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1988

### Deception and Descriptive Mentalism

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#### Repository Citation

Thompson, Nicholas S., "Deception and Descriptive Mentalism" (1988). *Faculty Works*. 65.  
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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.

## Deception and descriptive mentalism

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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 than 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.