

immediately the appropriate meaning of the tools, although no definitive reasons for her ability are offered. Eventually, two chimps were able to learn the names of six objects and to request them through a keyboard button press or to deliver them to an experimenter when requested to do so.

Unfortunately, the main point of the SR&B report seems to be chimpanzee-to-chimpanzee communication. The experiment fails to demonstrate any such communication, except for one reported anecdotal episode. The experimental procedure consists of having the chimpanzee request a tool from the other chimpanzee, the requester then obtaining the food and sharing it with the tool-provider. That a chimp would willingly share any amount of food with another chimp is surprising in itself, although not germane to the question at hand. The "control" to show that the two chimpanzees were actually communicating with each other was to turn off the keyboard. This seems much like teaching a person to drive a car and then removing the steering wheel. It tells us absolutely nothing about chimp-to-chimp communication. To find out the degree to which the chimps were interdependent on *each other* (as opposed to communicating with a machine), what should have been done was to have an automatic dispenser send the requested tool down a chute into the requester's grasp and to have the computer programmed to request tools from one of the chimps so that if the chimp put the appropriate tool through a hole in the wall, it would deliver some food to the chimp a little later. Would the performance accuracy be affected in any way? We doubt it. The communication between chimpanzees is limited to monologues, with one chimpanzee requesting and the other complying. One piece of evidence that the presence of another chimp is important (as opposed to a computer or a tool delivery mechanism) is an anecdote:

"... on one trial Sherman requested *key* erroneously when he needed a wrench. He then *watched carefully* as Austin searched the tool kit. When Austin started to pick up the key, Sherman looked over his shoulder toward the keyboard, and when he *noticed* the word *key*, which he had left displayed on the projectors, he rushed back to the keyboard, depressed *wrench*, and tapped the projectors to *draw Austin's attention* to the new symbol he had just transmitted."

This richly-interpreted anecdote (italics added to emphasize the richness of the interpretation) is the best evidence SR&B have to indicate that the *chimps* are communicating with one another, as opposed to the computer. SR&B incorrectly state that such anecdotes are the only data "suggesting that Washoe and other signing apes are producing anything more than short-circuited iconic sequences."

The vitriolic attack by SR&B on Project Washoe might seem puzzling to the reader familiar with the published reports of the Gardners and Fouts, and seems rather difficult to explain purely in terms of objective science. It is ironic that SR&B present their most interesting result as an anecdote (see above). Apparently one investigator's "observation" turns into an anecdote when seen through another's eyes.

NOTE

1. Order of authorships was determined by the flip of a coin.

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*Should mentalistic concepts be defended or assumed?* Although we consider ourselves cognitivists of sorts, and although we would have been at least 75 percent favorable toward any one of the target articles taken separately, together they had a cumulative effect which has left us uneasy and wondering what is in store for animal psychology in the future. Indeed, the harder we have struggled with the authors' treatment of awareness, intentionality, language, symbolism, iconicity, self concept and animals' theories of mind, the more inclined we are to concur with Popper: "One should never quarrel about words, and never get involved in questions of terminology. One should always keep from discussing concepts. What we are really interested in, our real problems, are factual problems, problems of theories and their truth" (Popper, 1972, p. 310). Perhaps the difficulty is that all-or-none questions such as "Do animals have awareness?" or "language?" create qualitative if not artificial dichotomies of what are psychological continuities, and run the risk of obscuring as much as they clarify.

*Are mentalistic concepts useful?* Until the advent of behaviorism and ethology it was generally assumed that the study of behavior was of interest only insofar as it could be shown to shed some light upon mind. Today the situation is to some extent reversed. The prediction and control of behavior is one of our major goals (for some it is the sole goal), and there is a tendency (illustrated in all three articles) for people interested in the functioning of minds either to try to make their

questions sound as behavioristic as possible or to try to set them up in direct opposition to behaviorism. Is it really necessary, however, to bow to or to flail at this demon? Why not simply say that what constitutes a useful, interesting, or fruitful question depends upon one's point of view? From our point of view, mentalistic questions or concepts need no more defense than behavioristic questions need defense for a behaviorist. As a matter of fact, even if it should prove possible to predict and control 100 percent of the variance of behavior as a nonmentalistic defines it, this would not necessarily tell us anything we care to know. Consider, for example, a man who is falling from a tower. Where is he going? From the point of view of physics, a Newtonian account would be just as accurate and complete for the man as for a stone (assuming, of course that the man has no parachute); but it would say nothing about whether or not the man was going to his death (a question that we would not even pose in the case of the stone and that could not be answered by a Newtonian physics), let alone whether he was aware of this fact but perceived it as going to meet his maker and to a new life (questions that we would not ask about most animals or even about an anesthetized human).

The most important function of theories in our present state of ethology and psychology is not that they summarize knowledge already obtained but that they serve a guiding function, affecting the sorts of new hypotheses we propose and the sorts of new data we collect. In this respect cognitive approaches need no defense - or at least, no more than any other approach.

*Defining mentalistic concepts.* The idea that it is possible to identify mind (or awareness, or what have you) is not substantially different from a naturalist's belief that it is possible to identify "life," a behaviorist's belief that it is possible to identify "real behavior" as opposed to simple motion, or a linguist's belief that it is possible to identify "language." None of these terms refer to physical objects as such; they all refer to processes that we attribute to some objects or beings and not to others. While G initially suggests that it does not seem necessary to be overly concerned with formalistic definitions (and we agree), he then seems to encourage this very formalism. Ostensive definitions probably work just as well, but it is important to remember that at least two such definitions are possible and each alone has serious limitations. On the one hand, following the logic of Turing (1950) one can simply produce or point to one or more specimens (A) to which most scientists would unquestionably attribute mind, intent, or what-have-you. Other specimens (B) about which we are unsure are then assessed against this norm. To the extent that one would find it difficult or impossible to discriminate between the performances of competencies of B and A, we would accept the null hypothesis of no difference and (by definition) attribute to B whatever specific process in question we attributed to A. Today, of course, the accepted norm (A) is a "normal adult human being" (some would even say a linguist), and B is anything else, for example, a computer, a one-month-old child, a chimpanzee. As we see it, all students of "chimpanzee language" operate on this Turing logic (Menzel, 1978). The target articles by SR&B and by P&W are cases in point.

On the other hand, following the inverse of Turing's logic, we might produce or point to a normative specimen (A) to whom we would definitely *not* attribute the internal process in question. A stone or a gas molecule that is moving purely in accordance with the known "laws" of physics, chemistry, or "chance" is a simple case in point; a computer simulation is a more complicated case. The performances and competencies of an unknown specimen (B) may then be compared against those of A, and if we cannot discriminate between them, we assert that there is no basis for rejecting the null hypothesis of "no difference, therefore no mind (etc.)." Williams (1966) says we know we are dealing with life when, in order to achieve an adequate explanation of what we see, we are forced to invoke principles above and beyond those of physics and chemistry, which apply to all objects in general. To expand on this, we know that we are dealing with *sentient* life when we must invoke still more specialized principles, above and beyond those that apply to all objects in general, and to all *living* things in general, including plants and ... (here already the list grows problematical).

Quite obviously, whether we use the performances of adult humans or of rocks as our definitional criteria for abstract concepts rests on conventions which are apt to change from time to time and on which there is less than 100 percent agreement at any one time. These conventions (including what performances on the part of A and B are critical to an issue) are philosophical and sociological as well as scientific. Which of these criteria is most fruitful may depend on the issue being addressed, however, it is safe to say that keeping both in mind is apt to be less limiting than concentrating on either one alone.

*Can one animal take into account the behavioral and cognitive capacities of another?* It is hard for us to imagine how any animal could survive otherwise. We therefore concur with P&W that this question should be answered in the

affirmative. We have developed our own argument in some detail elsewhere (Menzel and Johnson, 1976 *op. cit.* G). It should, however, be noted that even nonmentalistic accounts of evolutionary epistemology would have no quarrel with such a position. In the evolutionary sense, animals, and even plants, are problem solvers. The conjectures and tentative solutions that they incorporate into their anatomy and their behavior are (if one chooses to use such terminology) biological analogues of theories, and vice versa (Campbell, 1974; Lorenz, 1977; Popper, 1972). Just where one is justified in attributing to animals phenomenological understanding of others, or theories of mind in the literal, anthropomorphic sense, is an interesting and open question; and there are not many animal researchers who have addressed themselves to this question as directly and explicitly as have P&W. However, we would suggest that the important issue is not whether animals have "theories of mind" but what these "theories" are, how they came to be "formulated," and so on. We would emphasize that certain concepts, seen in the overall context of the animal's natural behavior, including its economic situation and biological and social history, do not seem so strange or mystical as they might when proposed in the abstract.

As another example, take the question: Can animals withhold information or lie? If by "withholding information" one means inhibiting one's response under circumstances in which another animal might profit more than oneself from this response (or in which one might stand to lose a good bit), then withholding of information is common, if not ubiquitous (see Dawkins and Krebs, 1978). Why else, for example, do many animals "freeze" at the first sign of a predator? Here, as in many other "mentalistic" issues, the presumed special status of information withholding - or prevarication, for that matter - tends to obscure the similarity between this problem and the general one of assessing perceptual and conceptual categories and capacities of animals, or indeed of humans.

Is human language the archetypic communication event or the archetypic cognitive event? All three of the target articles seem to imply that human language is the archetypic communicative or cognitive phenomenon. In our opinion, such a position generates a number of pseudo-questions that deflect research away from more interesting and fundamental questions. One of these pseudo-issues is "What is language?" Quite possibly because they are so concerned with this issue, students of "chimpanzee language" (see also the symposium on this topic in Harnad, Steklis and Lancaster, 1976 *op. cit.* G) seem to be the most severe critics of each other's work. SR&B in particular seem to require far deeper understanding on the part of other investigators' chimpanzees before they use the terms "communication," "language," or "intent" than could easily be demonstrated for most humans.

Adequately operationalized concepts are, of course, critical for studying psychological processes. However, one can operationalize away the main point of an idea. For example, SR&B emphasized the importance of finding context-free demonstrations that the chimp has knowledge about arbitrary relationships between signs and the things they stand for, which sounds to us like S-R psychology revisited. They also seem to imply that reliance upon recall cues make a performance less like language. This is part of the source of their criticism of American Sign Language as a vehicle for studying language in the chimpanzee - the signs resemble the things they stand for (iconicity). It is as if they require the animal to talk out of context and hopefully without reference to memory before pronouncing the act "language." Suppose an Englishman visited France and was able to communicate by constantly referring to an English-French dictionary (or a vocabulary list). Or suppose our traveler has used some mnemonics to learn words and could remember them only when external cues reminded him of his mnemonics. We would probably say that the person was not very facile, but not that he wasn't engaging in language. The issue of "iconicity" is a particularly big Pandora's box because it assumes, with no evidence, that we know the basis on which animals judge similarity (and how judgments of similarity are made is probably a more fundamental question than whether a particular communication meets a formalistic definition of language).

Part of the impetus to focus on language probably comes from the prevalent view that linguistics has rescued people (and might similarly rescue animals) from behaviorism. Thus there is a tendency to pit ideas like rules, which have figured saliently in linguistics, against ideas like generalization, which have figured saliently in associative and behavioristic theories. But these are not mutually exclusive ideas. A number of people working within associative frameworks would be quite comfortable with the notion that rules are applied to information that builds up according to associative principles. With respect to P&W's discussion, we would also suggest that the idea of rules is really no more specific than the idea of generalization. Finally, recent work on natural categories (e.g., Rosch, 1975) highlights the fact that many of our concepts do not have a neat

rule structure. The idea, therefore, that understanding and communication are governed by rules may not by itself take us much further than the idea that they are governed by generalization from past experience.

In a similar spirit, the SR&B paper seems to imply that appropriate use of signs or symbols is somehow not a good criterion for inferring what they call "semantic comprehension." From a functionalist point of view, it is hard to imagine a better criterion. However, it is generally true that the adoption of linguistic-style analyses of meaning for psychological purposes has tended to lead toward an overly "taxonomic" view of meaning. For example, suppose a man is shown an apricot, some wheat, and an apple, and asked to choose the odd item. If he grew up on a farm, he might say apple is odd because apricots and wheat both ripen in the spring and apples in the fall. We wouldn't want to say he responded nonsemantically because he didn't respond according to conventional taxonomic categories of fruits and grains. It would seem to be reasonable as a general approach to simply assume that animals are responding "semantically," and then to try to find out the basis of their responses. That is, how do they organize the world? An argument can be made that all signs are symbolic (even the dinner bell) and the interesting questions are: what do such signs signify to various individuals or species? how did they come to signify what they do? what are the implications of a given cognitive structure? and so forth.

Would any new ethical questions arise if animals could be shown to "have language" or to be "aware?" We see no reason why ethical considerations should wait upon, or be subordinated to, "openminded" scientific research that is conducted in strict accordance with Lloyd Morgan's canon. Indeed, in our treatment of animals as well as people we see every reason to adopt the opposite of Morgan's canon: "Assume until proved otherwise that others are just as intelligent, complicated, and so on, in their own way as you are in yours. And be very skeptical of your own motives and intellect if you think you have proved otherwise." As a matter of fact, even in some areas of scientific research this constitutes the more appropriate "null hypothesis." The study of "animal language," after the fashion of the target articles, may have, if anything, tended to increase rather than decrease expectations of human chauvinism and presumed "biological superiority," especially in the popular press, where it is more and more often suggested that chimpanzees, gorillas, and perhaps dolphins may deserve special consideration based on the outcome of research projects demonstrating their similarity to humans. The poet Robert Frost, noticing the apparent fear reactions of a paper mite, spared its life and wrote, "I have a mind myself and recognize / Mind when I meet with it in any guise." While we would question the usefulness of his statement if it were given as the principal conclusion of a scientific paper, it does not seem inappropriate as a starting point for ethics, or for a cognitively oriented ethology or animal psychology.

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Can the concept of cognition bear the weight psychologists place on it? [G, P&W] Today there is a lot of interest in experiments testing the so-called cognitive skills of animals. These experiments typically involve planning, tool using, complex expectations, and responses mediated by the use of what are allegedly symbol systems. The interest, however, centers often not on the intrinsic value of the work, but on its alleged connection with sweeping questions concerning the uniqueness of the human species, ethical issues, and the evolutionary hypothesis. The articles by P&W and G give us an opportunity to place the work on animal cognition into proper perspective.