

## Aberance in Science: Should Behavior Analysts Know About Psychological Behaviorism?

Arthur W. Staats  
University of Hawaii

Plaud's main point in criticizing psychological (paradigmatic) behaviorism is that it adds nothing "to the study of behavior that is not already provided by radical behaviorism" (1995, p. 167). Let me put this in context by pointing out that Ulman, in contrast, states the two are quite different, with an "inherent incompatibility between radical behaviorism and paradigmatic behaviorism" (1990, p. 29). However, another behavior analyst suggests that the two behaviorisms did interact and psychological behaviorism did contribute. "At the beginning (1962), the PhD program at Arizona State was experimental analytic. . . . Ayllon and Michael (1959) provided an isolated example of applied work. . . . Art Staats had just begun his work in reading (e.g., Staats, 1965)" (Osborne, 1995, p. 247). These conflicting opinions are representative of a widespread phenomenon; many contemporary behavior analysts generally have little contact with psychological behaviorism and know little about its past relationship to radical behaviorism and behavior analysis or its present character. Let me suggest that the behavior-analytic evaluation of psychological behaviorism is marked by a profound communication problem; in my view, it constitutes one of those noteworthy cases of aberrance in science. Let me give a few examples, first by correcting Osborne's historical note in a manner that also is relevant to Plaud's criticism as well as Ulman's.

The behavioral program at Arizona

State actually began in September of 1955, when I arrived as a new instructor, continuing my program of applying conditioning principles to the study of human behavior. In that year I wrote up the data I had collected at UCLA for my dissertation (*A Behavioristic Study of Verbal and Instrumental Response Hierarchies and Human Problem Solving*), further developed a research project analyzing language in terms of classical and operant conditioning principles (supported after 1956 by the Office of Naval Research), and began constructing a behavioral teaching program. I designed the token reinforcer system and a set of reading materials in 1958, and with the assistance of Richard Schutz, Carolyn Staats, Karl Minke, and Judson Finley conducted a project in a public school teaching children with learning deficits to read. That work was funded by the U.S. Office of Education in 1960, and Mont Wolf was a graduate assistant on several published works in my continuing reading study (e.g., Staats, Finley, Minke, & Wolf, 1964; Staats, Staats, Schutz, & Wolf, 1962). By the 1962 Osborne refers to, I had been a behaviorist for 10 years. Taking a clinical as well as experimental PhD, and with a focus on studying complex, functional human behavior, I had early on discarded Hull's theory in favor of employing the fundamental principles of conditioning in naturalistic as well as formal research. It was on the basis of this experience that I explained (Staats, 1957) the clinical report of a schizophrenic patient's abnormal behavior (see Laffal, Lenkoski, & Ameen, 1956) to be the result of inappropriate reinforcement by the hospital staff. My analysis said treatment should consist

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Correspondence and requests for reprints should be addressed to Arthur W. Staats, Department of Psychology, University of Hawaii, Honolulu, Hawaii 96822 (E-mail: staats@hawaii.edu).

of extinguishing the undesirable behavior and reinforcing normal behavior, principles that with Ayllon and Michael's (1959) support became a foundation for behavior modification. In 1957 I designed a teaching laboratory for my undergraduate behaviorism course, each pair of students working with a rat using an operant apparatus. I was especially interested in behaviorists who applied conditioning principles to human behavior and got our chairman in 1960 to hire Izzie Goldiamond (whose work on perception impressed me) and my graduate student friend Jack Michael. I then gave my teaching lab to Jack, while I continued to teach the lecture part of the course, which was basic in our program. Gray Osborne, although not aware of this psychological behaviorism history, makes the important point that the radical behaviorism and psychological behaviorism interaction goes back to the beginning of the modern era.

Let me elaborate this a bit, with respect to human applications that used the method I first labeled *behavioral analysis* (Staats, 1963, pp. 459–460), a term Mont Wolf used in 1968 for the *Journal of Applied Behavior Analysis*. Besides the token reinforcer system (as I called it), my reading project introduced the behavioral study of developmental disabilities. In addition to the specific studies, in my 1964 articles and my 1963 book *Complex Human Behavior*, I projected the need for extending the token reinforcer methods and behavior-analytic principles widely to “special populations of children” (Staats, 1963, p. 456). I said that “many children labeled as mental retardates or autistic children are only victims of poor training conditions” (Staats, 1963, p. 456). “The present methods would . . . be useful in the study of the acquisition of complex behavioral repertoires of immediate significance to human adjustment . . . [such as] remedial reading programs, the training of autistic children, . . . deaf children, mutes, etc.” (Staats et al., 1964, pp. 146–147). I also stated

that my behavioral methods could be used by various professionals (teachers, social workers, child psychologists, and clinicians) as well as subprofessionals. There was no counterpart in Skinner's radical behaviorism for this early blueprint (see O'Leary & Drabman, 1971). To trace the lines of development a bit further, Mont Wolf introduced the token reinforcer system and the reading training methods to Sid Bijou's project with mental retardates (see Birnbrauer, Bijou, Wolf, & Kidder, 1965). As Bijou and colleagues indicated, with social reinforcement they had obtained “little, if any, improvement in sustained studying behavior,” but their introduction of my token reinforcer system “did indeed establish and maintain higher rates of effective study” (Bijou, Birnbrauer, Kidder, & Tague, 1967, p. 512). And with respect to what was taught, Bijou (1965, p. 73) said “instruction in sight vocabulary [was] patterned after the work of Staats, Staats, Schutz, and Wolf (1962).”

There are other equally important cases in the literature of psychological behaviorism developments that have become part of behavior analysis. Compare, for example, the radical and psychological behaviorism works up to 1963 with respect to the behavioral taxonomy of abnormal behavior, the need for a “learning psychotherapy,” and the attached call for assessment (see Staats, 1963, chap. 11) that became foundations for the fields of behavior modification and behavioral assessment (see Goldfried, 1976; Goldfried & Sprafkin, 1974; Silva, 1993). Psychological behaviorism also presented various specific analyses of behavior and behavior problems—such as the analysis of the learning of the plural morpheme (Staats, 1963, pp. 177–178) and toilet training for children—that were later elaborated in behavior-analytic works.

Plaud (1995) is right in part: There is much that the two behaviorisms have in common. It is important to understand that this is true of the various

behaviorisms (in comparison to cognitivism)—there is a body of empirical principles and findings, a basic philosophy of science, and a methodology that are common to all. For example, a study demonstrating how reinforcement principles apply to a human behavior supports Skinner's radical behaviorism, Hull's behaviorism, and cognitive behaviorism, as well as psychological behaviorism. To illustrate, when I began applying the principle of reinforcement (and also classical conditioning) to human behavior I knew nothing about Skinner's behaviorism. Furthermore, with respect to radical behaviorism and psychological behaviorism, there has been much interaction, in both directions, for almost four decades. How could it be otherwise?

However, Ulman (1990) is also right, in the sense that there are differences between psychological behaviorism and radical behaviorism, in philosophy of science, in worldview, in theory, in methodology, in analyses made of different behaviors, in theory-specific findings, and in directions for development. Plaud, however, still asks for indication of differences, although the article his comment addresses describes a number that are fundamental. For example, psychological behaviorism's full, *behavioral* theory of personality, with its program of research and a broad agenda, is presented as a major difference. As another fundamental example, Skinner (see 1975) treated classical and operant conditioning as independent, and did not consider in a related way the various functions that stimuli can have that are affected by motivation (deprivation-satiation) operations. Psychological behaviorism's basic principles treat the several functions of stimuli and the effects of motivational operations as part of relating the interactions of classical and operant conditioning (see Harms & Staats, 1978; Staats, 1975; Staats & Hammond, 1972; Staats & Warren, 1974). Michael (1993) points to the limitations of radical behaviorism on this fundamental level and introduces his

establishing operations theory as another means of repairing the same deficits. As another example, the behaviorizing program and methodology of psychological behaviorism, and the multilevel theory construction formulation, are contrasted to radical behaviorism's corresponding characteristics. Finally, psychological behaviorism's empirical approach to the study of language is contrasted with Skinner's *Verbal Behavior*. That Plaud will not address these differences, while criticizing an alleged absence of differences, is indicative of the problem the present note addresses, for the differences need systematic study and comparison to establish the productive paths for behaviorism to pursue.

Finally, Osborne's (1995) historical statement is also correct in exposing something that is presently not generally understood. One of behavior analysis's pioneer programs, called "Fort Skinner in the desert," actually was as much based on psychological behaviorism as it was on radical behaviorism. What does this indicate about behaviorism today? This note can only suggest that psychological behaviorism constituted an integral part of behavior analysis, and plays a role today, but could contribute much more if the avenues of communication were systematically established. As it is, most behavior analysts do not have adequate contact with psychological behaviorism and thus cannot exploit its heuristic value. Ordinarily scientists in a field know about relevant developments in that field—in this case behaviorists as a group should know about developments in behaviorism. That this is not the case requires examination, because the development of behaviorism in its competition with cognitivism calls for study and use of all that our field has to offer.

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