Goal-Directed Behaviorism?

A Review of

The Goal of B. F. Skinner and Behavior Analysis

by R. W. Proctor and D. J. Weeks

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Behaviorism has always been controversial; behaviorists have participated often in discussions both of the principles discovered in laboratories and of the broader implications of these principles. Criticisms of behaviorism have ranged from scholarly disagreements to diatribes, sometimes in the same forum (Wann, 1964; Wheeler, 1973). More recently, it seems that scholarly disagreements have diminished in number and visceral attacks rule the day (e.g., Mahoney, 1989). Such attacks, like racist humor, betray a more general attitude that impedes reasoned discussion, even when the specifics are obviously wrong. They leave a residue of influence that outlives specific errors.

Proctor and Weeks's book escalates the battle and then offers a comprehensive solution to the problem apparently posed by behaviorism: Eject behavior analysts from leadership roles in scientific psychology and any organization that claims to represent science. They defend this recommendation by arguing that behavior analysts are intolerant "pseudoscientists," a claim supported with judiciously selected quotes, often out of context, from behavior analysts.

The book is part of a series called "Recent Research in Psychology" published by Springer-Verlag. This series publishes camera-ready manuscripts that are "preliminary drafts of original papers and monographs; technical reports of high

quality and broad interests; reward winning theses" and conference reports "quickly, informally, and professionally." The publication of this book in a relatively uninfluential outlet raises a question as to whether it deserves attention from behavior analysts. The answer is "yes." Publicly expressed misrepresentations require attention because they may be shared and promulgated by others (Todd & Morris, 1983). They reveal how other psychologists view behavior analysis and should be confronted directly. The book by Proctor and Weeks, more than any in recent memory, pulls together and attempts to document opinions often spoken but rarely printed and therefore difficult to address. Here, such opinions are presented in a single publication.

The book under review contains three parts, and each part contains three chapters. The first part is entitled "What is the Experimental Analysis of Behavior and Why Has It Not Become Psychology?" The description of behavior analysis in this section includes a fairly sympathetic treatment of some successes in applied behavior analysis and an introductory-level account of the research strategy used in experiments. It also attempts to place the experimental analysis of behavior in a larger context formed by scientific psychology, and then argues (using quotes) that the experimental analysis of behavior has been a failure. The second section is entitled "Relation of the Experimental Analysis of Behavior to Psychology, Science, and Pseudoscience" and contains their argument that the experimental analysis of behavior is a "pseudoscience" shared by members of a "paranoid pseudocommunity" that long ago lost interest in experiments. The third

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and last section is entitled "Implications and Future Prospects" and raises dark prospects for scientific psychology if behavior analysts are allowed to assume leadership roles.

(Mis)representations of behavior analysis. Proctor and Weeks claim that they undertake a "detailed examination . . . with the goal of understanding the behavior-analytic conceptions of science and psychology" (p. xiii). They further claim that "as experimental psychologists [they] approached this project as [they] would in investigating any other behavioral phenomena" (p. xv). Citations indicate that others share their perception of behavior analysts. The number of errors contained in their examination is too great to permit each one to be confronted, or even listed. Some that are pertinent to their recommendations that behavior analysts be blocked from leadership positions in scientific organizations include (a) behavior analysts are contemptuous of peer review (pp. 85–91) and the leading journals do not practice it (pp. 91–95); (b) the applicability to humans of the three-term contingency has no empirical basis (pp. 77–81); (c) behavior analysts have no interest in the "truth value" of their claims, only in their "survival value"; therefore, they will do or say anything to survive (pp. 141–146); (d) behavior analysts feel that conceptual analysis is sufficient, so experimental analysis is unnecessary (pp. 139–141); (e) behavior analysts have no interest in conducting human research, only in applying dogma to human problems without concern for effectiveness (pp. 131–135); and (f) there is no interest in quantifying the principles of behavior (p. 55). An examination of two of these conclusions can illustrate the lack of support for their general thesis.

Peer review. According to Proctor and Weeks, "A general disregard for peer review and evaluation is evident in Skinner's training and career, as well as in the field of TEAB [the experimental analysis of behavior]. . . . The apparent lack of appreciation for the peer review process demonstrated by Skinner was carried over into the field as a whole. . . . [the *Journal*

of the Experimental Analysis of Behavior was] founded primarily to circumvent the editorial process" (pp. 86, 90, 92).

The charge that peer review is not practiced derives partly from E. G. Boring's description of Skinner's doctoral dissertation conducted in the 1930s and largely from reading some of a special section in the 30-year anniversary issue of the *Journal of the Experimental Analysis of Behavior*. In that issue, members of the original board of editors recalled the founding of the journal. One, Charles Ferster, had initially suggested that the journal should publish reports as they are submitted. That suggestion was overruled; authors who have submitted a manuscript to the *Journal of the Experimental Analysis of Behavior*, the *Journal of Applied Behavior Analysis*, or *The Behavior Analyst* can certainly confirm that.

While asserting that behavior analysts do not practice peer review, Proctor and Weeks simultaneously assert that behavior analysts *do* practice it, but do so incorrectly, because the scrutiny of "behavior analysts comes from other behavior analysts" (p. 91). This puzzling and contradictory charge suggests a misinterpretation of the term "peer review." It is the practice of most journals that practice peer review to have experts review their peers, as implied in the term itself.

The conclusion by Proctor and Weeks that behavior analysts are contemptuous of peer review is enlightening, not because it is so clearly wrong, but because it undermines their larger claim that they drew from the writings of behavior analysts to describe their view of science. Proctor and Weeks isolated damaging quotes about peer review, but further reading would have revealed that Ferster's initial opinion was not representative of the founders or of other journal editors, and is clearly not representative of current or past practice.

Interest in empirically based descriptions of human behavior. Proctor and Weeks claim that behavior analysts are uninterested in extending experimental analyses to humans, to comparisons of different species, or to other areas of psy-

chology. These claims can be dismissed by simply reading the table of contents (or even the papers) contained in any randomly selected issue of the *Journal of the Experimental Analysis of Behavior*. Mere perusal should quickly convince any observer that the field is empirical, quantitative, and rigorous. If a more substantial counterargument is necessary, it can be found in the "Special Issue on the Experimental Analysis of Cognition" (White, McCarthy, & Fantino, 1989), published the year before Proctor and Weeks's book and advertised during the preceding year. This issue contains 16 empirical papers, of which 4 studied humans (young, middle-aged, and old), 2 studied monkeys, and 10 studied pigeons. Three theoretical articles are also included in this issue. One, by Turvey, Solomon, and Burton, is based on James Gibson's ecological approach to perception based on human haptic perception; a second, by Wixted, is an examination of nonhuman remembering; and a third, by Kehoe, discusses connectionist models of conditioning.

Proctor and Weeks also charge that the three-term contingency is not supported empirically in humans: "In sum, the behavioral principles derived from the animal research have not been verified to any great extent in the basic human research" (p. 81). Their summary is of several quotes taken from behavior analysts arguing for an acceleration of the extension of basic research to include human subjects. Although it may be true, for example, that the particulars of fixed-interval responding do not always appear in human behavior (or nonhuman behavior) in some settings, the empirical support for extending such fundamental principles as the three-term contingency can be found in a vast open literature or in countless undergraduate laboratory projects.

A recent article (Baron, Perone, & Galizio, 1991) and subsequent discussion, published since Proctor and Weeks's book, illuminate many of the issues surrounding attempts to relate the experimental analysis of human and nonhuman behavior. But behavior analysts'

approaches to these issues are not limited to direct application of the experimental method to humans. They also take the form of confronting data produced by a broader collection of investigators. Consider a textbook published before Proctor and Weeks's book by A. C. Catania (1984), a former president of the Association for Behavior Analysis. In six chapters (out of 14), he presents data from studies of human cognition and attempts to integrate cognitive and behavioral approaches. The time and effort required to do this is testimony to the respect that this behavior analyst has for the data and issues raised by cognitive psychologists, as well as for empirical work in general. Catania is certainly not alone in this interest.

A desire to build upon a solid foundation of empirical work conducted with nonhuman animals may represent an important distinction between experimental analysts of human behavior and other experimentalists. Respect for animal research is not widespread in psychology, which may be the only life science (if it is that) in which eminent participants can publicly display ignorance of, and even contempt for, the literature on animal research without embarrassment. In other life sciences, people are *influenced* by animal research. In psychology, one can be in the mainstream without knowing principles derived from animal laboratories or attributing any importance to them. Ignoring the literature on animal research may even be regarded as a virtue (Rollin, 1985), despite the many benefits that have derived from such research (Miller, 1985). Even in behavior therapy journals, where principles derived long ago in animal laboratories appear routinely, references to studies using nonhuman animals are rare (Kelly, 1986). To provide a context, imagine the treatment that a neuroscientist would receive if he or she asserted that nothing can be gained from studying animals and therefore will neither read nor refer to anything using animals as subjects.

Intolerance. Proctor and Weeks assert that behaviorists should no longer be tolerated in science because behaviorists are

intolerant. After claiming that behaviorists are intolerant (a claim probably true of as many behaviorists as nonbehaviorists, to be sure), Proctor and Weeks suggest that anyone tainted with the behaviorist brush should be prevented from taking a leadership role in the American Psychological Society (pp. 180–186). They stop short of arguing that such people should be excluded from the society altogether, but the implication is barely avoidable. They don't specify their criteria for exclusion, but one could imagine wrongdoings ranging from sympathetically delivering a lecture on conditioning to holding a leadership role in the Association for Behavior Analysis. When reading this section of their book the phrase "Are you now or have you ever been . . .?" is easily recalled.

At least three responses to the charge of intolerance are possible: (a) point fingers at psychology, and cognitive psychologists, for ignoring and eliminating from departments research on animals as well as the experimental analysis of behavior; (b) agree, because intolerance impedes reasoned discussion; or (c) consider whether a global condemnation of intolerance is justified.

The first response is only partly correct. Animal research and the experimental analysis of behavior thrive at some departments (Lattal, McFarland, & Joyce, 1990). The second, also, is only partly correct. Intolerance is not a lone voice but part of a cacophony that prevents truth from emerging. The third alternative is worthy of consideration. A universal condemnation of intolerance may go too far; indeed, there may be some virtue to it. Is there intolerance in science? There certainly is, and it may even play a role in producing change. Scientific thought has developed partly through the intolerance by some of those who wish to preserve the old ways (King, 1963; Kuhn, 1962) or who advance groundless claims about nature. Sometimes advances are possible only because of individuals whom many of us would call intolerant. (For one of many possible examples, see King, 1963, for a delightful

discussion of Vesalius, a "splenetic, prejudiced, and ungenerous" man.)

Intolerant people are not always pleasant to be around, but they have made important contributions. Such individuals may even be necessary to instigate change. However, more common displays of intolerance among scientists are directed at superstition, errors of fact, and, sometimes, ideas, both old and new (Kuhn, 1962).

There have been important *scientists* who were intolerant, but Proctor and Weeks are correct in saying that the scientific community actively supports and maintains forums for the free discussion of new ideas and the advancement of knowledge. In these forums, empirical observations of nature are of paramount importance. Such building on firm empirical foundations and the entertainment of reasonable ideas represent the tolerance of which scientists (including behavior analysts) are proud.

Summary. Considering the generally poor scholarship of Proctor and Weeks's book, the question arises as to why the book is worth paying attention to. The reason is that its appearance may reflect a deep-seated misunderstanding of a successful enterprise whose purpose is the understanding of behavior. The ignorance that often lies behind the charges directed at behaviorists leads to bewilderment on the part of behavior analysts at much of what passes for criticism. This issue is not confined to Proctor and Weeks's book. Mahoney (1989) recently asserted that behaviorists circulate lists of books that graduate students must be forbidden to read. That this ridiculous charge passed peer review in psychology's major professional journal is testimony to the fact that past misrepresentations of contemporary behaviorism are becoming felt in critical analyses. Now, Proctor and Weeks advocate a systematic purge based upon the "facts" that they present. It should be enough to discredit those "facts," but it probably is not. The public perception, even among psychologists, of behavior analysis is sufficiently distorted by published works and bigoted

comments that their suggestion may take root.

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