In Response

Methodological Commitments and Other Philosophic Necessities

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Behavior analysts have engaged in debate with other psychologists for decades (e.g., Wiest, 1967; Catania & Harnad, 1984). The time and effort that this debate has required might have been better spent on behavior analysis itself. But, clarification is often necessary, especially when misconceptions about the nature and purpose of behavior analysis are published in our own journals: Hence, an earlier paper (Lee, 1989), which was written in response to comments by Proctor and Weeks (1988) about a paper by Harzem (1987), and the present paper, which was written in response to further comments by Weeks and Proctor (1990).

Behavior analysts have a methodological commitment, as Weeks and Proctor have suggested. But, this fact should not let us forget that mainstream psychologists also have a methodological commitment. In fact, one critic (Koch, 1974) implied that mainstream psychologists have a greater commitment to their methodology than to their subject matter, and another critic (Smith, 1989) suggested that graduates of psychology departments know more about a research methodology than about a subject matter. Although behavior analysts are not alone in having a methodological commitment, they are alone among psychologists in having a sustained tradition of investigating the behavior of individuals in context and through time. Weeks and Proctor (1990) suggested that this latter commitment promotes the isolation of behavior analysis and is counterproductive. But, if behavior analysts abandoned their methodological commitment in order to reduce their isolation, they would have to embrace another commitment, one that has little to do with their task of checking and extending knowledge of

behavior principles and of applying behavior principles to particular aspects of the content of human conduct.

Weeks and Proctor said that behavior analysts are obliged to account for all behavioral data. In support of this, they have quoted the statement by Sidman (1960, p. 3) that "good data are always separable from the purposes for which they were collected." But Sidman's statement was about *good data* and does not support the conclusion that behavior analysts are obliged to account for *all behavioral data*. The meaning of "good data" from a behavior-analytic perspective can be grasped by reading Sidman (1960) and Johnston and Pennypacker (1980). (Incidentally, the term "cognitive data" (Lee, 1989) was used, not without knowledge of the fact that much of the data collected by psychologists are behavioral data (Weeks & Proctor, 1990), but rather as a shorthand reference to data that have been collected by psychologists for the purpose of understanding the hypothetical system inside the head of the individual. Such data are seldom good data even if some aspects of the behavioral phenomena might interest some behavior analysts.)

One response to data collected from outside the behavior-analytic tradition has been to explore the phenomena of interest in an appropriately behavior-analytic way. One example is the study by Burgess (1968) of the behavioral effects of communication networks in which individual members of a group assemble information, make a decision, and issue an order under constraints determined by the experimenter. After reviewing the relevant literature, Burgess commented that the design of previous studies had taken into account neither the distinction

between stable behavior and transition behavior nor the effect of the consequences of current behavior on subsequent behavior. When Burgess conducted experiments which took these variables into account, he found no difference in performance under two kinds of communication networks once performance was stable. This investigation of the developmental histories of social groups under explicit contingencies led to results which made sense from a behavior analytic point of view and which contrasted with the inconsistent findings of the relevant psychological literature. Other examples of this approach toward the data of traditional psychology have been described by Harzem (1984) and Baron, Myerson, and Hale (1988). These examples illustrate the view that the task at hand is not to debate the interpretation of data collected by other psychologists but rather to extend a contingency-oriented psychology by collecting new data which reflect successive episodes in the behavior of individuals in context and through time.

Weeks and Proctor (1990) noted that behavior analysts need not abandon their ontological commitment but that they should abandon the ideological stance which leads them to deny the possible validity of other approaches. But the fact is that it is inconsistent to hold a commitment to a contingency-oriented psychology and at the same time to accept other positions that ignore, reject, or contradict this commitment. The rejection of these other positions is a matter of internal consistency and not of ideology.

Weeks and Proctor said that they intended to recommend that behavior analysts should become familiar with psychological research in the broad sense. Interpreting this recommendation to mean that behavior analysts should become familiar with cognitive psychology (Lee, 1989) seems reasonable when the terms "cognitivism" and "cognitive psychology" have been used elsewhere to refer to the traditional approach which locates the subject matter of psychology inside the individual. For example, as Sampson (1981) pointed out, this as-

sumption about the location of the subject matter of psychology underlies most psychological research and theory. From that perspective, the mainstream of psychology is cognitive, as the term "cognitive" is understood here.

Weeks and Proctor stated that scientific disciplines can acquire knowledge relatively independently of philosophical debates and that progress can be made by letting empirical evidence, and not philosophies, guide research. They also stated that only explanations of data and not the data themselves are theory laden. But, this view, which puts observation ahead of theory and gives no place to philosophical inquiry in science, is only one view about the nature of science.

An alternative view is that the empirical and conceptual aspects of scientific inquiry are inseparable. In this view, what we know already, whether explicitly stated or not, influences the observations we make and the data we collect. It cannot be otherwise. We have a choice only in whether we examine what we already know. We can examine what we know with both empirical and conceptual tools. Using empirical tools, we can check what we know directly against the subject matter; using conceptual tools, we can improve the internal consistency of what we know, we can explicate its assumptive base, and we can explicate its ramifications.

The inescapability of prior know-how and prior knowledge (i.e., contingency statements; Lee, 1985) cannot be overemphasized. We cannot talk about our subject matter free of prior commitments. We hold philosophical commitments, whether we acknowledge them or not, at least in the trivial sense of declaring that philosophy does or does not have a place in science and, certainly, in the more profound sense of doing our psychology as if psychology's subject matter consisted of behavior and its context and not of the organism and its contents (or vice versa). We have commitments to conceptual frameworks, whether we acknowledge them or not, even if only in the sense that we inevitably classify everything we talk about because classi-

fications are embedded in the language we speak. So much the better if we make these commitments explicit and if we examine them. Denying these commitments, refusing to examine them, and insisting the science is best done without them will not free us from them.

Of course, if we do not examine our commitments, then we might still produce voluminous collections of data, and our colleagues might judge us at least as productive researchers. We have available the methods of research that psychologists initially borrowed from agricultural statistics, and we can use them without pondering the general nature of psychology's subject matter and task. But these methods do not follow from the known properties of behavior as a subject matter in its own right, and they have not led to collections of thematically-inter-related data. Like the collections of data noted by Burgess (1968), the resulting collections of data have often turned out to contribute only a body of conflicting and inconsistent findings that have not presented a coherent and illuminating picture of the phenomena of interest.

At this point in the history of psychology, we can do better. We know, among other things, that our subject matter consists of units that comprise bodily effort and contextual variables (i.e., actions; Lee, 1988), that it consists of complex networks of n-term contingencies (e.g., Sidman, 1986), that its most elementary properties are time and count (e.g., Johnston & Pennypacker, 1980), and that one of its basic characteristics is captured by the distinction between form and function (e.g., Carr & Durand, 1985; Goldiamond, 1974). Research methods that are consistent with these features of behavior have produced sets of data that have an orderliness that is unusual in psychology.

The problem for behavior analysis is that no amount of orderly data collected with methods consistent with a contingency-oriented psychology will convince other psychologists that a contingency-oriented psychology is worth pursuing. Other psychologists seem often enough not to understand why anyone would

want to collect such data, and they probably will not understand until they have grasped the idea of a contingency-oriented psychology. Grasping that idea and becoming fascinated with its empirical and conceptual ramifications requires an involvement in foundational inquiries (i.e., in philosophy), and so we return, once again, to the barrier that has too often discouraged scrutiny, explication, and revision of the assumptive base of psychology. We also return to the barrier that has excluded behavior analysts from participation in general psychology. That barrier turns out to be as much philosophic as scientific: It has to do with fundamental assumptions about the nature of psychology's subject matter and task and with the conceptual and empirical ramifications of those assumptions.

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