

## Chapter 2:

### The Evolution of the Concept of Behaviorology

*Before concrete actions were taken to launch a newly organized discipline, the concept of that discipline had to be shaped to maturity in the verbal repertoires of many people. This chapter, Chapter Two of this account of the emergence of behaviorology, examines the nature and origins of the behaviorology concept worldwide—and its increasing ill fit within organized psychology where the incipient stages of its organizational coalescence occurred.*

**B**ehaviorology, broadly construed, is a comprehensive discipline including philosophical, analytical, experimental, and technological components. “Behaviorology” in a more limited context denotes *the science featured within that discipline*—a natural, life science of functional relations yielding change in the behavior of individuals, with emphasis on the causal mechanism of selection. Behaviorology takes into account determinants from the environment, both socio-cultural and physical, as well as determinants from the biological history of the species. While many behaviorologists focus on human behavior, the science is relevant to the behavior of all organisms. From such components, comprehensive definitions of behaviorology arose after its formal organization in the late 1980s. (For example, see Ledoux, 1997a, for two versions of a comprehensive definition.) These defining components of behaviorology owe their cogency to the evolution of the *concept* of behaviorology. The first section of this chapter qualifies the nature of some of these defining components.

#### *The Concept of Behaviorology*

Behaviorologists have provided natural science answers to perplexing questions that have endured since antiquity: Why do people do what they do? What, exactly, *is* behavior? What is sentience? What are such things as *knowing*, *feeling*, and a *sense of importance*? What are the causes of behavior? How can knowing those causes help us to do more, act better, and behave effectively in all facets of life (for example, in child care, health care, education, daily living, work, leisure, art, entertainment, academic pursuits, and even science itself)? Can accurate descriptions of behavioral processes lead to practical behavioral technologies, especially ones conducive to cultural survival? To such questions behaviorology provides a kind of answer that is satisfying to scientists and professionals as well as engineers because it enables effective action.

Behaviorology emerged in the last quarter of the twentieth century from other disciplines through which its origins can be traced. This account compares and contrasts behaviorology with its disciplinary predecessors—explaining how it differs and why its people departed from those organized disciplines. It describes how the founders justified the existence of a separate discipline apart from others also focused, in their own ways, on behavior. E.A. Vargas (1988) wrote:

...our discipline is simply the verbal community that results from a scientific effort pursued in common. No [other] science covers the same subject mat-

ter, though many of the efforts of other science communities overlap with ours. (p. 2)

Different basic disciplines begin with different basic assumptions, ask different questions, and generate different methods and technologies. To a substantial degree behaviorologists do not use the same methods nor measure the same things as do practitioners based either in other disciplines or in applied fields informed by those disciplines—even those that are behavior-related. Nor do behaviorologists share the same philosophies of science and analytical paradigms prevailing in most of those disciplines.

### ***On the Nature of the Disunity***

The natural science *discipline* of behaviorology and the behaviorology *movement* are different concepts. The *emergence* of an independently organized verbal community of behaviorology was an early product of an independence movement. That movement, while acknowledging the shared, early history with psychology (Ledoux, 1997a), took this scientific discipline from under the organizational umbrella of psychology where behaviorological science and philosophy could not be made to fit. The behaviorology movement resolved an issue of intellectual property rights.

A study of any separatist movement logically begins with the question of why people would have borne the substantial costs of the undertaking. Two domains of variables suggest themselves: (a) the aversive reactions to one another among proximal groups as their differences become extreme, and (b) the contrasting natures and implications of their respective paradigms. The former domain is predictable and can be expected to emerge in whatever forms circumstances facilitate. Early behaviorologists, most of whom were formally affiliated in some way with psychology, represented a tiny minority there. Limited in their capacities for countercontrol as is typical with minorities, they faced pervasive difficulties from which they could escape only by taking their discipline elsewhere. But reiterations of specific wrongs done to individuals as revealed in examination of the former domain, although necessary to make the point and while perhaps therapeutic for those individuals, are less important than examining the latter domain, the *nature* of the disciplinary differences and the implications of those differences for the culture. That is where secessionists seek and find more fundamental and broadly relevant justifications. This work now turns to those issues.

### ***The Early History of Behaviorology***

The discipline *now* called behaviorology traces back over three quarters of a century before this writing, having evolved continuously at least over that period. Moments such as its formal organization and naming are simply steps in that longer history. Tracing the branch that became behaviorology takes us back in time along limbs, and down an historical trunk, through periods shared with other contemporary movements.

Those shared evolutionary paths are pieces of the history of each discipline involved. Many observers of that history mark Watson's conception of behaviorism, in the early 1900s, as a starting point for behaviorology (Watson, 1913; also see Drash & Freeman, 1973, for a detailed historical account). While Thorndike's (1911) emphasis on the consequences of behavior and Watson's emphasis on behavior as the proper subject matter for a science of behavior both preceded B.F. Skinner's research, other observers, including the authors, mark the beginning of behaviorology with Skinner's early experimental work and the publication of his book *The Behavior of Organisms* in 1938.

The focus on behavior by both Thorndike and Watson represented one evolutionary trend in traditional psychology—a minority movement that focused more on behavior itself as the phenomenon of primary concern though still approached with many traditional psychological assumptions and perspectives. But behavior remained a minority interest in traditional psychology. Later, when B.F. Skinner came on the psychology scene in the early 1930s, many saw his work as a continuation of that minority interest based in psychology. But Skinner, as we will review in more detail, had been trained primarily in biology-based natural science. When historical accidents pertaining to early training opportunities diverted Skinner into organized psychology, he came not only as a natural scientist (and stayed that way) but also with a repertoire of scientific principles derived from biology, some of which had been neglected in psychology and, Skinner's efforts notwithstanding, would remain largely unappreciated there.

The history of behaviorology tracks only temporarily through the historical turf of psychology—an historical thread inserted from the natural sciences, especially biology, into the psychology domain by way of Skinner (and the accident of his being in psychology)—and subsequently departing psychology by way of the newly organized and independent behaviorology discipline (for example, see E.A. Vargas, 1993).

Skinner's conceptual contrast with traditional psychology, first saliently posited in *The Behavior of Organisms*, relied strongly on the causal mode of selection in analyzing the production of behavior, and set forth the radical behaviorist perspective. (Selection, which addresses the postcedent effects of behavior and thus focuses analyses on its consequences, differs from the more purely methodological behaviorism of the early behaviorists who pursued studies of behavior in the stimulus-response [S-R] mechanical causal mode). Skinner extended the selectionist causal mode from biological phenomena to behavioral events. That selectionist causal mode, along with the radical behaviorist natural science philosophy, differentiated behaviorology, the science of behavior relations, from other approaches to behavior. (For an introduction to the philosophy of radical behaviorism, see Ledoux, 1997b.)

Skinner wrote *The Behavior of Organisms* long before his ultimate ambivalence about considering himself to be any kind of psychologist. Yet in it he noted that his extensive experimental work had contributed little to reducing psychology to physiology, a trend in some approaches to studying behavior to which he explicitly objected. He recognized very early that, while he had adapted biology-based selectionist concepts to behavior, the analysis of behavior that that adaptation afforded represented its own unique level of analysis—a discipline of its own apart from both biology and psychology. Skinner stated (1938) that:

...there are two independent subject matters (behavior and the nervous system) which must have their own techniques and methods and yield their own respective data....

I am asserting, then, not only that a science of behavior is independent of neurology, but that it must be established as a separate discipline... (p. 423)

As behaviorologists now construe the origins of the discipline, scientific problems to be solved and subject matter issues to be addressed came from pre-1930 psychology. But the philosophy of modern behaviorological science stemmed from the natural sciences. And biology, in particular, supplied the essential selectionist causal modalities that behaviorological scientists adopted for application to the new level of analysis required for the study of behavior.

The philosophical and scientific foundations of behaviorology were well established by the time Skinner published *Science and Human Behavior* (1953). That book detailed the discoveries of the science up to that time, delineated its philosophy of science, and extrapolated its applications to broad sectors of the culture. Of *Science and Human Behavior* Skinner wrote, “It is still, I think, the best introduction to my position” (letter to Ledoux, 14 April 1987). But through all of those years two things had been missing—a discrete disciplinary identity for the separate discipline to which Skinner referred and an appropriate name to connote emphatically its independent status.

### ***Skinner’s Philosophy of Science and the Behaviorology Movement***

The philosophical foundation that informs behaviorological science is the philosophy of science that Skinner called radical behaviorism. This philosophy is also at the core of the movement that brought forth the organized behaviorology discipline. Four of the components of radical behaviorism were especially important in the emergence of behaviorology (and receive due attention in relevant contexts in this work). These four are: (a) Radical behaviorists respect behavior as a *natural* phenomenon as part of respecting, along with all other natural scientists, the continuities of events in space and time which, in natural sciences, accumulate as a researchable natural history. (b) Radical behaviorists emphasize experimental control over behavioral variables and the application of that control in culturally beneficial ways. (c) Radical behaviorists recognize private events, such as thinking or emotions, as covert behaviors involved in the same laws discovered to involve overt behavior. (d) Radical behaviorists acknowledge that scientists are also behaving organisms whose behavior, scientific or not, is affected by the same variables that affect other behavior, and that those variables include scientists’ philosophy of science (see Hake, 1982; Ledoux, 1997b; also see Skinner, 1953 or 1974, or Chiesa, 1994, for more thorough treatment).

### ***Radical Behaviorism plus the Causal Mode of Selection***

Long before the behaviorology movement became organized (and even afterward) ambivalence about disciplinary identity plagued many of those who operated under Skinner’s radical behaviorism. That philosophy, together with its experimental analysis of behavior and the resultant practical technologies, connotes a comprehensive natural scientific discipline that some have thought more aligned with biology than with any previously established discipline (Ator, 1986; Logue, 1988; Michael, 1985; Skinner, 1953, 1974). Skinner was the first to describe many of the basic principles of behaviorological science. However, although earning his doctorate through the psychology department at Harvard University, he conducted much of his pre-graduation work under W.J. Crozier, head of the physiology branch of Harvard’s biology department (Skinner, 1979, p. 16). Crozier had been a student of biologist Jacques Loeb, and both Crozier and Loeb emphasized studying the behavior of an organism as a whole as well as the causal mechanism of natural selection. Skinner, while also emphasizing the behavior of an organism as a whole, transferred the concept of natural selection from genetics to behavior, a context in which *selection* refers to the lasting effects of the consequences of an individual’s behavior. Behaviorologists retain that departure and it serves as a *major* point of difference with psychologists. It also puts behaviorology on the life science continuum near biology, although the abstract selectionist concept that they share supports a relation only at the level of analogy—leaving behaviorology as an independent discipline.

Psychologists of the 1930s construed Skinner's adaptation of biological selection to behavior as just another small conceptual step on the broad scientific frontier of psychology. Its implications for a separate discipline passed unnoticed. Among those psychologists, selection represented an unfamiliar and subtle concept not readily incorporated into traditional repertoires. Nor were its implications obvious. Skinner began researching the implications of his conceptual breakthrough and developed a small school of operant behaviorists within psychology. But, increasingly, to understand and appreciate his movement required particular scientific training that only his students were getting. Over time his work impressed the psychology community, which toyed with behaviorism especially in the 1940s, 1950s, and into the 1960s. But the philosophical and scientific divergence was never eliminated or even significantly reduced.

The occasionally reported *dominance* of behaviorism in psychology through the 1940s and 1950s has been disputed by Lovie (1987) who argued that it was an exaggerated propagandistic myth. Furthermore, by the 1950s, many psychologists were beginning to sense the profoundness of the departure of the behavioral movement, especially the operant school, from the original assumptions underlying the psychological paradigm. The shift in psychology during the 1960s toward the information processing and computer simulation models of cognitive psychology was perhaps partly a counter reaction because it preserved a role for the internal agent while probing continued for the mental mechanisms by which that agent affected behavior. Lovie (1987) suggested that cognitive psychologists had, in fact, over-implied the earlier influence of behaviorism to create a straw man over which their so-called "cognitive revolution" could be seen to have triumphed.

As Skinner forged ahead, he pursued a consequence-driven mode of inquiry rather than the theory/method-biased investigations prevailing in psychology (Skinner, 1956). The expanding implications, of what he subsequently discovered to have been his novel adaptation of a *selectionist* paradigm, opened paths to solving fundamental problems traditionally definitive of psychology as a field. Across the widening scientific gulf created by his movement, Skinner's experimental analysis of behavior solved old problems in psychology in ways that ran counter to psychology's dominant internal *transformation* paradigm. Furthermore, in disposing of the mystery in problems that had always intrigued psychologists, the new analytical approach destroyed a kind of allure to which some have always appeared particularly responsive. But Skinner's solutions were resisted and ignored by most psychologists, partly because they were little trained in the science and philosophy upon which Skinner's perspective was based (see Ledoux, 1997c).

E.A. Vargas (1991a) compared the paradigms of disciplines concerned with behavior. He examined and named both the selection paradigm of behaviorology and the transformation paradigm of the other behavior-related disciplines, most notably psychology. E.A. Vargas (1991a) described the transformation paradigm as follows:

An event occurs, described in any number of ways. The meaning of that event inheres in the action of the organism. The organism perceives, interprets, assesses, integrates, and processes its perceptions and cognitions, and then stores the results of its own actions. It then (or later) engages in a performance with respect to that event—or, rather, the transformed nature of that event. In psychology's paradigm, some aspect of the world incites the organism to take action; but before that action occurs, the organism engages in a series of operations, typically called mental or cognitive, that determines the significance

of the event and thus determines the nature of the action. In the transformation paradigm, the organism itself, through structures and processes inherent in it, is the agency of its action. (p. 141)

Psychology as a whole has always left open the question of whether the internal cognitive operations and transformations required the guidance of an implicitly meta-physical self, a characteristic of the mentalistic tradition that has always flourished within psychology. Implicit reliance on some kind of an internal agent–manager can be seen in most general psychology–related textbooks (e.g., the currently popular Biehler & Snowman, 1990, p. 389). Skinner’s detailed exploration of this issue in Chapter One of *Beyond Freedom and Dignity* (Skinner, 1971) included Karl Popper’s illustrative question, posed in the mid 1960s: “What we want is to understand how such nonphysical things as *purposes, deliberations, plans, decisions, theories, tensions, and values* can play a part in bringing about physical changes in the physical world” (p. 8). Seeing no physical bases for such events, and with concepts of those events that leave their definitions vague, many psychologists have been content to accept them as non–physical.

In the selection paradigm of behaviorology, behavior is not forced out of the organism by transformed events, nor does an internal creative agent originate or manage such events. The body simply behaves in the natural and functional way determined both by its immediate structural condition and the environmental milieu in which it exists. The body is then altered by the consequences of its behavior so that the changed body that confronts future occasions behaves more in accordance with the implications of those past consequences. The consequences of the past behaviors are said to have *selected* the behaviors that now occur, and the selection paradigm takes its name from that interpretation. But in each instance of behavior, the body is assumed to behave *in the only way that it can behave* under the existing circumstances—an assumption that respects the deterministic natural science philosophy that informs behaviorology. No explanatory appeal is made to a redundant psychological self that would decide or choose the behavior to be exhibited by the body. E.A. Vargas (1991a) continues:

The essential characteristic of behavior is that it affects its immediate milieu.

The results of these effects determine the degree to which that milieu shapes adaptive and maladaptive behavior. Whether the organism eats or runs, for example, is determined by the consequences of its prior actions, or the consequences of prior actions of the biological and cultural communities to which it belongs. What follows behavior selects the future forms it will have and that will be maintained. Changes that occur do so through the “agency” of the particular properties of the environment as these interact with the specific characteristics of the organism operating upon that environment. (p. 142)

The paradigm difference between behaviorology and psychology provided a theoretical and philosophical basis for acknowledging the existence of different, separate and independent disciplines.

Organized psychology resisted the implications of both the selection mechanism and the deterministic natural science philosophy ever since Skinner’s scientific departure based upon them. Catania (1987), reviewing Bowler’s (1983) *The Eclipse of Darwinism*, suggested a parallel between (a) opposition to selection as a principle of behavior, and (b) opposition to selection as the mechanism of Darwinian evolution. Darwinism in biology survived its opponents. Similarly, having answered the criticisms leveled at earlier forms (Skinner, 1974), and having survived premature claims of its demise, radi-

cal behaviorism has survived and continues to expand and evolve, even amidst widespread resistance (see Wyatt, Hawkins, & Davis, 1986, for more detail; see Thyer, 1991, for data; and see Catania, 1991, p. 62, for additional references).

### ***The David Krantz Assessment***

The behavioral movement, arising historically in a confluence of intellectual traditions from biology and psychology, enjoyed the innocence of its youth from Watson, through Skinner's *The Behavior of Organisms* (1938), and into the 1940s. During that interval the psychology community debated behaviorism on merit, generally regarded it as interesting, and mostly remained oblivious to many of its implications. When, in the late 1940s, bands of enthusiastic advocates began to develop programmatic emphases around operant behaviorist studies, the contingencies driving the debates began shifting from the scientific to the political (e.g., see Keller, 1986).

The issues around which mentalistic psychologists focused their attacks on the behaviorists, and which seemed safe to debate in public, increasingly dealt not with the scientific evidence but with the nature and quality of organized scientific disciplines. A typical suggestion held that the behavioral movement, in spite of obvious scientific successes and benefits, ultimately portended adverse effects on scientific progress. Questions were raised: How should people who thought themselves in possession of powerful new principles and practices conduct themselves as citizens of the long established disciplinary community in which their careers had developed but in which their work did not fit? How should the remainder of the community behave toward them? Does an emerging philosophical and scientific faction within a field of study help or hinder the ultimate effectiveness of that field?

Within psychology a local eruption of the broader philosophical clash of social science and natural science was occurring, though few protagonists of those times appear to have viewed their squabbles in that way. Calling behaviorology a natural science raises questions about other types of science. Since the coverage of behaviorology includes, even emphasizes, human behavior, is it, for instance, a social science? The answer involves the distinction between natural science and social science. Social sciences not only have an interest in people but also easily reach contradictory conclusions after following the same scientific procedures. This is partly because social sciences allow untestable and metaphysical events to enter their explanatory accounts (also, see Skrtic, 1991). Natural sciences like behaviorology often have an interest in people also. More importantly, however, natural sciences, including behaviorology, more easily reach consistent conclusions after following the same scientific procedures. This is partly because natural sciences disallow the inclusion of untestable or metaphysical events in their explanatory accounts (see Ledoux, 1997a).

A *separate* discipline of behaviorology was not yet in view. The movement was evident within the psychology community only as certain facets of today's more comprehensively defined behaviorology. A manifestation called "operant psychology" had been identified as the salient aspect upon which attention focused. A dichotomy between operant and non-operant psychology was increasingly mentioned in the literature. Perhaps the most influential treatment of that split appeared in 1971 when David Krantz published "The Separate Worlds of Operant and Non-Operant Psychology," a special featured article in the *Journal of Applied Behavior Analysis*, a publication normally devoted to applied experimental work, but which made an exception for that article.

Krantz's treatment was scholarly and sophisticated. Though not thoroughly trained in operant psychology, Krantz had familiarized himself with critical elements of its science and philosophy so that he might address the rift as an informed historian and philosopher of science. His main data base for that article consisted of tape recorded interviews with 35 "key researchers" prominent in "operant" circles, at least a few of whom were critical of the operant movement. Krantz referred to "encapsulated schools of thought," or "movements," occurring *in* established sciences. Thus his perspective subtended a divergence between factions only *within* a discipline; the emergence of a separate discipline entered his considerations only as a somewhat remote implication.

Krantz first documented the extent of the mutual isolation by using data obtained from journals. The paucity of cross-referencing between operant and non-operant journals offered convincing evidence of the increasing differentiation of the operant school from others within psychology. Next addressing the "incommensurability" question, Krantz acknowledged that if operant and non-operant psychologists were really measuring and talking about different things, the mutual failure to cite would be justified. Two disciplines would exist, not one. But Krantz let stand a quoted phrase from one of his interviewees that described this as an "extreme" view. He did, however, elaborate on it descriptively so that readers might better understand the nature of what some operant psychologists argued *was* a fundamental disciplinary difference between operant and cognitive/mentalist views of behavior. Krantz referred to Sidman (1960) for the central point: As Krantz phrased Sidman's view,

...between-groups designs [*avored in traditional psychology*] assume that variability is intrinsic to the organism, thus the need for its statistical control; the within-groups design [*avored by the behavioral people*] assumes an imposed variability, thus the need for experimental, not statistical, control. (p. 63)

Underlying this difference is a major philosophical assumption that Krantz did not pursue: The operant people in general have always assumed that behavior is a *natural* phenomenon, and that behavior of any kind therefore occurs only in *total* functional relation with the controlling environmental milieu (including genes). However, among those in the non-operant majority, acceptance of intrinsic variability in many cases meant more than variability stemming merely from natural but unknown events occurring within the behaving organism. The *operant* analytical scheme easily accommodated those cases because, in the operant paradigm, part of the total controlling environment pervades the body. Therefore, independent variables of a valid and functional nature can exist on *either* side of the skin, although many of them—especially those inside the skin—resist accurate detection and analysis, and remain beyond the practical reach of intervention technologies. But for many non-operant researchers, the unexplained variation due to internal causes left operating room not only for thus-far unexplained natural causes but for *non-natural* internal causal events as well. For those scholars the internal variations could still be due to spiritual or mystical entities of non-natural origin (e.g., see Scoresby & Price, 1991). The way remained open for intervention by deities, spiritual selves, or similar non-natural entities, a belief in which is strongly shaped by various traditional cultural agencies. Unexplained internal variation also preserved the domain of mystery important to some scholars whose reinforcers are attached to an unending pursuit of the unattainable (one of academia's most venerated facades from behind which misdirected scholars have attacked the views of others as well as protected their own through the ages).

Krantz (1971) pursued the question of whether the operant/non-operant division represented a true disciplinary divergence or merely differences in procedures, terminology, and concepts inhering in the pursuit of investigations that could, and perhaps should, be conducted more in common. Could the differences between the factions be products of their isolation, or are those differences the fundamental reasons for the isolation in the first place? The *majority* of the operant researchers interviewed by Krantz made clear that they preferred the latter interpretation as well as the benefits of continued exclusiveness. They saw little reason to modify the direction and nature of their work to mesh more closely with traditional psychology.

Then, in a section on “conceptual imperialism” (a phrase which Krantz credited to Don Baer), Krantz (1971, pp. 66–67) described the confident attitude exhibited by many operant psychologists who insisted “the operant strategy can deal successfully not only with its own domain of problems but handle, as well, if not better, many diverse issues in psychology” (p. 66). Krantz described as “brusque” those operant researchers who, in providing what they deemed better approaches, did not cite or give much credit to the more traditional strategies that the putatively more effective science and technology was displacing. Krantz chided such “imperialists” for their failure to accommodate, and assimilate with, the traditionalists whom he assumed *had to be persuaded*. This was in spite of the fact that, in the history of most fields including psychology, as a matter of economy, disciplinary traditionalists have often been circumvented and left behind rather than confronted and changed. To the behavioral researchers who thought it pointless to cite or dwell upon ineffective approaches when in possession of a more powerful one, Krantz (1971) wrote:

To maintain militantly such a position is to make the philosophically naive and inter-personally insensitive assumption that one can change the science without the consent of the scientists. To act in contradiction to this truism, that scientific ideas are produced, evaluated, and changed by scientists, is to invite conflict, lack of success and very likely, rejection and other’s isolation of the imperialist’s position. (p. 66)

But science is *supposed* to change in response to demonstrations of greater effectiveness. So perhaps Krantz here alluded to additional non-scientific controls on the behavior of scientists. Most such controls remain unaffected by improvements in science. They also retard the change that those improvements should produce. Also, the implicit consensus of the behavioral subcommunity was that Krantz was wrong: Science, if not scientists, *could* be changed without the consent of the scientists. It would simply be done by *other* scientists. Insofar as science is merely behavior in the first place, to change it is only to start behaving differently; the operant behaviorists had already been doing that for some time. They did not see progress in their own scientific discipline as requiring the persuasion of those in another discipline—especially a discipline with a paradigm so antithetical to their own. And they thought that their neglect of that questionable task should not be charged against them as a breach of propriety.

To exemplify the kind of attacks that the offended traditionalists might mount, Krantz referred in detail to Wendt’s (1949) criticism of the pure operant program developed by Fred Keller (Keller & Schoenfeld, 1949) at Columbia University. Wendt had argued that although the Columbia program successfully attracted students from engineering and other natural sciences and was enthusiastically received by students, the program was academically unhealthy because its rejection of eclecticism promoted a

“behavioral cult,” a kind of isolationist movement that no good academic discipline can afford. Obviously, Wendt did not view those events as the loyalty and enthusiasm that students in most natural sciences give their disciplines. He ignored the effectiveness of the behavioral science taught in the program as the basis of its success. Instead Wendt attributed the popularity of the Columbia program to a supposedly *propagandistic* tactic of introducing implicitly invalid simplification into what was justifiable confusion. He implied that an allegedly more balanced, long term search for truth would eventually reveal the purported inadequacies in any such narrow approach that a small enthusiastic band might develop and impose on an institution. (Keller, 1986, reported that he “ignored Professor Wendt’s assault,” [p. 144] and that, upon reflection, so had Skinner. See Ledoux, 1997a, on the role of eclecticism in these debates.)

Krantz also quoted an anonymous critic of the behavioral isolationists who spoke of the operant psychologists’ “self-maintenance of true conceptual imperialism, at the expense of actually succeeding in practical imperialism or conceptual assimilation by others.” Again, neither Krantz, Wendt, nor the anonymous critic construed that a separate discipline was gaining prominence. Thus they did not consider applicable the principle that scholars of one discipline need not expend themselves, to the detriment of their contributions, by trying to win over, or to resolve their differences with, scholars of another. The unnamed critic was also wrong. The operant people *were* succeeding at the practical level. They tacted (Skinner, 1957, Ch. 5) carefully, having acquired that skill through its important consequences. They strived for accurate descriptions of relevant behavioral and environmental events. The resulting scientific principles reliably related environmental and behavioral events—yielding in the process the power of prediction. When subsequently they altered independent variables to produce prescribed behavioral outcomes (i.e., control), they were so effective that a new discipline, though not inferred by many, was nevertheless implied. The operant people had done nothing less than discover the conceptual key or approach to practical behavior technology, and they were beginning to appreciate the vast enabling power inhering in that discovery.

Krantz (1971) was apparently unprepared to describe effectively the concept and function of scientific (event-shaped) verbal communities. But he appeared alert to some implications of isolating both verbal and nonverbal event-shaped communities when he provided the following cognitive approximation:

Graduate education can be viewed as the main socializing force in communicating the norms of being a scientist and in training for particular orientations and strategies within a scientific field. . . .the learning of a scientific approach occurs not only through a verbalizable, didactic strategy but also through ostension via relevant “doing.” Such resultant knowledge is tacit, implicit, and not immediately accessible to awareness. The failure to teach other systematic options, or the consistent devaluation of other approaches, coupled with ostension experience in only one systematic option, can lead to a non-critical acquisition of a scientific approach and perpetuation of a modeled style of conceptual and practical imperialism in evaluating others’ research strategies. (p. 67)

Krantz did not appear equally bothered by a *cognitive* imperialism. He ended his discussion of “imperialism” by suggesting that behaviorists either can be “rude” and aggressive or can opt for a “softer sell” aimed both at converting cognitivists to the behavioral view and at assimilating the two schools, again as if that were necessary. He said nothing about whether time exists for the slow progress characteristic of the latter

approach. Nor did he appear to take seriously that criteria for separate disciplines were relevant in this case, though his article did mention these criteria.

In his discussion section Krantz said some typically erroneous things about the operant position: He spoke of establishing validity on the basis of correctness of prediction, but it is to the more rigorous level of *control*, not merely prediction, that radical behaviorists carry that test. He stated, from a modest analytical perspective, that inferences or statements about consciousness are not valuable to operant people in a science of psychology. But that idea was entertained only by the methodological behaviorists. Even before Krantz wrote his article, the radical behaviorists, operating at a more complex level of analysis, had already developed a useful science of private (singly observed) events including those behaviors of the kind known as “consciousness” or “awareness” (e.g., see Skinner, 1953, Ch. 17; 1957; also, see Ledoux, 1973).

Nevertheless, Krantz’s basic conclusions, though endowed with shortcomings for other reasons, were little damaged by such inaccuracies. He reiterated his warnings about allowing truth to be defined by fiat as one school politically displaces another. Ironically, much of what he seemed to fear along those lines has occurred because of a suppression of the behavioral minority, that is, because of a cognitive imperialism, not a behavioral one. Krantz did not, however, explicitly reject the argument that “operant psychology” represented “revolutionary” differences yielding “incommensurability” between the conflicting systems. At one point he referred to the settlement of that fundamental issue as “unclear.” Many readers, however, reacting to the overall tone and style of Krantz’s article, inferred that the rift might *not* be justified on the basis of incommensurability. As Coleman and Mehlman (1992) noted:

...Krantz’s investigation—and the widely held interpretation of its findings, to the effect EAB [Experimental Analysis of Behavior] is in an unhealthy state—came out of an historically situated ‘ideology’ regarding scientific progress in general and the progress of psychology in particular. (p. 48)

Coleman and Mehlman (1992, p. 48) attributed that ideology, and its influence on Krantz, mainly to the reports published in the 1960s by the American Psychological Association’s *Project on Scientific Information Exchange in Psychology*.

The Krantz article was well reasoned and rather sophisticated in the development of its arguments (though not without its own propagandistic slant). As an exceptionally featured piece in a highly respected journal, it undoubtedly had a moderating effect on the operant separatist movement within psychology. Krantz’s article exerted pressure on operant psychologists to move back toward the path of reconciliation and gentle persuasion, a path which, in the view of behaviorologists, has since proven costly to the integrity of the science as well as strategically ineffective. Unfortunately, at a practical level, for many behaviorists that togetherness amounted to little more than compromising their philosophical probity. They found themselves continually acquiescing and maintaining pretenses that eclecticism connotes respect for some academic procedural ideal instead of merely lack of resolution on the question of how best, scientifically, to proceed (see Ledoux, 1997a).

Krantz’s (1971) article appeared in the middle of the eight years of discussions that ultimately led to the founding of the Midwestern Association of Behavior Analysis (MABA, later known as the Association for Behavior Analysis [ABA]). The extended time required to establish an independent MABA, and the very gradual recognition of the significance of the revolution represented by that movement, may both be due in part to

Krantz's article. Even as this is written, in 1991, 20 years after Krantz's article, some who might otherwise join readily with the behaviorology movement, linger hesitantly and plead for yet another inquiry into Krantzian wisdom on the merits of togetherness as opposed to separation.

Like many who reviewed the rift, Krantz implicitly framed his research question to test the hypothesis that the "operant movement" represented just one of any number of "sciences" or "perspectives" in an eclectic social science mix denoted by the psychology label. But increasingly, the followers of Skinner's movement were reacting to the rift as if it were fundamentally of a more simple *dualistic* nature: On one side was natural science, represented in this instance by themselves, and featuring a predominantly selectionist mode of causality. On the other side was social science, represented by traditional psychology, and featuring a theory-based paradigm, laced with metaphysically inspired assumptions and dwelling on presumed transformations of experience into behavior by way of cognitive mental mediations.

Krantz (1971) both described and contributed to the attitudes prevailing during a critical period of discontent that preceded the formal emergence of behaviorology. His assessments have been reviewed here because, as an influential and respected analyst of science history, Krantz seemed to lend scientific validation to then-prevailing biases and arguments favoring abandonment of the "behavioral" revolution.

### ***Recent History: Disciplinary Identity, Name, and Support***

In the 1970s radical behaviorists in psychology established a professional organization (discussed later in detail) outside of the American Psychological Association (APA). It became the Association for Behavior Analysis (ABA). It evolved, however, more as a scientific and professional interest group than as the anchor point for an independent discipline. Most ABA members who began as psychologists continued to give themselves that disciplinary identification. By also calling themselves "behavior analysts" they implicitly classified behavior analysts as psychologists. Since the 1980s, with the emergence of an independent behaviorology discipline, some behavior analysts, anxious to preserve organizational ties with a well-entrenched and endowed organized psychology, have grown more explicit, as will be seen, in proclaiming behavior analysis to be a kind of psychology.

ABA has continued to focus, not on establishing an independently organized discipline, but on how most effectively to endow psychology with a more worthwhile science. Along these lines the ABA leadership has remained consistently critical of the science pursued in mainline psychology. For instance, Philip Himeline, who would later serve as president of ABA, noted that psychology is pursued with verbal repertoires based on often inconsistent terminology adopted with few restrictions from everyday language (Himeline, 1984). In his 1990 ABA presidential address, and again in a later article (Himeline, 1991), he thematically reiterated that view and counseled renewed efforts to change psychology. But by then the behaviorologists had abandoned that strategy and organized an independent discipline.

***The separation debate.*** The years between 1984 and 1987 were a time of extensive debate about disciplinary status as numerous authors discussed, pro and con, the disciplinary separation from psychology of what is now called behaviorology (see Ator, 1986; Barry, 1986; Comunidad Los Horcones, 1986; Deitz, 1986; Epstein, 1984, 1985,

1987a, 1987b; Fraley, 1987; Fraley & Vargas, 1986; Gaydos, 1986; Lee, 1987a; Leigland, 1985; Malagodi & Branch, 1985; Staats, 1986; and E.A. Vargas, 1987).

Epstein (1984) began this debate with a proposal for a new discipline under the name “praxics.” Leigland (1985) and Malagodi and Branch (1985) disagreed both with some of Epstein’s arguments (though not with the possibility of separation) and with his proposed name; Epstein (1985) rejoined. Fraley and Vargas (1986) summarized the issues, disagreed with Epstein’s disregard for the philosophy of the science, and spoke for separation under the name “behaviorology.” Barry (1986) subsequently objected to both “praxics” and “behaviorology,” preferring “anthroponomy,” a name proposed much earlier by Hunter (1925). Gaydos (1986) objected only to the name “praxics.” Comunidad Los Horcones (1986) supported “behaviorology.”

Staats (1986) objected to separation from psychology, arguing instead for unification under a philosophy called *paradigmatic behaviorism* which “characterizes psychology as a disunified science” (p. 232) and which tolerates that disunity. Many contemporary psychologists welcomed such attempts to make a virtue of the paradigmatic differences within psychology (e.g., Hishinuma, 1989). The troublesome problem of how incompatible philosophies and sciences can all represent a single discipline was made to vanish by redefining “discipline.”

Catania (1973) had earlier taken a different approach. He insisted “psychology is not in the midst of a paradigm clash” because points of contact cannot be found. By that he meant that the “different schools of psychology, ...have been concerned with different problems” (p. 442). But if they *did* ask different questions, in what way were they different? Was it merely interest in different aspects of the complex problems being addressed by psychologists (subject matter differences), or would the different schools not find each other’s questions valid because those questions arose out of analytical paradigms too different to garner respect across those schools? Or, to cast the issue in another light, if those schools were to trade problems for awhile, would the paradigmatic treatments of those problems remain unchanged? That is, did cognitive and behavioral psychologies represent only *problem*–imposed differences in work informed by a *common* paradigm? Or, in contrast, are different kinds of problems, arising out of different subject matters, addressed with fundamentally *different* paradigmatic approaches? If so, different disciplines are implied. In the prevailing view among behaviorologists, when common problems had been addressed by the different schools in psychology, real Kuhnian paradigm clashes (Kuhn, 1970) did appear. Those differences manifested in very different technological implications. A typical example was described in the article “Cognitive Analysis of Language and Verbal Behavior: Two Separate Fields” (J. Vargas, 1990).

Epstein returned to the debate with an appeal targeted mainly to students (1987a). He argued again for the emergence of “*an independent, multi-disciplinary, biologically-based science of behavior.*” He criticized the fragmentation of behavior-related studies across a broad spectrum of separate disciplines and called for their unification:

A true science of behavior must be *multi-disciplinary*, ...because behavior is a complex subject matter that requires the *joint efforts* of individuals in many specialities, both to advance our understanding and to devise effective treatments. Behavior is affected profoundly by nutrition, physiology, sleep deprivation, ...sexual deprivation and trauma, chemical interventions, social phenomena, surgical interventions, physical trauma, anatomical variables, or-

ganic disease, hormonal cycles, air temperature, humidity, illumination, airborne chemicals, radiation, electrical stimulation, genes—and, of course, learning history. It is not folly to think that individuals with different specialties can be brought together to build a new science; it is folly to think that a handful of scientists who now study behavior in almost complete isolation from each other in a dozen different disciplines can advance our understanding significantly. (p. 128).

Epstein reminded us that no comprehensive behavioral science can afford to treat the behaving body as a constant while focusing exclusively on contingencies of reinforcement. And behaviorologists, while emphasizing the importance of contingency relations, do respect a far broader range of concerns and principles than those implicit in an overly simplistic understanding of the phrase “contingencies of reinforcement,” as did Skinner.

But Epstein continued to insist that the work of a collection of scientists with diverse behavior-related specialties can be coordinated under control of natural contingencies without their sharing a common supplementary verbal repertoire of the kind known as a philosophy of science. He envisioned praxics as a “pure science, driving real and promised applications, and uncluttered by an irrelevant and unattractive credo...” (p. 128). (Also see Epstein, 1987b.) Behaviorologists, among others, disagreed with Epstein. One’s philosophy of science *does* share in the control of one’s scientific responses to data—a general principle recognized far beyond the bounds of behaviorology (e.g., see Hake, 1982). And, importantly, people doing scientific work inevitably bring *some* philosophy to that work. That philosophy shares in controlling the person’s behavior regardless of the intensity of the *natural* consequences of the work upon which Epstein seemed to be counting for a rather exclusive control.

For other authors, the issue of an independent and comprehensive scientific discipline of behavior arises implicitly and without expressed concern about its name or organizational status. For instance, Lee (1987b) stated that:

...behavior analysis is outside the mainstream of psychology, and its foundation in assumptions that depart markedly from those of the mainstream let most psychologists ignore it with impunity. (p. 145)

But on another occasion, and apparently concerned about reversals in progress toward a distinct identity, Lee (1987a) asked in a published book review if we should “expect a commitment to radical behaviorism in a book on behavior analysis” (p. 95), observing that:

...the less-than-full commitment of many of us is apparent in the conceptual poverty of applied behavior analysis, in the infiltration of cognitive terminology, in the re-appearance of traditional group designs, and in the reduced interest in behavioral control techniques, among other things. (p. 95)

Fraley (1987) described the difficulties with unification, the reasons for separation, and the cultural mission of the independent discipline of behaviorology, especially as it affects many other fields that deal with human behavior. Concurrently, E.A. Vargas (1987) linked the survival of effective behavioral science to disciplinary independence. He argued that departure from psychology can be supported not only on the basis of differing subject matters, but also on the basis of the sociological considerations relevant in the development and governing of professional disciplines. Vargas noted that

psychologists whose actions suppress behaviorology within organized psychology act rationally in defense of their own self-interests.

Then, on 17 October 1987 in a general session for all in attendance, the annual meeting of the Southeastern Association for Behavior Analysis concluded with a formal debate chaired by Fred Keller. Lawrence Fraley and Ernest Vargas (pro) debated Mark Branch and Peter Harzem (con) on the establishment of behaviorology as a separate discipline. (The debated issues are addressed throughout this paper.)

Debate, however, was rapidly being rendered moot by independent actions establishing behaviorology as a comprehensive natural science of the behavior of organisms. In the name of behaviorology, its adherents were pursuing applications to, and interpretations of, a broad range of human affairs (Barry, 1986; Fraley & Vargas, 1986). For example, in May 1988 Guy Steven Bruce produced the first master's thesis to reflect its behaviorological content in its title (NTSU, 1988), "Problem Solving: A Behaviorological Analysis with Implications for Instruction." His thesis committee at North Texas State University, chaired by Sigrid Glenn, included Ernest Vargas, of West Virginia University, under whom Bruce subsequently earned his doctorate.

***Debate continues anyway.*** Some behaviorists who were opting to remain within organized psychology continued publicly to draw distinctions between mainline psychology and the behavioral science and philosophy that they believed should supplant it. These distinctions paralleled the points raised by behaviorologists. The presidential address of Steven Hayes (1988a) to the behaviorally oriented Division 25 of the APA, is one example. The distinctions to which Hayes and others pointed were often the same ones to which behaviorologists were pointing. Philosophically and scientifically, the discipline advocated by Hayes and others is fundamentally different from mainline psychology. The residual issue pertained only to how best to act upon this difference.

Burns (1988) reiterated Staats's attempt to link together the verbal subcommunities within psychology by ascribing to each its own level of analysis (i.e., its own approach to theory construction) and arranging them hierarchically from "basic" to "less basic." The whole assemblage is then said to be the unified "discipline" of psychology. Jerome Ulman (1990a) rejected that concept, referring to it as "hierarchically schematicized eclecticism." Burns also accused radical behaviorists of "rejectionism" which, he argued, "does not foster harmony" within psychology. On the latter point, Burns was correct. Behaviorologists saw no long-range cultural benefit from elaborate reinterpretations intended to make psychology seem like a coherent discipline. Nor were behaviorologists, as natural scientists, sanguine about the political, social, and economic negotiations that inevitably replace scientific persuasion in academic alliances that, scientifically, are too disparate to bind in that way. Harmony in psychology was simply not the goal of the behaviorologists.

Psychology, in the broadest concept, had not evolved as a single discipline. Even authors of articles in publications sponsored by the APA had occasionally reiterated this point. For example, Nessel (1982), in an article on the state of psychology, had referred to "the *various disciplines* of psychology" (emphasis added). The disunity of psychology was also evident in the other competing and often mutually contradictory perspectives that long comprised the area—others besides the behaviorological/psychological rift. Evidence for this could be seen in Nessel's article (1982). The majority of psychologists had long agreed on a broadly encompassing paradigm respecting internal transformations as the essence of behavior. Yet, as Nessel's article revealed, in spite of that

paradigmatic commonality, eleven distinguished psychologists showed little agreement in their perceptions even about the most important developments in psychology over the preceding fifteen years.

In her book *Beyond Behaviorism* (1988), Vicki Lee presented a comprehensive analysis of both the recent and historical differences characterizing the disciplinary fragmentation in psychology. Lee's book offers a compelling argument for psychologists to "make over" their field by bringing themselves less under control of economic and political contingencies and more under control of scientific ones. But over the decades attempts to do that have fallen short. Organized psychology does not seem amenable to overhaul merely by demonstrations of better science. The frustration of those who have proffered a more effective science only to have it ignored as if irrelevant can be seen in Lee's (Lee, 1989) quote of Murray Sidman (1986b, p. 44), that:

...the reluctance of some psychologists to use the body of knowledge that has accumulated in behavior analysis suggests 'a kind of scientific malpractice.' (p. 86)

In September 1990 Lee herself would send an open letter to her behavioral colleagues worldwide describing and lamenting the scientific isolation she experiences as a behaviorist within her Australian psychology verbal community:

I have given up talking about radical behaviorism and behavior analysis unless "invited" to do so in a conversation with an interested listener. It is simply hopeless to do so otherwise. Even if the misconceptions can be cut through in the presence of an audience, ...there is the impossibility of getting over the hurdle that requires the listener to see the insanity of much that passes as psychological research and theory.

**The name.** The *science and philosophy* of behaviorology can be traced at least to the early decades of the twentieth century along certain historical paths, and even further back to early recognition in biology of the selection mechanism. But the *name* "behaviorology" followed different historical routes from multiple origins. A December 1986 computer database search for references to behaviorology (or behaviourology, the British spelling) revealed only two references. But that search, of Dialindex's Biosci and Socsci databases, proved not to be exhaustive for it did not reveal some known cross-references to, or uses of, the term.

The earliest discovered author using the name behaviorology *consistent with the present usage* was Makram Samaan (1973). Samaan's description of behaviorology focused on the behavior modification technologies of the 1970s and did not dwell on radical behaviorist philosophy. Others had used the term before him, though differently. In a one paragraph article, Vitulli (1969) suggested changing the name of psychology to behaviorology because people rely more on behavior than on the psyche. Even *Time* magazine, he noted, had begun a "Behavior" section in 1969. But Vitulli's usage implied no change in mainstream psychology other than its name, and nothing came of his suggestion.

On the other hand, Samaan's (1973) opening statement, from his university-published book (English was not his native language) advocated a more sweeping change:

With the discovery of the functional relations of human behavior and its contingencies, and the successful technology of behavior for effecting behavior change, a call for the independence of behaviorology from psychology which was conceived as a study of the "self," "mind," and "soul," is in order....

It is time now for the scientific analysis of behavior to call for its own scientific discipline. It is contradictory to its function, objective, methods and content to stay within the realm of psychology. The discipline of "Psychology" is originally derived from the Greek term "psyche," which means "soul," "mind," or "self." ...psychology was conceived as the science of the soul, mind, or self. ...behavior was assumed a function of the "mind," the "psyche," or the "self." (p. 5)

Samaan noted that in 75 years of searching, psychologists had not found the autonomous behavior-controlling mind. But scientists of the kind who were to become behaviorologists *had* discovered the basic controlling relations between environment and organism that account for behavior. Samaan continues:

It is a contradiction to carry a name of a so-called science of inner process, souls, and minds for a discipline of operationally defined behavior. ...independence of Behaviorology is a necessary and significant step to abolish the confusion of incongruence between the name of the discipline and its methodology and subject matter. (pp. 5-6)

Samaan is historically important for his early explicit call for an independent discipline of behaviorology. However, he contrasted behaviorology with only one concept of psychology, which might be classified as non-natural science wherein mind is viewed as the locus of a behavior-controlling ethereal essence.

In the years since Samaan, others, acting independently, coined or adopted the name behaviorology to tact the same discipline, although with radical behaviorist philosophy more clearly included. In 1974, cultural engineers within the Los Horcones Community in Mexico coined the name behaviorology. They defined it as "the natural science that studies the behavior of organisms" (Comunidad Los Horcones, 1986). To them behaviorology encompassed "basic research, applied research, and a philosophy" (p. 227). While teaching in Australia, Stephen Ledoux (1977) coined and similarly used the term to describe the position of radical behaviorists who would separate from mainstream psychology. Like others, he was unaware of Samaan. After Joseph Morrow, at California State University, Sacramento, mentioned Samaan's work (letter to Ledoux, August 1977), Samaan was discovered to have been writing his 1973 book in a different department at that same university where Ledoux was then studying. Although Ledoux had commented in his paper that his usage would likely be unrelated to any previous appearances of the term, Samaan and others were later discovered to have used the term in much the same way.

Communications with colleague and library searches revealed two examples of the use of "behaviorology" by mainline psychologists. Dr. A. Ph. Paschalis, a behaviorally oriented counseling psychologist in Greece, reported to Jerome Ulman (letter to Ulman, 6 July 1989) that cognitivists and social learning theorists in Greece had used that name "a number of years back" to distance themselves from those reflecting a radical behavioristic approach to which these theorists were opposed. However, such counter-use of the name did not spread among members of the world-wide cognitive verbal community. Much earlier, David Sherbowsky, in his apparently self-published book *An Outline of Behaviorology—The Psychology* (Sherbowsky, 1935) used "behaviorology" as the name for what he viewed as his correction of Watson's behaviorism. Preferring to call Watson's behaviorism "Watsonism," he stated "Behaviorology is not Behaviorism (Watsonism)" (p. 3). This usage also did not spread.

After E.A. Vargas's coining of the term anew, Fraley and Vargas (1986) gave their reasons why behaviorology should be the name of a discipline (including its experimental, applied, and philosophical aspects) that would be independent of other behavior-related disciplines. Comunidad Los Horcones (1986), based on their own longer history with the name, endorsed the attendant philosophy and offered this support:

First, [the name behaviorology] is etymologically appropriate. The word "behaviorology" is a combination of the English word "behavior" and the Greek word "logos."

Second, "behaviorology" does not eliminate the experimental analysis of behavior, applied behavior analysis, or behaviorism. Instead, this new term includes them as sub-fields of the same science. It is an integrative name.

Third, "behaviorology" ...does not imply that behavior analysis pertains only to experimental analysis. Once, perhaps, the inclusion of "experimental" in the name of the science was necessary in order to emphasize its empirical basis, but...no longer....

Fourth, by using the term "behaviorology," the length of the name for the science is shortened considerably [from "The Experimental Analysis of Behavior"], which is an obvious advantage when talking and writing. (p. 227)

The repeated coining of the term *behaviorology* to name this discipline presumably occurred because of similarities in the contingencies under which its professionals operated. But some behaviorists remaining within organized psychology were not similarly affected. Lamal (1988) objected that behaviorology is "unfelicitous." Hayes (1988a) also criticized the term, saying "... 'Behavior' is too readily viewed as the act separated from the context," adding "this is one reason that the call by some for a field of 'behaviorology' is misguided" (p. 14). But behaviorologists, like other scientists, take for granted that they must study their subject matter in its functional context. On appropriate occasions behaviorologists explain that theirs is the science of behavior *relations*, not behavior in isolation. This distinguishes them from those said by Skinner (1972) to endow behavior with a curious ontological status, to explain it with "appeals to events taking place somewhere else, at some other level of observation, described in different terms, and measured, if at all, in different dimensions" (p. 69).

Different names will serve a movement in different ways, each affording some gain from one perspective while costing with respect to others. Before deciding on a name, it is important to determine precisely which problem the name is to help solve. Followers of a discipline quickly adapt to whatever name is used. But how the general population responds to the name is more important for a discipline that would contribute scientific underpinnings to inform all behavior-related fields. The founders of the behaviorology movement wanted a term that worked in the culture at large. The multiple coinages of "behaviorology" suggested that they had found it.

Such an intuitively obvious name was deemed more important than a less useful though etymologically pure term based on ancient languages. Skinner, during a 1987 ABA symposium, expressed displeasure with the name "behaviorology" for its polyglot etymology featuring English, French, and Greek derivatives. But Skinner's objection to the ancient history of the term seemed irrelevant to many of those who appreciated its appropriate functional control over the behavior of most contemporary listeners, lay as well as professional. At that same symposium, Robert Epstein was also critical of the name "behaviorology." He had earlier, and without documentation, speculated in print

that the term had probably been considered and rejected by others half a century ago (Epstein, 1987a, p. 129). But he had not said why such a history, even if documented, should reduce interest in the current usage.

As the behaviorologists pondered the question of whether members of the general public would respond appropriately to the stimulus “behaviorology,” historian Daniel Bjork (letter to Fraley, 14 February 1989) offered a historian’s perspective: Bjork speculated that many lay people associate “psychology” with “intellectual”—which might imply arrogance or elitism plus an impractical science. Because “behaviorology” has a more practical ring, it sounds more useful, and certainly less mysterious. People might be more comfortable associating with its practitioners. The founders, guessing that that name would tact the activities of behaviorologists without inducing confusion, began informal testing of the term with a variety of people. Confirming reports came trickling back. Most were of this kind: When Ernest Vargas (personal communication, 28 December 1987) responded “I’m a behaviorologist” to his dental hygienist’s query about his occupation, she paused very briefly and then said, “Oh, that’s different from psychology; you study behavior.” Similar responses came from his barber and from his cat’s veterinarian. Through many such simple probes, the founders gained confidence in their choice of “behaviorology”—an informal mode of confirmation that has continued with virtually unchanged results.

**European support.** The behaviorology movement was not exclusively of American origin. Support arose in different parts of the world. European activity was easy to document, and early reports about it bolstered the importance of organizing the movement internationally.

After a conference in West Germany in 1986, Julie S. Vargas reported having encountered a Belgian, Werner Matthijs, who had told her that the Dutch equivalent of “behaviorology” was sometimes used in his country. This suggested the possibility of a European movement toward separate disciplines. Lawrence Fraley accepted the task of directing inquiries to some Europeans who might be involved.

Claus Thiermann of the German Behavior Academy in Stuttgart, West Germany, responded (25 September 1987) that he and his colleagues had started their work in 1976 “on the straight behaviorology line you are favoring.” He described the Academy as a private institution existing to “distribute the application of Behavior Analysis and nothing else.” He reported that his group had given training to about 500 people in the past ten years; he said about 50 had become recognizable as “behavior analysts.”

Thiermann provided a brief history of the movement in Germany: A German translation of Skinner’s *Science and Human Behavior* (1953) had appeared in the late 1960s. But the translation had been poor and its philosophy seemed unconventional to continental Europeans. So few read it and even fewer understood it. In 1971 Holland and Skinner’s *The Analysis of Behavior* (1961) was published in German. It was easier to read, and a few thousand people apparently did so. According to Thiermann, that book spawned a short-lived German behavior therapy movement in the early 1970s. But he called that movement “superficial”—practiced mainly by persons who did not comprehend the underlying science and philosophy. Thiermann concluded his historical report as follows:

Behavior therapy soon gained a very bad reputation. Some who did it got punished, and, though reinforced by therapeutic success, had their behavior suppressed and emitted avoidance behavior by developing cognitive behavior

therapy, following Mahoney. By the end of the 1970s pure and clean behavior modification/behavior therapy was out. A combination of cognitive methods and behavior therapy is fashionable today. Hard core evaluation methods, of course, have no chance under those circumstances. (Thiermann to Fraley, 25 September 1987)

As among Americans, some European behavioral practitioners doubted the efficacy of a separatist movement, preferring instead continued operations within organized psychology. As the European psychologist Marc Richelle wrote (letter to Fraley, 22 October 1987), they feared that “leaving psychology to opposite trends, essentially cognitivism, ... might give them [the cognitivists] an easy victory on an abandoned territory.” Richelle reported that he thought he detected a reaction against the most extreme forms of cognitivism, and that he looked forward to a better future for behaviorists in European psychology “within a few years.”

Werner Matthijs, the Dutch speaking behaviorist in Belgium, answered a further inquiry with two more letters to Fraley (8 & 29 July 1987) In these he described his own commitment to behaviorology and his efforts to promote a separate discipline.

Matthijs reported that almost all European psychologists were mentalists who regard behavior only as a symptom of important events in the mind. He also mentioned some interest in cognitive behavior modification among European psychologists. But he regarded this trend as a further indulgence in mentalism.

Matthijs described his own antithetical reaction to all of this as follows:

The more psychological theories about the “psyche” that I had to absorb during my graduate studies, the more carefully I began to read Skinner’s work, and the more obstinate and uncompromising I became in my verbal resistance to the full-fledged mentalism to which I was daily exposed. This, in turn, only served to increase the opposition I experienced, even up to the point that one of my professors, whom I had criticized because of his constant misrepresentations of Skinner’s views, no longer allowed me to take his courses on psycholinguistics.

The contrast between his own behavior-focused science and the mainstream psychology featured in his formal training led Matthijs to seek a better disciplinary name:

Finding such a term was especially reinforcing.... The Dutch translation of “behavior” is “gedrag”; *gedragsologie* is a term which in the most direct way refers to the study of “gedrag.” “Gedrag-sologie” is thus the Dutch equivalent of behaviorology. For people whose verbal behavior has been shaped by a Dutch speaking verbal community, *gedragsologie* is easier to pronounce than *psychologie* (the Dutch term for psychology). For those of us whose verbal behavior has...been shaped by the behaviorological community, the term *gedragsologie* has not the aversive connotations associated with the term psychology. In fact, reading, hearing, and speaking about *gedragsologie* is...automatically reinforcing.

Matthijs reported first using the term *gedragsologie* in print when in 1980 he surveyed reactions to that name by colleagues. He said that he found increasing comfort with the term as people used and encountered it more frequently.

Matthijs also added this anecdote to the history of the “behaviorology” name:

By the time R. Epstein published his praxics article [Epstein, 1984], I and a few colleagues of mine had already grown accustomed to the term

gedragsologie. In our own teaching and seminars we found it quite natural to speak about “gedragsologie.” I wrote a letter to Epstein asking him whether he had...considered using “behaviorology” as a new term for our science. I also explained to him that I would greatly appreciate knowing whether he had other objections to the term behaviorology besides, perhaps, stylistic ones. In a very short letter, he said that the term behaviorology is “a bit silly” and that in English, the term wouldn’t have “any chance to be accepted.” I believed him and gave up the idea of writing a short article (for the “On Terms” section of *The Behavior Analyst*) in which I would have proposed behaviorology....

Matthijs also disagreed with Epstein’s argument that the study of behavior should be separated from radical behaviorism, writing that:

...without radical behaviorism, you can only pay lip service to the study of behavior. That seems to be an extreme and intolerant view, but one function of radical behaviorism is precisely to be intolerant...of mentalism.... The problem is not that we are radical behaviorists, but that we are not radical behavioristic enough....

Echoing a sentiment also expressed by E.A. Vargas, Matthijs observed that:

...psychologists are right when they do not allow us in their mentalistic departments (just as behaviorologists would be right not to allow psychologists in their future behavioristic departments). I...was...pleased with Epstein proposing to separate from psychology, but I don’t see how you can successfully separate from psychology without also radically separating from mentalism, which has always dominated each kind of psychology.

**Contact with China.** The behaviorology movement also discovered supporting information on the other side of the world from Europe. During the 1990–1991 academic year, Stephen Ledoux taught behaviorology courses in the People’s Republic of China as an exchange professor at the Xi’an Foreign Languages University in Xi’an, Shaanxi. While there he held discussions about behaviorology with locally based senior members of the behavior science disciplines (see Ledoux, 1997d, for details).

Ledoux found that the Chinese define psychology to encompass more than what the term implies in English. In Chinese, “psychology” connotes a broad discipline drawing upon three sources. The first features traditional Chinese perspectives. The second stems from the discipline as pursued in the Soviet Union (especially the work originating with Pavlov on respondent behavior). The third is a mix of Western perspectives. The Chinese have included three parts in the Western component: (a) psychoanalysis (i.e., Freud), (b) traditional cognitive/mentalistic psychology (e.g., Maslow and Piaget), and (c) a behavioral approach based largely on Skinner’s science.

The Chinese, strongly oriented toward practical results, reportedly liked the natural science approach and experimental methods in the work of Pavlov and Skinner. And unlike Western philosophical thought, the Chinese seemed to have avoided much of the Western extremes in separating phenomena into mental and physical realms (soul/body, spiritual/material, mind/reality). The Chinese language, while it has a rich variety of terms for most of the varied Western usages of the term *mind*, actually lacks a direct translation of “mind” as Western psychologists use that term, with “mind” implying a metaphysical locus from which mysterious variables exert controls over behav-

ior. Instead, for that usage, Chinese professionals generally use a word that better re-translates back into English as “brain.”

Interestingly, the term Chinese professionals use for their “behavioral” component is *Xingwei Xue*. This term translates accurately, if generally, as behaviorology, behavior analysis, behavior science, or science of behavior. But they have no current specific term with *organizational* connotations to which they respond as we respond to the term behaviorology in the sense of a separate and independently organized discipline.

However, the Chinese were out of date, having lost contact with Western developments since the mid 1950s. They had been operating with a behavioral component that was 30 years old. Certain dissatisfactions with the behavioral approach stemmed from the antiquity and superficiality of the version that they knew. It often seemed inadequate to account for the complexities of human behavior. Ledoux found that the Chinese had spent the decade of the 1980s trying to update their 30 year gap in knowledge of Western developments. But accidentally their update had pertained only to the psychoanalytic and traditional cognitive approaches. They had not realized that the mainstream psychology sources for their update lacked information on the continuing disciplinary evolution and developments of the behavioral component. From their studies of recent cognitive/mentalistic literature, the Chinese professionals had learned little about the advances in behavior science or about the movement that culminated in establishing behaviorology as an independent discipline.

In addition, Chinese professionals had noted that their update of traditional Western psychology yielded little of practical use in dealing with the cultural, social, or personal problems to be addressed. But some Chinese scholars subsequently discovered that reports of more practical and wide ranging research and applications do exist in *behavioral* journals like the *Journal of the Experimental Analysis of Behavior* and the *Journal of Applied Behavior Analysis*. That discovery prepared them to attend more closely to the behaviorology that Ledoux had been invited to teach.

Establishing behaviorology in China, however, presents its own set of challenges. The higher education system of China is small relative to the size of the population. The American concept of general education is not widely known. Most Chinese higher education institutions feature a targeted curriculum. Before an academic discipline receives institutional attention, it must meet a requirement for demonstrated applicability. Furthermore, some of China’s senior education leaders have long held outmoded opinions about behavior science in general, and some of these opinions are now thoroughly incorporated into the system. For example, everyone in China who is involved in language training knows Chomsky’s theories. But since applying them effectively in language training is not feasible, few people show much interest in them; they are just something that everyone is expected to know. Unfortunately, the urgent preoccupation with immediately applicable techniques, and the concomitant reluctance to invest in basic science training, leaves the Chinese with little maneuvering room in which to get themselves well trained in the kind of basic behaviorological science that *can* readily spawn the practical and workable behavioral technologies that they seek.

**Historical summary.** Behaviorological science and its philosophy originated earlier than the formally organized discipline that coalesced during the 1980s to accommodate them (see Ledoux, 1997a). E.A. Vargas set priorities, which were included in Fraley and Vargas (1986): “...developing an academic home to reproduce our scientific culture is the larger problem. The middle-sized problem is the organization to foster our radi-

cal behaviorism. ...The smaller problem is what we call ourselves" (p. 54). The smaller problem was solved, and a long term solution for the middle-sized problem was established—steps that will facilitate solving the larger problem.

As the decade of the 1990s began, the culture was still in the grip of psychology (whose mainstream advocates are devoted to understanding a mysterious *internal* locus of behavioral determinants) and theology (whose disciples are devoted to understanding a mysterious *external* locus of behavioral determinants). But an organized behaviorology movement was present as well, a movement providing a natural science alternative based largely on the selection paradigm. Here was a discipline that treated behavior as a *naturally* occurring phenomenon. Behavior occurs as dependent variables in functional relations featuring environmental properties as independent variables. Behaviorologists define behavior to include not only mechanical movements of body parts, but also all emotional reactions, and all verbal behaviors (the latter being a large class that incorporates speaking, thinking, awareness, consciousness, knowing about, and similar phenomena). Behaviorologists view all of these as kinds of behaviors, each occurring in accordance with well understood basic principles that often recombine in ways accommodating greater complexity. This brings behavioral outcomes (including the behaviors of affect and intellect) within reach of an appropriate behavioral technology for any applied field. The discipline of behaviorology could provide scientific support for a behavioral engineer addressing behavior-related problems in *any* applied area.

### ***Summary of Chapter Two***

Before concrete actions were taken to launch a newly organized discipline, the concept of that discipline had to be shaped to maturity in the verbal repertoires of many people. Chapter Two described both how that concept arose and the variables that shaped people's responses to it. This chapter also discussed the nature and origins of the behaviorology concept, and its increasingly ill fit within organized psychology, even though much of its origin can be traced in the history shared with that discipline. Disunity in psychology stimulated reconsideration of the place of the *behavioral* science that had begun early in the century—in part with the work of Watson and Thorndike, and subsequently with Skinner's definitive biology-inspired departure from both a strict stimulus-response (S-R) psychology and its attendant preoccupations with the nervous system.

Skinner's paradigmatic revolution brought to the study of behavior a line of biology-based scientific thought for dealing with behavior-related subject matter. It emphasized selection causality and produced an increasingly isolated behavioral scientific community within organized psychology. A continuing question was whether or not psychology would change by adopting that behavior-focused paradigm and its attendant natural science philosophy. The rift eventually attracted the attention of scholars of science including David Krantz who, while apparently somewhat offended by the behaviorists' adamant defense of their scientific integrity, discovered substantial evidence for that integrity. Nevertheless, Krantz's prominent article (1971) emphasized what he implied were rebellious social improprieties. The chided behaviorists were perhaps slowed in their turn away from psychology, and many might have been influenced toward accommodations with traditional psychologists—a trend that certainly became evident.

In the two decades that followed, the behavioral psychologists and semi-independent behavior analysts toyed with the concept of a separate discipline. By the late 1980s

this issue was under intense debate, especially within ABA. When a name (behaviorology) was proposed for a separate discipline, an historical search revealed several different origins for it and for some earlier flirtations with concepts of disciplinary independence. Reviews of the status of behavior science around the world revealed that significant potential for an organized behaviorology movement existed, especially among a subset of the radical behaviorists on the North American continent.

*The next chapter, Chapter Three (“Issues Driving the Independence Movement”) will review and analyze five kinds of reasons to incur the high costs of organizing a separate and independent discipline. ❧*