

Good-bye Behaviorism!

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An ancient psychologist returning from a 60 year sabbatical in Katmandu or thereabouts and reading this volume of The Behavioral and Brain Sciences would experience an extraordinary sense of déjà vu taking him back to his graduate school days. That was a time when everyone was concerned with cognition and consciousness in nonhuman species—and not just that of the recently discovered great apes. Mr. Romanes (1885) for example, had already located the origins of consciousness somewhere between the Coelenterata and Annelida. Others vigorously and seriously debated the implications of demonstrations of learning in protozoa (e.g., Walkin, 1899) for theories of mind. The question of language in nonhuman species of course had continually been in the air from Descartes to Max Muller, and indeed for all we know our apocryphal centenarian psychologist might well have fled to the East to avoid speculation on the subject, rampant then as now:<sup>1</sup>

A great deal has been published in both magazines and newspapers during the past few years about the so-called “language” of animals, especially apes....in consequence there was given the widest publicity to an immense amount of the veriest nonsense, from which the average person who depends entirely upon his newspaper for his information is likely to have formed an entirely false conception of this very interesting matter (Gladden, 1914, p. 307).

In any event I am certain our psychologist would be familiar with most of the issues raised in this volume as well as impressed by the research discussed in the three papers. He might however be inclined to wonder what all the fuss is about; why devote a special issue to such mundane topics? We should then have to tell him about

Behaviorism and how Behaviorism tried convincing us, successfully in many cases, to forget about mental life, mind, and consciousness and to concentrate on pure Behavior. Pragmatically it advised its adherents to forget about common sense and not to bother with anything done by nonbehaviorists. We should also be pleased to point out that Behaviorism has been on the decline for several decades under pressures from such things as ethology, cognitive psychology, Noam Chomsky, John Garcia, “autoshaping,” and “constraints on learning”—not to mention an occasional mutiny (Herrnstein, 1977). Indeed the present volume might well be said to commemorate the end of Behaviorism.

#### Other Nonhuman Minds?

Prospects for a cognitive ethology Donald Griffins’s (G) paper sharpens the thesis developed in his book, The Question of Animal Awareness (1976). On one hand he calls for an end to the “behavioristic Zeitgeist” that inhibited inquiry into the mental life of organisms, and on the other, he suggests that extension and refinement of two way communication between ethologists and the animals they study offers the prospect of developing a truly experimental science of cognitive ethology. I am in considerable agreement with G on both of these points. If anything I think he understates the case against behaviorism; I have in mind the restrictive effects of removing hypothetical, causally effective constructs from the tools of scientific analysis and the practical difficulties in ascertaining such things as “behavioral repertoires,” “effective reinforcers,” or “histories of reinforcement.” Many psychologists simply do not see the point of giving up useful explanatory concepts, however mentalistic, for the sake of prescriptive scientific ideology. As Premack and Woodruff—certainly no strangers to

behaviorism—remark in their paper, how could they avoid mentalistic notions in their explanation of Sarah's behavior?

One thing I missed in G's paper is some recognition that behaviorism in fact did not completely snuff out all research into the mental life of animals. For example I see no mention of E. C. Tolman's purposive behaviorism with its suspiciously mentalistic notions of such things as cognitive map and hypothesis. It's my impression that Tolman's work not only gave a certain intellectual respectability to the rise of behaviorism but today serves as an important link between pre-behaviorist psychology, contemporary cognitive psychology, and G's cognitive ethology. Elsewhere—since behaviorism was primarily an American affliction—comparative psychologists and ethologists continued to seek empirical answers to the traditional questions about the minds of organisms, e.g., Katz (1939). Contemporary Soviet studies of the orienting response also seem to be directed at similar matters. It is just possible that G gives Behaviorism more discredit than it deserves.

The most important features of G's paper are his hortatory efforts to get psychologists and ethologists to look at the behavior of animals with an open mind and to stimulate research that might tap whatever mental life exists. In addition to what G has to say on the importance of two way communication between experimenter and animal, it may be useful not to overlook a cognitive interpretation (Tolman, 1932; Dulaney, 1968) of the traditional instrumental/operant conditioning paradigm as a communication paradigm. As Tolman pointed out long ago, the reinforcer, e.g., food pellet, has both affective and informational elements to it that are typically confounded in the behaviorist account.

Probably what excites G and others most about the recent developments with Washoe, Sarah, and Lana is the possibility that much more sophisticated quasilinguistic human-animal communication is just around the corner. One might imagine, although G was not so bold as to suggest it, that at some point we shall have direct Cartesian evidence that certain animals have a mental life analogous to ours. One might even fantasize that the structure of their quasilanguage should reflect the structure of their quasithoughts much as it is sometimes supposed that the structure of human thought is reflected in the structure of human speech.

At the present time however, there is very little reason to be overly concerned about any of this. First of all there are serious unanswered questions about the accomplishments of Washoe *et al* (e.g., Sebeok, 1978; Savage-Rumbaugh, Rumbaugh, and Bensen (SRB), this volume). Even if we accept the glosses given to the symbol-using apes by their interpreters at face value, there seems to be a striking lack of cognitive or intentional verbs, e.g., want, think, or decide. This may only be a temporary vocabulary gap; on the other hand it may prove extremely important in the search for nonhuman minds. For it is just these verbs and their sentential complements, (e.g., think that such and such) which are the foundations of the traditional linguistic analysis of other minds (e.g., Margolis, 1978). Moreover, it is just these complex constructions that inevitably mark the beginnings of syntactic speech in children between 2 and 3 years (Limber, 1973). I surely do not want to suggest on the basis of language alone that apes do not want, decide, or think. For example it is quite plausible that when Washoe signs "Gimme banana" she actually wants a banana. At a pragmatic (but not syntactic or semantic) level her signs may be functionally equivalent to the 18 month old child's "wa

nana” or the 3 year old’s “I want a banana.” However I do think these linguistic factors raise the possibility that apes may not have conscious access to their mental life in the same way that children do. Similar notions have been expressed quite independently of any specific linguistic considerations. Katz (1939, p. 253) for example, says “Man not only has consciousness, but he knows that he has it.” While Washoe may truly want a banana, she may not know it!

One should be careful about jumping to conclusions about the underlying basis for any putative consciousness level differences among primates. While they may exist independently of language as intrinsic cognitive differences, it may also turn out that language itself is in some way causally implicated in these differences. If this should be the case, training an ape in quasilinguistic skills may do more than let us study its mind as G suggests, it may in a sense create that mind.<sup>2</sup>

Do behaviorists have minds? I have maintained over the past few years to my students that probably the most significant effect of the various efforts to train quasilinguistic apes may be on the thinking of their behaviorist trainers. Premack and Woodruff (PW) bear me out in an interesting paper that reports not only on an important research program into the mind of Sarah but also provides an unusually reflective account of how the authors, on pragmatic grounds, were driven to formulate a primate cognitive psychology. The paper should be required reading for anyone interested in the traditional conflict between behaviorism and mentalism.

I have relatively little to say about the paper itself; the videotape paradigm seems most promising and I can only agree with the authors conclusions about the incompleteness of behavioristic accounts of behavior. Nonbehaviorists long ago

recognized the difficulty of fully characterizing even the simplest behavior without some intrinsically mentalistic concepts like purpose, want, or attention. PW suggest that the ape is not intelligent enough to be a behaviorist; in a sense, neither is homo sapiens. Behavior even in the simplest situations is a function of the organism's interpretation of stimulus conditions. Only with a model of the organism's mind can we generate predictions of its behavior in novel situations. Only through our theories of others can we overcome the egocentricism of childhood and positivism.

I do have various doubts about the PW program for examining "epistemic states" in the chimpanzee but at this point in time it seems proper to praise appropriate behavior rather than to criticize it. PW, I suggest, will have their hands full dealing with their colleagues and the ghost of Lloyd Morgan.

Some implications of symbol use by chimpanzees. The paper by SRB takes the trained apes and their acquired symbolic skills far beyond moot and irrelevant debates about whether apes can learn language. As I remarked elsewhere, the promising aspect of this research—the effects of symbol availability on animal behavior—had not yet been realized (Limber, 1977). Now SRB have reported what may be the first account of the utilization of arbitrary symbol systems in animal behavior. They have demonstrated that chimpanzees trained in a suitable symbol system can indeed use that system cooperatively to achieve ends well beyond their means without that system. I found the SRB paper thought provoking and will try, with some difficulty, to limit myself to brief comments on two interrelated issues—one concerns the functions of human language, the other its origins.

SRB, like most others who deal with animals and many who do not, take communication to be the “true adaptive function” of language, “enabling man to transmit specific information in abstract, context-free form.” Now it would be absurd to deny the importance of the communicative function of language; yet there is another function traditionally ascribed to human language, typically by logicians, philosophers, and cognitively oriented psychologists. This function concerns mental representation and thought itself. Without going into details here, it strikes me as premature to say the least, to suppose that the communicative advantages far outweighed any representational advantages in the evolution of human language. It seems, for example, that the representational capacity of English far outstrips its use in everyday interpersonal communication (cf. Limber, 1976). What is exciting about the SRB studies is that for the first time we can begin to think about experimentally studying the effects of mental

representations themselves on animal behavior. How would variation in the symbol system effect cooperation in the SRB paradigm? SRB's discussion of functional and nonfunctional naming capacities in chimpanzees immediately suggests intriguing experiments in the realm of "functional fixedness" (Duncker, 1935/1945). Numerous possibilities for investigating the recalcitrant problems of linguistic relativity and determinism suggest themselves. Many of the classic paradigms of experimental psychology, e.g., the delayed reaction experiment, double alternation problems, associative cue effects in problem solving, might be adapted to examine the effects of arbitrary symbol systems on behavior.

To sum up this point, the mental representation capacity inherent in human language seems at least as much of an adaptive advantage as does the communicative advantage. The SRB paradigm provides the potential to experimentally study adaptive advantages accruing to organisms having symbol systems of varying complexity.

It is not possible that a focus on the representational aspects of language will also give us a chance to break into the notorious problems of language origin. (Even formulating the issue adequately is difficult (cf. Kenny, 1973); among the traditional answers to this question at least Sussmilch's "divine origin" theory lays the cards on the table). There seem to be two fundamental obstacles to the problem. One is to understand how the rule governed, arbitrary symbol system that is human language came to exist among a population with no previous system; another is to enumerate whatever conditions—biological, cognitive, social, environmental—support human language, yet might have evolved in its absence. If however we reasonably suppose adaptive advantages came to those organisms with increasingly complex intrapersonal

representational systems, the fabled “private language” or “language of thought,” then we can postpone for a while consideration of the really tough problem of the externalization of that system into the social phenomenon of language. On this problem it may be that the SRB and PW papers offer some small pieces of the puzzle. It appears that SRB’s chimpanzees had little difficulty using their symbol system once given to them; there seemed to be a preexisting empathic relationship, initially developed between experimenter and animal, then readily generalized to other chimps. This suggests exactly what PW clearly demonstrate, namely that chimpanzees have quite elaborate theories of others. Now the adaptive advantages of such theories seem obvious; they enable an organism to understand the behavior of others, more importantly they probably underlie the enormously advantageous process of observational learning. Learning from another surely requires an elaborate conception of another in relation to one’s self. Now observational learning, perhaps evolved in connection with mechanisms of socialization (also see Beck, 1974), is important in connection with language origins in that its processes of rule induction and rule following resemble those processes required in the acquisition and use of language. In contrast to natural laws causally governing behavior, rules can be violated, modified, and become objects of awareness. For all I know, one of our clever ancestors whose population had independently acquired a certain representational capacity, communication skills at an animal level, and empathic learning ability just invented language serendipitously and it spread like wildfire. Naturally her invention need not have been structurally like ours; considerable subsequent cultural evolution may have taken place.

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## Footnotes

<sup>1</sup>We cannot be entirely certain of this; he just as well might have gone off to investigate the language of the Yeti.

<sup>2</sup>Some of these issues are hinted at in Limber, 1977 and discussed at greater length in my monograph in preparation on language among primates. Everyone should also read Vygotsky (1962, Chap. 4).