

**THE EXODUS OF BEHAVIOR ANALYSIS:  
IS "SPLENDID ISOLATION" THE WAY TO GO?**

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Psychology as a discipline is theoretically fractionated and has neither a generally agreed upon set of defining principles that determine its subject matter nor a common methodology that guides research. In the absence of a unified framework, psychology is therefore best viewed as a group of subdisciplines that can be distinguished by a conceptual approach (biological, behavioral, cognitive, humanistic, or psychodynamic) and content area (e.g., developmental psychology, learning, memory and cognition, or abnormal psychology). Each subdiscipline has its particular view of what counts as subject matter, what questions should be asked about this subject, and how one should go about finding answers to these questions (Lee, 1988). Despite this lack of agreement, I consider it reasonable to say that academic psychologists, regardless of their theoretical persuasion, adhere to an *empirical epistemology* and are committed to the principles and practices of *science*, albeit they are not all committed to the *same kind* of science.

I assume Lawrence Fraley would disagree with me on this latter point because he chooses to characterize psychologists who subscribe to a traditional (meaning nonbehavioral) perspective as members of a "scientized" rather than scientific community. According to Fraley's viewpoint, these misguided individuals are spending their days studying the "interface between the metaphysical and physical worlds thought to reside in the mind of man," while behavior analysts in the meantime either are caught up in a futile struggle of "making over psychology," or are busy with practicing *true science* with the goal of developing a strictly natural science of behavior-environment relations that will gain them admission to the "coalition of natural sciences."

Aside from its unnecessarily sarcastic tone, Fraley's juxtaposition of traditional psychology as a "pseudo science" and behavior analysis as a "truly scientific" discipline reveals either a misunderstanding or a deliberate rejection of the currently widely accepted conception of "science." According to this conception, behavior analysis differs from much of contemporary psychology in that its philosophical foundation, radical behaviorism, promotes an inductive rather than hypothetico-deductive scientific method, with the person standing in a dependent-variable relation

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to environmental contingencies that are the independent variables (Chiesa, 1994). Stated differently, behavior analysts explain behavior in terms of interactive relations between individuals (or nonhuman organisms) and their environment. In contrast, much of contemporary psychology (be it in the developmental, cognitive, social or clinical area) treats cognitive events and hypothetical internal mechanisms as independent or intervening variables. Thus, the most basic disagreement between behavior analysts and other psychologists hinges on the question of whether cognitive variables can have causal status. This is an epistemological question that cannot simply be resolved by "empirical facts" because the answer depends on basic philosophical views of the goals and truth criteria of science, assumptions that guide the scientist's analysis but which themselves are logically pre-analytic. Regardless of whether we are dealing with quantum physics or psychology, data are not independent of the scientist's conceptual assumptions, because scientific and philosophical considerations are inextricably intertwined (e.g., Feyerabend, 1970; Kuhn, 1996; Quine, 1953). As there are neither "correct" ultimate definitions of science, truth, and causation, nor crucial tests that can be applied to distinguish between competing conceptualizations, nothing is gained by accusing one's conceptual rivals of "mysticism" who pertain to a "scientized community" or by leading a crusade against nonbehaviorists whose theories are built upon explanations of behavior in terms of internal processes.

For nonbehavioral psychologists, the objective is to predict and *understand* behavior. Typically, their truth criterion is correspondence and the focus of their analyses is on internal psychological mechanisms (e.g., schemata, beliefs, attitudes, expectancies) that are thought to causally mediate between environmental inputs and behavioral outputs. Behavior analysts, on the other hand, view prediction and *control* as the goal of science. As they endorse a pragmatic truth criterion of "effective action," by necessity, they must begin the causal analysis with external events that can be manipulated. Given that scientists within each field conduct their work according to epistemological criteria that are incompatible with those of their rivals, their philosophical assumptions lead to different analyses and interpretations. Thus, neither behavior analysts nor traditional psychologists are persuaded by each other's arguments or data, and each side is simply behaving consistently with its own philosophical stance (Hayes & Wilson, 1995).

The conceptual difference between nonbehavioral and behavioral psychologists notwithstanding, in most academic psychology departments there is room for diversity and peaceful coexistence. After all, there is as much conceptual variety among those subdisciplines that Fraley has lumped together under the label "psychology" as there are theoretical differences among various branches of behaviorism (e.g., behavior analysis, social behaviorism, pragmatic behaviorism, teleological behaviorism, and associationism, to mention but a few). While most psychologists, including the majority of behaviorists, simply go about their everyday scientific business, every so often grumbings resound from the camp of the behavior analysts. Fraley's call for a segregation of behavior analysis from mainstream psychology is therefore nothing new (see, for example, the symposium on "Teaching behavior analysis: Should we have graduate programs in behavior analysis?" held at the 1996 conference of the Association for Behavior Analysis (ABA) in San

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Francisco). Over the years, there have periodically been advocates for developing an independent behavior-analytic discipline outside of mainstream psychology because, the saying goes, nonbehavioral faculty not only fail to appreciate the presumed superiority of a behavioral approach but actively impede its progress by maligning its practitioners and turning students against it.

After reading Fraley's paper, I wondered why anyone should be surprised that *some* behavior analysts have difficulty feeling at home in mainstream psychology departments and garnering the respect of their colleagues. Is it true that psychology departments promote professional conduct among their faculty "that renders shameful any serious criticism of what another faculty member is teaching" (p. 6)? Or is it that in most departments (psychology or otherwise) faculty would be shunned if they expressed *arrogant contempt* for the work of their colleagues and used *ad hominem attacks* on their colleagues' intellectual competence and scholarship for holding rival theoretical positions?

I am not unsympathetic to the plight of those behavior analysts who indeed work in a hostile environment. Behaviorists have no monopoly on intolerance! Some nonbehavioral psychologists are quick to categorize behavioral conceptualizations and interventions as superficial, simplistic, mechanistic, or even inhumane, and use vitriolic rhetoric that easily matches that of their behavioral counterparts. What strikes me as odd, however, is that some behavior analysts believe in segregation as the solution to this problem. Is the behavior of our psychologist colleagues not subject to the same laws as the behavior of people in general? And if so, where then is the presumed superiority of behavior analysis? If its practitioners are not even capable of influencing the behavior of those in their immediate professional environment to the point of peaceful coexistence and the equitable sharing of resources, why should anyone accept that behavior analysis is superior to more traditional approaches in changing human behavior? I cannot help but be reminded of the old joke that it takes only one psychologist to change a light bulb, but the bulb has to "want to change." How many behavior analysts does it take to change psychology? Why did B. F. Skinner himself fail to succeed in his quest of "making over the entire field [of psychology] to suit [him]self" (Skinner, 1979)? I think the answers to these questions are more complex than Fraley's contention that traditional psychologists would rather discount valid and reliable evidence from a science that contradicts psychology's fundamental mystical assumptions than abandon mysticism.

In the 1940s, behaviorism had led to a paradigm shift in psychology and throughout the following two decades behaviorists made a considerable impact on basic as well as applied subdisciplines in psychology. Behavior modification techniques developed in the animal laboratory were extended to human problems and for the first time in history offered an effective alternative to traditional psychoanalytic treatments, especially in circumscribed problem areas. However, during the 1970s a "cognitive revolution" occurred and much of psychology abandoned behaviorist conceptions in favor of the new paradigm that has remained dominant for the remainder of the 20th century,

Why did this paradigm shift come about? I believe that many psychologists studying human behavior recognized that traditional behavior-analytic techniques were successfully applied in settings where the behavior analyst could control the

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contingencies (e.g., with developmentally disabled and autistic individuals in residential settings) or teach adults to manipulate contingencies surrounding the behavior of dependents (e.g., parent training, teacher training). Unfortunately, many of these techniques have outlived their usefulness with typical adult psychotherapy clients, not because they were *in principle* not applicable to problems such as depression, low self-esteem, or personality disorders, but rather because the therapist could not control their implementation in the client's natural environment. For practical purposes, the behavior therapist, like any psychologist working with verbal adults, had to rely on techniques applicable in the psychotherapy session (e.g., skills training through in-session role plays, systematic desensitization, covert sensitization, thought stopping) or on verbal behavior, mainly instructing clients to employ behavioral techniques in their natural environment (e.g., exposure-based exercises). The former often showed limited ability to generalize and the latter, not surprisingly, were often not followed through consistently. With the advent of cognitive theories of psychological functioning, this situation changed.

These theories not only have drawn attention to the central role of cognition in human behavior but also have led to innovative and empirically validated interventions in many areas of adult psychopathology (Hayes & Wilson, 1995). Although behavior analysts would have no difficulty translating cognitive treatment strategies into behaviorese, the fact remains that these techniques were developed within the framework of cognitive theories and are applicable to a wide range of problems that behavior analysts had difficulty explaining because the problem behavior seemed to run counter to directly experienced contingencies. With the advancement of stimulus equivalence (Sidman & Tailby, 1982) and relational frame theory (Hayes & Hayes, 1992), new behavioral analyses of language and cognition are emerging that go beyond Skinner's (1957) analysis of verbal behavior. These new approaches have the potential of leading to novel clinical applications (e.g., Hayes, Strosahl, & Wilson, 1996; Hayes & Wilson, 1994); however, their effectiveness and range of applicability remains to be documented in well-controlled clinical trials. Stated differently, the superiority of novel behavioral interventions over widely used cognitive behavioral techniques has yet to be demonstrated.

Let me now return to the original issue, that is, whether behavior analysts should leave mainstream psychology departments and "apply for membership in the coalition of natural sciences." In response to this question, I would consider it worthwhile if those who call for an exodus analyzed the establishing operation for their escape and avoidance tendencies. It seems that Fraley's desire to emigrate is a function of aversive contingencies (such as not getting the respect he deserves; being isolated or ridiculed by his nonbehavioral colleagues). Unfortunately, he attributes the behavior analysts' plight to some flaw he views as intrinsic to the entire field of psychology, that is, that psychology is rooted in mysticism and thus incapable of appreciating a natural science approach to human behavior. Might it be possible that many behavior analysts who suffer indignities in their academic departments unwittingly evoke them through their own verbal behavior?

Richard Foxx, in his 1995 presidential address at ABA, gave an interesting interpretive analysis of why behaviorists are so disliked by other psychologists. First, Foxx stated, behavior analysts are viewed as arrogant because they appear to be

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overly confident in their own techniques and denigrate other perspectives. (I believe Fraley's paper is a case in point.) Second, they isolate and surround themselves with other behaviorists, which gives them a false sense of self-importance and acceptance and shields them from constructive criticism that might lead them to rethink and revise some of their methods. (Rather than striving to develop new methods that extend the current limited applicability of behavior-analytic strategies to adult clinical populations, Fraley's call for separate training programs constitutes precisely such a move). Finally, according to Foxx, they tend not to listen to feedback, which may be a natural outcome of their arrogance and isolationist tendencies. Feedback, however, is an essential mechanism for the continued development of any scientific enterprise.

I do not believe that behavior analysis has lost influence in mainstream psychology because colleagues from the mainstream prefer "scientized" methods and "mythical practices" over a true science of behavior, but rather because we have relied on arrogant rhetoric rather than data to "demonstrate" the superiority of our approach. The fact is that members of this "scientized community"-rather than "merely co-opting and redescribing" behavioral techniques in psychological language, as Fraley claims-have developed cognitive techniques with demonstrated effectiveness for a wide range of adult problems. Recasting these techniques as rule-governed behavior (e.g., Zettle & Hayes, 1982) is an interesting exercise in interpretation, but it hardly constitutes evidence for the proclaimed dominance of the behavioral approach. What is needed is a careful revision of our theories and the development of novel behavioral techniques that can be applied in the natural environment and serve to alleviate human suffering or solve important societal problems. If our philosophy of science is as superior to more traditional perspectives as we like to claim, our approach may have the potential to lead to unrivaled improvements of the human condition. But we are far from being there. We need to extend our analyses and place greater emphasis on a better understanding of verbal, especially rule-governed behavior and its relationship to human emotion and motivation. This might be accomplished better through cross-fertilization than isolation. Although I have no expectations that conceptual differences rooted in different philosophies of science can be easily bridged, I nevertheless consider it important for behaviorists to remain part of the larger scientific community of psychology. Without feedback and cross-fertilization, we might soon face death by extinction.

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