

Misdescription and misuse of anecdotes and mental state concepts

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"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" to 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

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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 re-orientation 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.