target (although see **Smith**, for the likelihood of within-family deception). And **Quiatt** suggests that neither very close proximity (such as a cage) nor infrequent contact (such as another group) permit tactical deception to be as useful as at intermediate levels of familiarity.

We could not help noticing that nearly all commentators who have worked with primates under natural or seminatural conditions support the basic enterprise we have begun (**Altmann, de Waal, Dunbar, McGrew, Reynolds, Strum**), and their critiques aim at doing the job better, not decrying it. (That the same is true of the philosophers, **Bennett** and **Dennett**, suggests that this is not due to the naivete of field primatologists!) **Burghardt** reminds us that bias against using rare observations in science led to primate infanticide going unrecognized for years, and **Menzel** notes that we still await more than "anecdotal" evidence for deceit in any king, dictator, or president!

3. A lifetime of deceit

Would the (enormous) effort of a day-in, day-out case history of a single individual primate (advocated by **Dennett, Dunbar, Mitchell**, and **Strum**, of whom the latter has tried it for one month with a baboon) be worth it? We believe the answer is an emphatic "yes," and we consider the idea one of the most exciting to emerge from this *BBS* Commentary.

Dennett provides a nice analysis of the problems of discovering any true creativity, making the point that repeated use of a tactic actually diminishes its claim to be clever (a Catch 22 that we have also lamented: Whiten & Byrne 1988a). It is the *first* use of a tactic that will tell us whether it was conditioned, imitated (**Baldwin**), or creative, and to be sure of recording the first use there is no substitute for a developmental case study. The same applies to the development of, or failure to develop, tactics of counterdeception: It is the ontogenetic route that is of interest, not the end product, if we are interested in mental capacities. Testing **Altmann**'s interesting speculations on the function of teasing in primate play will also need longitudinal study.

We do not, however, underestimate the logistical costs of this enterprise. Who will be the first to try this method of studying tactical deception with a wild primate?

4. Children are primates, too

We have been rightly rebuked for not referring to the existing literature on deception in child development (**Burghardt**), although in fact this literature is not extensive and "suffers" from a dependence on anecdotal observation, perhaps for the same reasons as does the study of primate deceit. (Note: **Menzel**, and *Webster's Dictionary*, would have us use "deception" for the act and reserve "deceit" for the habit of mind, but when the *Oxford English Dictionary* defines deceit as "a piece of deception" it seems too late to reform usage.)

LaFrenière (1988) has carried out an analysis of tactical deception in children which directly parallels our own on primates, and we refer readers to this. **Schultz & LaFrenière** report that second-order intentionality does not simply emerge "full blown" in children's development, but always follows the emergence both of representational ability and of first-order intentionality. We think that **Heyes**'s injunction to distinguish firmly between origins and operating characteristics is suggesting that a tactic which is learned by trial and error may nevertheless (later) be deployed with the full insight that its mechanism of action depends on affecting the mind of another – i.e., it may show second-order intentionality. If so, we heartily agree, and look to ontogenetic study of children and primates to help us sort out such cases.

Bennett argues that to understand the relationship between another individual's mental state and its consequent action, it is first necessary to understand the relationship in oneself. **Gallup** goes further in claiming that any ability to model the experience of others relies on reflection upon one's own experience. These points amount to a strong and testable claim about the order of developmental stages of understanding minds. Such beliefs, originating with Mill (1889), have also been used by Humphrey (1983) to explain the evolution of consciousness. It is not apparent to us, however, that these inward-looking capacities are logically necessary foundations for the development of second-order intentionality.

We also consider plausible what is almost the converse, that *some* reflection on one's own mind occurs through treating oneself as if one were any other person to be observed and studied: In other words, it is a behavioristic analysis.

Ristau notes our caution in avoiding classifying records in a taxonomy based on level of mental complexity, but she still urges that we need such an exercise, suggesting that developmental psychologists may contribute to this usefully. Indeed so, and we were delighted with **Chevalier-Skolnikoff**'s careful and thought-provoking analysis of our corpus of records in terms of Piagetian Stage Theory. Of course, any such attempt has to be tentative with the current quality of data, but it is an excellent start and data from the next questionnaire should allow it to be tested and refined.

5. Do primates know they may have been fooled?

Any primate that has insight into how its tactical deception works (i.e. second-order intentionality) should be logically capable of anticipating that it too may sometimes be deceived; we thus expect *counterdeception* to be associated with such insight (Byrne & Whiten 1987; **Chevalier-Skolnikoff**). Strictly, of course, tactical counterdeception may require third-order intentionality, and we are here assuming that a system which can handle second-order intentionality can produce a third-order by recursion. Memory may limit this recursion, and certainly (as Dennett, 1983, notes) even humans can handle only a few orders of nesting at best. The issues parallel those of embedded relative clauses in transformational-generative grammar (Chomsky 1965). We have few candidate examples of counterdeception so far (Whiten & Byrne 1986), but it is one of the patterns to which detailed ontogenetic studies must be alert (see section 3, above).

The one case of counterdeception we presented has been questioned by **Bernstein:** Since the second chimpanzee was evidently dominant over the first, why did he not simply displace the other and so obtain the food? Why should he *need* to resort to a counterdeceptive tactic? The explanation (Plooij, personal communication) is that provisioning at Gombe was strictly rationed at this time by means of remotely controlled boxes, opened according to a schedule. To avoid aggression, boxes were not opened if two individuals were present. Thus the dominant animal could not expect to obtain food by displacing the other, or even driving him right away, only by using him. And whether he could be used in this way would depend on an assessment of whether he knew he was about to be fed, and was concealing this fact. All records of counterdeception that we have at present are, like this one, from artificially provisioned or captive chimpanzees.

Humphrey suggests that a duped primate should show some sign of "moral outrage" if and only if it later understands that it has been intentionally deceived. Not only do we agree with this, but we can provide a candidate record: When one informant (Plooij) deceived an overly friendly young chimpanzee into walking away in the direction in which he looked intently (sect. 2.5.2, para. 3, target article), he added that she later returned, hit him on the head, and ignored him for the rest of the day!

Reynolds makes the fascinating and more general point that there might be "give aways" in the behavior of a deceptive agent that, though no doubt very subtle, might still be observable: behavioral lie-detectors. Of course this is the case in man, a fact which Bygott uses in his delightful cartoon illustrations of intentional hypotheses of tactical deception (Byrne & Whiten 1987). Why indeed should it not be so in other primates?

6. Consciousness

Talk of a primate "knowing" and "anticipating" may seem to imply that we are concerned with the animals' phenomenal worlds: whether they can be said to be conscious. **Griffin** thinks we should be, and **Heyes** believes we must be (as it is so interesting). Once more, however, we deny such a preoccupation: Perhaps we may explain with an example from artificial intelligence. Suppose a digital machine were programmed to simulate second-order intentionality in dialogues using its teletype and video display unit: This would be a desirable step in the development of accessible expert systems (Byrne, in press). What *we* are interested in is whether this intentionality, which users remark on, is achieved by means of the machine computing the mental states (current knowledge, beliefs, and desires) of the user. If so, then we would happily claim that the program showed second-order intentionality. If not, then the simulation would be a mere trick, produced by means of a series of (if . . . then) rules, but without the machine knowing anything about the mind of the user (this is the distinction that **Mitchell** finds unclear in section 2.5.3 of the target article). Others may be interested in whether or not it makes sense to talk of the program, or its machine instantiation, as conscious; we are not.

Use of this example from the artificial intelligence of the future will also make it clear that, to the extent that **Heyes** is right and Dennett is "really" concerned to help artificial intelligence model whole systems, we have every sympathy with his enterprise.

Issues concerning the role of different types of learning are not the same as issues concerning the presence or absence of second-order intentionality. But since laboratory conditioning experiments operate by teaching (if . . . then) rules, they are especially likely to generate behavior which – though it looks complex and intelligent – is in fact entirely first-order in its intentionality, with no insight on the animal's side into why it works when it does. This is why, despite **Heyes**'s urging, we still intend to concentrate our survey on natural or seminatural contexts.

7. Phylogeny or ecology?

In the target article we pointed out a number of potential reasons, other than a difference in intellect, for obtaining species differences in occurrence or frequency of tactical deception. Apart from bias in the number of observation hours and quality of observation (cf. **Menzel**'s worries), these reasons are chiefly ecological, and we would suggest ecology as the answer to **Reynold**'s very reasonable question as to why tactical deception occurs more often to gain food than sex: Food acquisition occupies most of many wild primates' lives. **Strum** extends our argument in an interesting way. She notes that humans are not too clever to be duped by tactics of types D and E, so our idea that chimps are too smart for this isn't likely. Instead, she treats the difference as real: Baboons are more deceptive, and more cleverly deceptive, than chimpanzees; and provisioned chimpanzees "benefit" from the artificial conditions which put an overlay of baboon-like social complexity on the natural chimpanzee system (see Whiten & Byrne, 1988a, for an examination of the relationship between intelligence and societal complexity).

Strum's view is more or less the opposite of **de Waal**'s; based on his long knowledge of chimpanzees and more recent experience with monkeys, he has no hesitation in stating that chimpanzees are more intelligent in social contexts. Could it be that the species one knows best seems brighter because one is more attuned to the detail of its communication system? De Waal, like Whiten & Byrne (1988b), points in particular to the chimpanzee's unusual ability to *inhibit* behavior, its self-control, whereas **McGrew** argues there is evidence of greater intelligence in crimes of commission than those of omission. Having sympathy with each of these conflicting ideas, we wait in the hope that further observations will settle the matter.

Despite our explicit unwillingness to make any claims about a chimpanzee/gorilla difference in intellect, **Bernstein** alleges that we do, and on the basis of 9 versus 2 *reports,* whereas these figures refer to different *types* of tactical deception and the figures for reports are 33 versus 2 (Whiten & Byrne 1986). We reiterate that this still proves nothing, for the same reasons that we gave in the target article. **Gallup,** however, considers the difference (in favor of chimpanzees) to reflect reality, and relates it to his failure to find a gorilla who can deal with his mirror test. Any strong record of a gorilla (or a baboon) showing second-order intentionality in its tactical deception would falsify **Gallup**'s theory, which is therefore a powerful one. As **Bennett** notes, most records in the current corpus are only weak evidence for intentionality, but this does not apply to all. We consider **Gallup**'s theory highly unlikely on the basis of several of the records (see Byrne & Whiten 1987).

8. The scope of tactical deception

8.1. Machiavellian intelligence. Deception is just one aspect of social manipulation (including cooperation), and should not be viewed as an isolated capacity (**McGrew, Strum**). We fully agree (Byrne & Whiten 1988). We also share interest in the debate about whether human intelligence was partly an adaptation to allow Machiavellian manipulations and cooperative tactics, or to deal with environmental complexity, or both (**McGrew, Shultz & LaFrenière**). Yet we did not feel that the target article was the place to air these issues (e.g., see chapters by Milton, Wynn, and editorials in Byrne & Whiten 1988).

8.2. Changing the definition of tactical deception. Our original attempt at a definition included the words "used at low frequency," and this has been very reasonably challenged (**Menzel, Mitchell, Shultz & LaFrenière, Smith**). We stand by our original arguments (Byrne & Whiten 1985) concerning why we would not be surprised at the rarity of tactical deception, and thus cannot agree with Danto that if it is important it really should be common (consider the case of infanticide). But we have to agree with **Mitchell**'s argument that when the penalty for an incorrect response to a bluff-call is, say, death, then tactical deception may well become rather frequent. Another case where we might expect high frequencies is one in which the TARGET cannot in principle know whether the AGENT was deceitful (e.g. infant food-calling). We suggest that the definition be modified accordingly.

Similarly, we now feel that it is prejudging the issue to insist, by definition, that the TARGET be a "familiar" individual. We have already noted (above) **Quiatt**'s hypothesis that intermediate levels of familiarity may be the ideal context for tactical deception; **McGrew** cites an example of cross-species tactical deception; and **Smith** makes a persuasive case for looking for tactical deception within the family. The last does not conflict with our point that (1) the pay-offs of deceit differ as both relatedness and habitual need to cooperate vary with social system, and (2) deception among intimates will be particularly subtle and hence hard to detect. But it will be interesting to ask empirically where tactical deception is commonest, and thus "familiarity" should be removed from the definition.

Mitchell argues that, although we specified that acts of tactical deception also occur as an "honest" version in the species' ethogram, the examples we were sent and that we use do not all meet this criterion. We agree that it is unwieldy to say that not staring at something one hasn't noticed, or casually resting a hand that conceals nothing on one's knee, is "honest," and we now change the definition. However, the sense in which we meant the term was evidently understood by informants and commentators alike, as Mitchell acknowledges.

8.3. Replication or duplication? It would be nice to be able to compare the frequency of forms of tactical deception between different primate populations and species (**Chevalier-Skolnikoff**). To this end, in our next questionnaire evaluation we will take steps to ensure greater comparability between numbers of observers per taxon (**Smith** notes a positive correlation between number of informants and number of categories of tactical deception submitted in the present corpus), and to measure the approximate hours of species-experience which back the submitted records. Nevertheless, the problems we describe in the target article will always impede easy comparisons.

As noted especially by **Dennett**, a high frequency of putative tactical deception is a two-edged sword: Multiple records give confidence, but repeated use of a behavior makes us question whether it is deceptive at all. We remain convinced, therefore, of the value of a questionnaire approach in pooling independent data from different observers and different primate agents to pick out recurring *patterns*. Such patterns will not be exact duplicates, but will be "the same" at a more abstract level of goals and tactics: In **Dennett**'s terms, "variegated replications," or in **Bennett**'s, "teleological patterns."

9. The new questionnaire

Reading the peer commentaries encouraged us to believe that there is extensive active interest in the study of primate tactical deception, and that, although both case-history and experimental approaches should be pursued, good science can be achieved by careful analysis of well-recorded observations. Furthermore, the commentaries have shown that such a questionnaire study can be formulated better than in our first attempt.

To this end, we now request that readers with unpublished observational data on *tactical deception* send them to us; as before, contributors will be acknowledged in any ensuing publication, and will receive a copy of the complete catalogue of records submitted.

The meaning of *tactical deception* will be well understood by anyone reading the target article, but it will now be defined as: "Acts from the normal repertoire of the AGENT, deployed such that another individual is likely to misinterpret what the acts signify, to the advantage of the AGENT." Records can be made very much more useful if they are augmented by good contextual background and control data. In particular, please carefully consider the following:

1. How did you know that an individual was deceived?
2. Was the tactic used more than once? If so, please give each record fully (if feasible; if it was used many times, please give the first in full and summarize the frequency and pattern of use thereafter).
3. Were there any indications in the behavior of the TARGET (or any duped animal) that it was aware that it had been manipulated or deceived?
4. In particular, was there any indication of tactics of counterdeception being deployed?
5. Were there any signs in the AGENT's behavior to suggest that it was being deceptive? More generally, was the behavior when deployed tactically different in any way from the normal version?
6. Was there any evidence of "mind reading" or "second-order intentionality" (see Dennett 1983; target article; **Bennett**) on the part of the primate AGENT?
7. Is it possible, from your knowledge of the animals, that the tactic was learned by trial and error or by reinforcement from an original coincidence? If so, please describe how this might have occurred.
8. Do you have any other evidence relevant to the possible origin or ontogeny of the tactic? For instance, were there opportunities for imitation or observation, facilitating circumstances, precursors to the full-blown tactical deception, and so forth? Can any possibilities be ruled out?
9. Do you regard the behavior as fitting into one of the thirteen categories we used in the target article, and, if so, which one?
10. How many hours of detailed observation experience of the species did you possess at the time of the observation? (Please estimate, even if only approximately.)

We expect that the best quality data do not yet exist, and that readers with these questions in mind will be the ones to collect them.

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THE AFRICAN ELE-FUND

PRACTICAL ELEPHANT CONSERVATION

The African Ele-Fund aims to improve the protection of elephants and their habitat wherever they are threatened and to raise public awareness throughout the world of the plight of the largest land animal on earth.

Elephant populations are declining in virtually all of Africa: every year East Africa loses 8.1%, Central and West Africa lose 17.8% and parts of southern Africa are losing 8.2%; only in the southern African countries where poaching is under control, is there a slight increase of 0.7% per annum, (figures from data compiled by Dr. Iain Douglas-Hamilton for UNEP in Nairobi). Surveys show that elephant numbers are declining more slowly in protected areas - National Parks, Reserves, etc. - than elsewhere, but that protection MUST be improved if the downward trend is to be halted.

The major cause of the decline in elephant numbers is the illegal ivory trade. Poachers are often better equipped, better armed and better paid than the park guard and rangers who try to enforce the law. Anti-poacher work is frequently hampered by the lack of simple equipment - sometimes just a spare part for a vehicle, or boots and waterproof clothing for foot patrols. The African Ele-Fund is appealing for donations and bequests to help even up the odds in favor of the elephants. Every penny and every cent given will be spent in the field; the Ele-Fund is organized by volunteers and administered at no cost by the *Wild in Britain* and by the *Eastern African Wildlife Society of Kenya*. The *Fauna and Flora Preservation Society* (UK and USA) has also agreed to accept donations earmarked for the Ele-Fund, as has *WWF-International* in Switzerland.

ELE-FUND RAISING

Fund-raising is centered around a seriers of Park Profiles, drawn up by scientists and conservationists working in the field. Problem areas are thus pinpointed and lists of urgent needs prepared and costed for each park. Individuals, schools and societies will focus their fundraising on certain items of equipment or sums needed to cover vital work. For example, the Mount Elgon National Park in Kenya needs a minimum of £3,500 (US $6,020) for vehicle repairs, and £15,000 (US $25,800) per year to cover the costs of extra anti-poacher patrols. Without this help, the unique salt-mining elephants will be wiped out by the current spate of ivory poaching.

Kindly send a SASE (self-addressed stamped envelope) along with your donation to an address below if you wish to receive a list of Ele-Fund raising ideas:

IWC/Care for the Wild, 26 North Street, Horsham, West Sussex, RH12 1BN, UK
IWC USA, 1807 H Street NW, Washington, DC 20006, USA
IWC Canada, 542 Mount Pleasant Road, Suite No. 104, Toronto, M4S 2M7, Canada
East African Wildlife Society, PO Box 20110, Nairobi, Kenya

Please make cheques, postal orders, or money orders payable to the African Ele-Fund.

Co-ordinators: Ian Redmond, 60 Seymour Avenue, Bristol BS7 9HN, England;
Telephone (0272) 46489

Hezy Shoshani, Dept. of Biological Sciences, Wayne State University,
Detroit, MI 48202, USA; Telephone (313)577-2865