

Chapter 3:

Issues Driving the Independence Movement

After the introductory Chapter One, Chapter Two of this account of the emergence of behaviorology examined the nature and origins of the behaviorology concept and its emergence worldwide. An idea like behaviorology can rise to the level of a mature concept, but subsequently to do something concrete about such a concept requires strength in the reasons for bothering. This chapter, Chapter Three, explores the increasing strength, in five different classes of contingencies, to incur the high costs of organizing a separate and independent discipline. It reviews the accumulating and intensifying reasons why the early behaviorologists would be led to take the actions that will be described in Chapter Four.

Disciplinary independence movements are costly and not undertaken lightly. Few of the potential early supporters of such movements actually become involved. Various contingencies must operate in some combination: The punishers from which people are escaping must have grown strong. Access to critical reinforcers must have come under serious threat. And participation in a new movement must hold promise of important outcomes, though some might be deferred.

The rarity and difficulty of such personal commitments evoke a special scrutiny with respect to why those commitments occurred. The early behaviorologists were individually affected by different sets of causal factors. This chapter turns from *what* was happening to *why* it was happening, and discusses several of the reasons which, in some combination, might explain the commitment of any given individual to the behaviorology movement. Each of the five following sections explores a different class of motivating variables. The first, concerning *the scientific rift*, addresses, in several subsections, various components of this perhaps most critical of those classes.

(1) *The Essential Scientific Rift*

By the latter part of the 1980s, the decade in which an independent behaviorology emerged, some behaviorists had developed in applied fields without ever having been affiliated with organized psychology. The many behaviorists of all kinds, who did operate within organized psychology, still represented a minority in psychology of only a few percent. For example, a 1990 list of regular members in the American Psychological Association (APA) Division 25, the behaviorist's division, contained only about 1200 people including student and affiliate members (Division 25, 1990). In contrast, in 1987, APA had nearly 65,000 members (Hayes, 1987b, p. 41). By that time the rift between behaviorist and cognitivist/mentalist camps had become a salient problem for many persons concerned with the disciplinary future of psychology. Many argued that the differences did *not* or *need* not conflict. Some even invented new concepts of "discipline" more tolerant of disparity (Staats, 1986). But others emphasized seemingly irreconcilable differences and distrusted the implications of disciplinary integration.

Having thoroughly criticized cognitive psychology in 1977 (Skinner, 1977), Skinner a decade later (1987b) asked rhetorically why “the experimental analysis of behavior as a function of environmental variables and the use of that analysis in the interpretation and modification of behavior in the world at large” have not *become* psychology (p. 782). He offered some reasons in the form of obstacles that he identified as humanistic psychology, psychotherapy, and cognitive psychology. Having described the essential effects of those obstacles, he concluded that:

...by their very nature, the anti-science stance of humanistic psychology, the practical exigencies of the helping professions, and the cognitive restoration of the royal House of Mind have worked against the definition of psychology as the science of behavior. (p. 784)

Skinner, until as recently as May 1989, had always hoped to change psychology into a useful science of behavior. He had presented a lifelong analysis of why it is not. Concurrently he was providing the natural science and philosophy that psychology would have to adopt to fulfill its cultural mission. Yet it remained for the vast majority of psychologists to adopt that science and philosophy.

Some argued that the prolonged dominance of less effective or impractical science in psychology was largely a result of politically defended access to a variety of reinforcers, some extraneous, within that organized discipline. Resistance to behaviorism on the scientific and technical front where Skinner had fought his battles (e.g., see Baer, 1987) was of lesser importance. Others might have been reluctant to turn away from the familiar implications of mentalism. Either way, some therefore saw a *political* rather than a purely scientific solution as appropriate and increasingly necessary. They saw recognizing a behaviorology discipline, separate both in subject matter and organization, as that kind of step.

The scientific rift hinged on two major classes of difference: paradigmatic and thematic—or, roughly, *how* to think, and the subject matter *about which* to think. The following four parts of this section examine significant components of these aspects of the scientific rift.

Mentalism versus behavior-behavior relations. Historically the internal agent has been difficult even to locate much less analyze. In more ancient times false starts had seemed to lead to the lungs or heart. But many contemporary psychologists, following the quest of these predecessors going back to the time of the ancient Greeks and Romans, now seek the elusive agent in the brain. An organ (the brain), rather than an organism, is said to behave. Presumably, the brain acts like another person within. It processes this, recognizes that, perceives something else. Those who accept the premise of internal autonomous causal agency are limited in their scientific approach to searching out some physical reality presumed either to be the heretofore ill-described agent or to be symptomatic of its ethereal presence. In either case the agent is circularly inferred from the same behavior to be explained (Michael, 1989, Ch. 6; Skinner, 1953, Chs. 1–3). From a natural science perspective, laboratory methodologies are wasted to investigate what is not really there, while legions of students are trained to replace the professionals expended on the crusade. (An enlightening exposition appears in *Beyond Freedom and Dignity* [Skinner, 1971, Ch. 1].)

Behavior analysts Hayes and Brownstein (1986), in their article “Mentalism, Behavior-Behavior Relations, and a Behavior-Analytic View of the Purposes of Science,” argued that the majority of psychologists are mentalists because their philosophies and

sciences provide no analytical means to avoid mentalism. They noted that treating thinking as a “mental” event treats it as a hypothetical “non–spatiotemporal” activity—that is, as non–natural.

In contrast, radical behaviorists have long regarded thinking as *behavior*, mostly verbal and controlled, as is all behavior, by variables in the natural environment. Thinking is apparently all, or nearly all, behavior of the operant kind. Though usually covert, thought has the same properties as more overt behavior (also see Moore, 1981, 1984). Construing thought to be natural, radical behaviorists transform the troublesome question of how metaphysical mental events can control physical events, like behavior, into the more rational and answerable question of how covert behavior can control overt behavior. Hayes and Brownstein referred to that class of relations as “behavior–behavior relations” wherein the second behavior (an observable overt kind) occurs under the antecedent control of a preceding behavior (such as a thought). These sequential relations, though at times obscure, are not mystical. They merely involve two kinds of behaviors—a covert one serving an antecedent stimulus function for the next. And, especially when contemplating intervention, behaviorologists are quick to go back a link in any causal chain and search in the accessible environment for the functional public antecedents of that covert event. By tracing the functional relations back to events in an *accessible* part of the environment, they locate potentially manipulatable variables. This affords control over the subsequent internal and otherwise inaccessible parts of the sequence as well as over the external parts. The previously insoluble problem, recast in this way, becomes routinely manageable. (Also see Hayes & Brownstein, 1985.)

The mentalistic account of thinking, prominent in psychology, does not even reach the point of behavior–behavior relations, in part because the controlling antecedent variables, called thoughts, are seldom if ever regarded as behavior. The psychological analysis of an observable overt *behavior* that has been “stimulated” by an antecedent covert behavior usually stops at the interface between the exterior and interior domains. The behavior is regarded merely as the final emitted stage of some changing entity that undergoes a transformational sequence—usually entering the body as information and emerging as behavior. Thus, transformations from some preceding state in the internal domain are thought to yield the observed behavioral manifestations. That internal domain is, by some accounts, possessed of mysterious properties, and metaphysical events thus supplement the processes. In other accounts, information is accepted as something real, and therefore real mental processors have to be hypothesized to deal with it. Without the kind of science necessary to carry analyses of multi–term contingencies back through the functional chain—and tending to attach little importance to such an effort anyhow—mentalists are ill–prepared to avoid what behaviorologists construe to be the fallacy of original internal causation (Sidman, 1986a; Skinner, 1969).

Conversely, behaviorologists regard “mental” events as covert behavior, mostly (if not all) verbal. An understanding of these events can support *prediction* of the overt behavior that they may share in controlling. However, behaviorologists do not regard analyses to be complete until not only prediction but also *control* is attainable. That usually requires tracing any causal chain further back ...back to locations—generally outside the body, in the external environment—where controlling variables are *accessible*. Behaviorologists describe these analytical moves to the exterior in terms of “four–term contingencies of reinforcement” (see E.A. Vargas, 1991b). Unless that move to accessible variables is accomplished, effective behavioral technologies, which depend on

manipulating accessible independent variables, cannot be developed by design and must emerge, if at all, as intuitive or scientifically unsupplemented practice.

Engineering versus inquiry. Cognitivists sometimes *do* attain control adventitiously in direct response to practical contingencies. But too often when that happened, the radical behaviorists were left to contend with invalid claims of credit made by the cognitivists on behalf of the prevailing cognitive science.

Mentalists sometimes disavow control as their goal, because their approach does not readily facilitate analyses that backtrack through the chain of functionally related variables to the “environment.” (Radical behaviorists define “environment” as that domain, part of which might be inside the skin, in which independent variables *can be manipulated in behavior–controlling relations.*) Instead of stressing control, mentalists emphasize “understanding.” Their practical work is often confined to predicting behavior from other behavior—as when behavior on a specific occasion is predicted from a putative causal trait induced from behavior on previous occasions. For example, educational psychology courses in the measurement of behavior typically ignore direct measurements of the properties of behavior (as delineated, for example, by Johnston and Pennypacker [1980, Ch. 7]) and instead feature textbooks devoted to identifying and measuring the intensity of what are assumed to be behavior causing traits.

The quest to gain control of behavior is sometimes criticized as an allegiance to superficiality. However, as Hayes and Brownstein (1986) wrote, control

...is a required element for the successful functioning of the [behavioristic] perspective. Thus, mental causality is a form of theorizing rejected because its pursuit threatens the successful operation of science as viewed from the standpoint of behavior analysis. [p. 187] ...an emphasis on prediction and control is not arbitrary in behavior analysis because it is a necessary part of successful forms of the philosophy [of radical behaviorism] that underlies behavior–analytic theorizing. (p. 175)

Failure to include control as a planned final step condemns a science to immaturity (see Skinner, 1953, Ch. 2). As a practical matter, those who would not attain control in their subject matter can only *interpret* life in various ways. And mere *interpretations* evoke little threat to the acceptance either of the science that informs them or of the persons who offer them. But most important scientists echo Gould’s (1987) notion about life—that “the point ...is to change it” (p. 154). Those whose science carries to the level of effective control must confront a world well organized to defend established modes of access to long and strongly conditioned reinforcers. Those who resisted the behaviorologists did so in ways never wasted on others who merely teased with counter interpretations but who lacked the technologies to affect actual changes.

Among persons working under the rubric of psychology, those pursuing the developing behavioral paradigm increasingly gained more of a capacity to control behavior than did the cognitive/mentalist remainder. Reinforced for their successes in the domain of practical behavior engineering, the behavioral people increasingly concentrated on exploiting *that* aspect of their science. This widened the scientific rift, because the cognitive/mentalist psychologists, lacking that opportunity, focused their efforts on understanding, in their own way, the inner workings of what they construed to be a behavior–controlling mind.

Quality of science issues. Since the concept of mind had been induced entirely on the basis of behavioral evidence in the first place, efforts to explain behavior through ap-

peals to the features and functions of the mind were logically circular. This offended the scientific sensibilities of many behavioral people, whose analytical paradigm paralleled the non-circular logic common to the well-established natural sciences. When, across the last half of the twentieth century, the cognitive/mentalistic psychologists turned increasingly to physiologically based brain science for independent corroborations of their theories of mind, the future behaviorologists again took umbrage, because old theory was being carted to new evidence in *theory-biased searches* for fits and matches. These were inevitably “successful” because something physiological is always occurring internally. No matter how fanciful or far-fetched the theory, some physiological activity is always present to be correlated with the behavioral events said to be external representations of whatever internal functions the theory hypothesizes—an approach prone to fallacies and low on the quality scale in scientific practice. Some future behaviorologists, especially those more sensitive to quality of science issues, were embarrassed before the at-large community of natural scientists by the psychology community to which many of them were nominally attached. And, becoming more so as time passed, they increasingly recognized the need to regroup separately; they wanted out.

Irrelevant subject matter. The physiological alternative to non-natural mentalism pursued by many psychologists focuses on a subject matter somewhat irrelevant to behaviorology. Behaviorologists distinguish between (a) the valid natural science of physiological linkages between behavior and environment (a field of study in which they have had limited though valid scientific interest) and (b) the science (or pseudo-science) of mentalism which they construe to be an indulgence in metaphysics unworthy of status in the scientific community at large. Unfortunately, those two scholarly traditions have remained so intertwined within psychology that they can be difficult to discriminate there.

Those scholars of mind who eschew a metaphysical mentalism and assume thinking to be a purely natural physiological activity nevertheless also depend on cognitive science. But that approach does not bring thoughts and feelings unequivocally into the realm of behavior in accordance with the kind of scientific principles recognized by behaviorologists. Those cognitive psychologists probe for independent, non-behavioral variables in the nervous system, a line of inquiry going back to when people first noticed that from nerves come the behavior-initiating impulses that stimulate muscles. But because the activity within nerves only mediates the functional relations between a controlling environment and exhibited behaviors, that effort can be somewhat like searching myopically through the mechanical steering linkages of an automobile in an effort to discover the driver who remains external to that system and cannot be found within it. From the behaviorological perspective, the behavior-mediating neural events that might be discovered there, though necessary if real, are little relevant to the practical operations that constitute behaviorological technologies. The classic argument was set forth by Skinner (1938) in his often-quoted chapter 12 (entitled “Behavior and the Nervous System”) of *The Behavior of Organisms*. There Skinner defended his assertions

...not only that a science of behavior is independent of neurology but that it must be established as a separate discipline whether or not a rapprochement with neurology is ever attempted. (pp. 423–424)

E.A. Vargas put it this way (1990; also see E.A. Vargas, 1991a):

The fact that cellular action underlies every muscle action that underlies every limb movement that underlies the behavior of two teams playing foot-

ball does not explain why the game is played as it is or why those teams play *that* game.

The extensive physiologizing by cognitive psychologists also shifted their subject matter into disciplinary territory claimed by physiology. However, whether those internal events are real events or not, the independent variables that are relevant to practical concerns always remain in the behavior–controlling environment or milieu. The inter–nerve variables that play a mediating roll between independent variables and behavioral manifestations do not lend themselves to feasible intervention even if one could possibly determine, on given occasions, what changes within those nerves might be worthwhile. Thus psychologists cannot attain the capacity for control over behavior through analyses focused on nerve functions.

Increasingly, the behaviorists within psychology exploited their growing capacity to develop effective behavioral technologies based on the manipulation of external variables. They were becoming behavioral engineers, and discovering largely untapped markets for those skills in all facets of the culture. Internal physiological linkages, though necessary, played little or no role in the practical interventions undertaken to produce behavior change and remained of little concern to the behaviorists. Most others who called themselves psychologists—a vast majority—continued to focus on those internal events. The subject matter difference was becoming ever greater.

Summary of the scientific rift. Most early behaviorologists saw themselves organizing a new disciplinary support structure—a scientific verbal community—apart from the organized discipline of psychology. They viewed psychology as a discipline focused, with an unreliable mix of natural and non–natural science, on subject matters largely irrelevant to behaviorological concerns.

Behaviorologists, in separating from psychology, would be leaving the enormous problem of purging that discipline of non–natural mentalism to the psychologists. Should the psychologists eventually succeed in doing that, the residual natural science of internal events will still pertain to a field largely apart from behaviorology. A demystified psychology could leave its natural scientists in far more territorial conflict with the discipline of physiology than with behaviorology. As Skinner (1974, Ch. 13; 1983b, 1987b) has noted, the discipline of physiology relates internal physiological events to observable behaviors as an aspect of *its* mission, and it is far better prepared to do so than is psychology.

The work of behaviorologist Carl Cheney (1988, 1991, 1992) later exemplified the *appropriate* natural science bond between behaviorology and physiology. In his work Cheney completes accounts of some behaviors by treating the body as a set of independent variables in environment/behavior relations. That differs from attempting to exploit the findings from physiological studies to validate what most behaviorologists construe to be ill–conceived mentalistic theorizing.

(2) Behaviorological Training Issues

The problems in training behaviorologists are largely those of training scientists in any discipline that provides complex scientific alternatives to common wisdom and traditional assumptions. But in the 1980s those who would soon acknowledge themselves as behaviorologists faced increasing difficulty training their students within their respective nominal disciplines, especially organized psychology and its satellite fields. The repertoires of behaviorology—its philosophy, science, technologies, and vocabulary—are as

extensive, and as difficult and demanding to acquire, as those of any other scientific discipline. The verbal community of behaviorology, like those of other natural sciences, must therefore claim access to the controls on all of the discipline-related behavior of its trainees. This monopoly on control over training in behaviorology is further necessitated by a particular characteristic of the task: The substantial lore about behavior that pervades the general culture will have informed the prior training of behaviorology students in nearly all subject matters. The training must eliminate that legacy from their technical repertoires as it shapes their new verbal skills (just as it must for students of other natural sciences so that, for instance, astronomy students learn not to refer to “the sun rising” in technical contexts).

Where behaviorologists and psychologists have been compelled to share the same training program and compete with each other for sufficient student contact to get students properly trained, both sides have tended to regard as extreme any efforts of the other to monopolize the training. But to master behaviorology, like any other complex natural science, requires a substantial commitment on the part of behaviorological aspirants. However, “commitment” merely describes the strength of certain controls over the trainees’ behaviors that their verbal and social communities have managed to arrange. A trainee’s aspirations are also products of the community’s behavior-controlling practices. Their aspirations are functions of the reinforcers to which the trainees are conditioned and of the schedules of reinforcement (Ferster & Skinner, 1957) by which trainees are afforded access to those reinforcers. Given (a) the degree of control that must be established between subject matter variables and trainee behavior, (b) the extensive new verbal repertoire that must be acquired, (c) the traditional repertoire that must undergo extinction (at least in technical contexts), and (d) the new and different reinforcers that the trainee must behave to contact (i.e., the new perspective of the trainee in approaching the subject matter), the necessary interaction between trainee and the behaviorological community is too great for the trainee’s time to be shared with the verbal community of another discipline.

To the extent that they dilute the training curricula, compromises with any other discipline reduce students’ opportunities not only to acquire skills in their own discipline but also to acquire the supportive emotional behaviors as well (see Branch & Malagodi, 1980). Also, the trainees’ reliance on the science of their discipline diminishes. For example, Mahoney (1989) reported a longitudinal attitude survey conducted on 42 “influential representatives” of cognitive and behavioral psychology using a “lengthy questionnaire” on “beliefs about scientific method, the nature of learning, and so on.” Having discerned three subsets among his *psychologist* subjects—(a) extreme behaviorists, (b) cognitive-behaviorists, and (c) extreme cognitivists—Mahoney reported that all three groups had become more cognitive, though behaviorists changed least. The extreme cognitivists found even less merit in behavioral ideas than what little they previously conceded, while the cognitive-behavioral psychologists had shifted the balance in their views significantly toward the cognitive perspective. Mahoney noted that:

...both cognitive-behavioral and extreme behavioral respondents showed signs of shifts in the direction of personal agency as an important counterbalance to exclusive environmental determinism. Cognitivists appeared to be going even further in this direction, exhibiting a clear trend toward acknowledging the influence of unconscious processes. (p. 1375)

Mahoney found that behaviorists *in psychology* were increasingly variable in their “meanings and models of behaviorism” which in his view were “in the throes of dramatic revision”—away from what he characterized as the extremism of radical behaviorism. Extremism in this context, though, mainly reflects characteristics that imply a different discipline whose practitioners resist training compromises that degrade the scientific repertoires of their students.

Furthermore when students, whose skills and enthusiasm for behaviorological science are reduced by mixed training, eventually succeed the behavioral faculty members in the programs of research, practice, and training, they tend to enter into those positions as less competent behavioral experts than the former mentors whom they replace. This reduction in the behaviorological competence of each succeeding generation of faculty decreases the capacity of the science to contribute to the culture. (For an early warning, see Michael, 1980, p. 16; for an analysis of the implications when one operates concurrently in incompatible verbal communities—a probable circumstance for behavioral psychologists—see Fraley, 1984; for tactics to nurture behavioral repertoires within non-supportive environments, see Morse & Bruns, 1983.)

Many training problems in behaviorology stem from delaying the study of behaviorology until a student enters higher education—in many cases, graduate school. Unlike other basic sciences, which are introduced to students in primary and secondary schools, behaviorological science *under any label* has been generally unavailable to students until they have become adults. In contrast, mentalistic and cognitive psychology courses are occasionally offered in high schools. Perhaps worse, most kindergarten-through-twelfth grade curricular materials on other subjects are heavily laced with inaccurate references to behavior that are based on the mentalistic assumptions prevailing throughout the culture. Behaviorologists support replacing that cognitive/mentalistic psychology with appropriate, practical behaviorology in those curricula (e.g., Ledoux, 1987a). A strictly natural science, behaviorology provides young learners with *explicit* principles by which to deal more effectively with behavior in *practical* contexts.

Some behaviorologists have deemed as acceptable the practice of providing behavior-related training for lay persons and beginners *in lay language*, believing that students would then more readily understand behaviorology. But lay language is implicitly cognitive and mentalistic—a legacy of Western cultural history (Ledoux, 1997a; based on Lerner, 1991). That language legacy maintains a counter-implication to the very concepts that the teachers would be seeking to establish. The rather widely understood realities of the *physical* world, which are contrary to ancient and mistaken wisdom, are appreciated by lay persons to the extent that the technical language of physics has become part of the ordinary citizen’s language. *That* circumstance is the legacy of two or three centuries of science teaching in the schools. Other behaviorologists have therefore counter-argued that the most educationally sound approach to teaching the science of behaviorology is to get people to talk and think in behaviorological language—and to do so as part of their everyday mode of expression.

On a more practical level, some behaviorologists who were attempting to train students in university units controlled by other disciplines found themselves without behavior laboratory facilities. Administrators representing the interests of other disciplines do not tend to divert resources to the laboratory space and equipment that behaviorologists deem important. In 1993 Ernest Vargas, after years of unsuccessful efforts to secure behavior laboratory space at the College of Human Resources and Education

(West Virginia University), opened a laboratory in the basement of his home. With assistance from Carl Cheney, he and graduate student David Feeney began to conduct behavior research. This represented the first experimental laboratory facility opened under the general umbrella of organized behaviorology. A small research support grant to Carl Cheney from The International Behaviorology Association—the first such grant made by that organization—defrayed some of the costs.

Summary of training issues. Some behaviorologists came to the movement in part out of a conviction that their discipline could only continue and advance through relief from the constrained and limited training opportunities available, especially within the psychology community. However, behaviorologists must still solve the curricular and tactical problems intrinsic to instruction in any basic natural science. Creative interim measures have included private ventures to fill gaps in research and training opportunities within university units controlled by advocates of other disciplines.

(3) Employment Opportunities

The modern psychology establishment, for its part, has already arranged a kind of separation based on hiring. “Behavioral” psychologists are, in general, no longer hired in psychology. For example, the September 1989 issue of the *APS Observer* (The coherence, 1989) advertised over 85 employment opportunities for psychologists organized by categories, including 15 under “Cognitive Psychology Positions.” No corresponding category for behavioral psychologists was present. Most of the remaining ads specified or implied a cognitive orientation among desired applicant characteristics, while no ad was equally explicit about a behavioral orientation. Earlier, Epstein (1987a) had reported finding a similar pattern in the *APA Monitor*. For some years he had been tabulating the Monitor’s job listings and considered those for 1985 as “typical for nearly a decade.” He called them “good indicators of the composition of future psychology departments.” He found no entry-level, tenure-track positions for “animal learning, behavior analysis, or any related area” (p. 127).

The rare advertisement for a “behavioral” psychologist is the exception that proves the rule: Mainline psychology continues to provide no niche for behavioral people. One can infer that psychologists do not construe the behavioral approach to represent their field. Behaviorologists agree. Since behavioral types did not seem likely to get hired as psychologists, the behaviorology movement became attractive in part because it would afford access to the job market under an independent disciplinary label. Applicants also hoped that their entrance into the job market as disciplinary independents would facilitate appealing to an even broader spectrum of potential employers than those traditionally seeking to hire psychologists.

Summary of employment opportunities. The appeal of the behaviorology movement was increased in part due to few positions being available for behavioral people within job markets controlled by organized psychology. The appeal also increased in part because a broader job market seemed accessible through training provided by an independently identified behaviorology discipline.

(4) Capacity for Contributions to the Culture

Whether behaviorists could fully provide their potential contributions to the culture (Fraleigh, 1987) without an independently organized discipline had, by the mid-1980s, become an urgent question for many of them. Increasingly they saw important

long-term implications in that issue. The contributions of behaviorology, both actual and potential, were being ignored and often stifled by the traditional bond to organized psychology. Behaviorologists encountered impediments in the areas of training and funding—and, in the perception of many of them, behaviorologists were denied appropriate recognition, respect, and other common consequences of successful and meaningful scientific and academic endeavors (see Fraley & Vargas, 1986).

Psychologists of many varieties continue to denounce behaviorism and behavioral approaches while enthusiastically describing the latest cognitive and mentalistic models. In some cases those psychological constructs depend on reiterations, in imprecise cognitive language, of well established, purloined, behaviorological principles that have found their laundered way into the cognitive literature. In one example Kauffman, describing conceptual models in the fourth edition of his *Characteristics of Behavior Disorders of Children and Youth* (1989, p. 85), regarded contributions from behavioral research as inadequate and touted the putatively more complete “social cognitive theory” said to represent the “natural-science perspective.” That model has two main features. One deals with behavior in its relations with a controlling environment. Although Kauffman gave no hint of it, those relations constitute a basic *behaviorological* concept (attributed in part to Bandura, although Skinner had been the one who had earlier spent years experimentally probing those functional relations and authoring several books developing that concept). The second main feature of social cognitive theory is a reliance on “personal agency.” This is a remnant of theological epistemology to which many cognitivists cling in outright antithetical rejection of natural science. “Personal agency” requires that one either be ignorant of the science of verbal behavior or fail to appreciate it. (For another example, see Fraley & Vargas, 1986, pp. 49–51.)

Vicki Lee (letter to Fraley, 15 June 1989) mentioned a further typical example. In her native country, Australia, where “behaviorological activity is virtually nonexistent,” one of her psychology colleagues recently published a paper reporting the “discovery” that reinforcers have to be identified empirically. That point is of fundamental scientific importance. It is also an essential point reiterated in the behaviorological literature from the beginning (see Skinner, 1938) and has always been included as an elementary principle in introductory behaviorological texts (e.g., Skinner, 1953; Fraley, 1996a).

On 22 January 1991 an English language broadcast in China provided yet another example of educational psychologists “discovering” a half-century-old behaviorological principle. Some educators in Hunan Province reported documenting a “new” teaching method that yielded substantial improvements in students’ academic performance. The “new” method was positive reinforcement. Students were positively reinforced for exhibiting the desired academic behaviors. In 1938 the historical behaviorology literature reported laboratory documentation of the basic relation in the positive reinforcement process (Skinner, 1938). By 1953 the process had been elaborately tested and documented, and its wide applicability was discussed in engineering detail in the literature (Skinner, 1953). And by the 1960s entire books were detailing its specific adaptations in educational practice (e.g., Skinner, 1968). (At the time of this discovery, the Chinese were completing a ten-year long attempt to update a 30-year loss of contact with the West; the Western *psychological* sources for their update would have been ill-prepared to inform them about this application. See Ledoux, 1997d.) Even the half-century-old *basics* of behaviorological science often have to await “discovery” by psychologists who then invalidly attribute the favorable outcomes to their own disci-

pline. But in most such cases, the technological practices in question would not have followed as logical implications of the kinds of basic assumptions upon which psychology rests.

Some people counter examples like these by insisting that ultimately what is important is how well humans deal with their world, not *who* makes the discoveries or gets the credit. But such arguments are economic nonsense for a discipline that must compete in the cultural marketplace for its resources in order to continue to make its contributions. Furthermore, in pirating intellectual merchandise from behaviorology, seldom are the practices simply borrowed and properly used (though under false labels). Separated from their quality-controlling philosophy and underlying scientific principles, the practices become distorted until eventually they lose their effectiveness. Then they are discredited and discarded.

Additionally, the *kinds* of threats facing the global culture (Skinner, 1978, Ch. 2; 1982; 1984; 1987a, Ch. 2), and even our species (Marcattilio & Nevin, 1986; Ulman, 1986), implied urgency in getting those threats reduced. That urgency provided a prompt leading some participants toward the behaviorology movement. Under the circumstances some early behaviorologists did not believe that time was available for mainstream psychology's bit by bit rediscovery of behaviorological principles so that eventually those principles might be applied, by way of the psychological establishment, to our cultural problems. A separate discipline was needed, and needed right away so that it could help insure a viable future for humanity.

Perhaps global human culture is already fatally and irrevocably flawed (Skinner, 1987a, Ch. 1; Ulman, 1988). Or, instead, perhaps time for meaningful change is short. Organized psychology has from its inception been allowed to be keeper of the behavioral science upon which our culture depended. Psychology has had the mission to furnish both the scientific foundations upon which to build workable behavioral technologies and the analytical power to correct flawed policy in behavioral matters. Other cultural agencies such as organized religion, government, and education function (with some help from psychology) as instruments of organized eclectic behavioral lore. But organized behavioral *science*, traditionally represented by psychology, has had the mission to resist mystification of the human condition and provide more culturally propitious alternatives. Behaviorologists, voting with their feet, judged psychology to have failed in *that* mission.

However, the organized verbal community of psychology *had* succeeded in conditioning legions of people, including many of its critics, to feel inhibited about bluntly exposing that failure. Behavioral psychologists remain sensitive to their vulnerability within psychology. But behaviorologists, in circumventing psychology, have by design developed a discipline that requires neither persuading psychologists to change nor avoiding analyses criticizing the cultural role of organized psychology—although the choice of the most appropriate and effective ways to engage in that criticism remains an important strategic issue.

Perhaps psychology tried to do too much. Studies of mind do not spawn the practical behavioral technologies needed throughout the culture for solving major behavior problems. Behaviorology does that. A disciplinary division of labor seemed appropriate to the early behaviorologists, especially in view of the failure of psychology to prove its worth as an explicitly studied behavioral foundation science. Here is just one example of the conditioned neglect of psychology as a worthwhile foundation science: Among all the graduate degree programs in 17 different fields that focused on hu-

man behavior, listed in the 1990–1992 *Bulletin of the West Virginia University Graduate Catalog*, none were found to require *any* courses in the psychology department, although three required their own versions of traditional psychology courses. Fourteen of those seventeen departments posted no requirements for any coursework centered on psychological subject matters. The trainers in most behavior–related fields do not appear to regard as worth their students’ time and money whatever contribution psychology might make as a basic science foundation for their respective fields.

This neglect extends to the lack of representation of psychology in the general scientific literature in the culture. Skinner, who worked to change psychology into a worthwhile natural science, was troubled by that neglect because it reduced the contributions from his work. In a paper entitled “Whatever Happened to Psychology as The Science of Behavior,” Skinner (1987b) complained that the editors of *Science* magazine tended to avoid publishing psychology articles and observed that apparently they “no longer regard psychology itself as a member of the scientific community” (p. 784). To the extent that Skinner’s interpretation was correct, behaviorologists tend to side with the editors. Focusing more narrowly, perhaps the editors of *Science* are reluctant to entertain significant *behavioral* contributions because they construe these to represent psychology. Can natural scientists who maintain a close and affirmative affiliation with what is being construed as a pseudo–scientific discipline expect other than to stand “damned by association” within the community of natural sciences?

Also, the literature of psychology has included distorted views of behavioral principles and products put forth as straw men to suffer invidious comparisons with cognitive and mentalistic concepts. Such continuing misinformation reduces the effectiveness of behavioral contributions to cultural evolution. One well known example has been the consistently inaccurate treatment in psychological commentary concerning the airtrib invented by Skinner (see Skinner’s foreword in Ledoux & Cheney, 1987; also Skinner, 1983a, pp. 385–386).

Summary of capacity for contributions. Behaviorologists pursued their separatist course partly to create an independent literature. That literature would report behaviorological contributions accurately. This would allow those works to find their way to effective cultural applications through authoritative print sources more respectful of their validity. But behaviorologists did not pursue independence simply to gain control of their own work, their own literature, and their own training programs. A more global objective was to afford the independent disciplinary status necessary for effectively interacting with other facets of the culture in providing behaviorological contributions.

(5) Control of Disciplinary Infrastructures

Difficulties arise in resolving the many *kinds* of contingencies in the mix necessary to bond a scientific discipline. Organized disciplines do not operate only under scientific contingencies. They also maintain political, economic, and social contingencies to regulate their members’ professional lives. Historian Daniel Bjork noted that in the public view the essence of psychology is mystery rather than fact, and offered this speculation (letter to Fraley, 14 February 1989):

Perhaps most American psychologists don’t really *want* a science at all. Rather they want a profession. That is another way of saying they want status most of all. Moreover, in behavioral terms, isn’t “status” more reinforcing than “science”—especially in America.... Reduce it to science and the media loses interest.

Psychologists themselves have struggled over this and related issues. Their struggles precipitated a divisive crisis within the APA (discussed in detail in a later chapter).

Even though a guild and profession focused majority might benefit in important ways from a scientific minority's products, it cannot let that minority gain control of the political, economic, and social infrastructure of the organized verbal community if that infrastructure is what sustains that majority. Although scientific psychologists of all kinds were, increasingly over the years, adversely affected, a disproportionately large number of *behaviorists* in psychology felt exploited in this way. Many behaviorists had spent years bringing to the attention of their more mainstream colleagues how effective and applicable their evolving behavioral technologies could be. But this often left the contributions of mainline psychologists open to invidious comparisons whether that was intended or not.

Since a traditional operating dictum in a scientific verbal community is that those with the demonstrably most effective repertoire are entitled to inherit leadership, a *political* suppression of any such minority can result. Giles (1984) posed the following question to some "diplomatic" behavior therapists whom he deemed to be too deferential toward their mainstream psychology colleagues. Referring to how, in their publications, those behavioral therapists tended to obscure the behavioral science that actually informed their work, Giles asked them: "With the evidence so substantially in favor of behavior therapy, why do you state your conclusions in such guarded fashion?" Giles reported that "without exception" they responded that "they perceived aversive consequences from taking a strong position in favor of behavior therapy." Furthermore, ...in general, they were fearful of incurring antagonism from non-behavioral clinicians; of being labeled rigid, biased and hostile; of bitter replies in the literature...of rejection of papers submitted for review or greater difficulty in obtaining tenure or alternative employment. *Some of the respondents were able to document loss of employment or refusal of promotion as a result of their orientation.* (p. 24; emphasis added)

A more explicit example concerned a behavior analyst attempting to operate as a psychology faculty member. With respect to the attacks that are sometimes mounted to preserve psychology, such professors of behavior analysis are no safer (and perhaps are less so) than behavior therapists. As reported in the article "Court Battle" (1991), Dr. W. Joseph Wyatt, who edits and publishes *Behavior Analysis Digest*, had been denied tenure at Marshall University,

...because he was a behavior analyst.... In recommending against Wyatt's tenure the psychology chairman held, in essence, that being behaviorally oriented had rendered Wyatt incapable of understanding psychology generally. "He argued that because behavior analysis was not psychology's 'mainstream' my knowledge base was insufficient to allow me even to teach introductory psychology...."

The chairman also held that Wyatt's research was unacceptable because it was published in behavioral journals and presented at the Association for Behavior Analysis....

This approach touched anti-behavioral nerves with several of Wyatt's colleagues. One claimed to have thoroughly studied (and rejected) behaviorism, but could not name the terms of the three-term contingency.... (p. 2)

These challenges, which some might label intellectually dishonest, were met by Wyatt in the courts. He said, "We all must stand for something, and for me behavior

analysis and a faculty member's right to it as a legitimate point of view were worth taking a stand for" (p. 3). The county circuit court agreed with him and ruled on the issue; as a result,

...Wyatt, with the judge's order in hand, will return to Marshall University's faculty.... (p. 3)

While justice prevailed in the end, the energy that goes into these fights is not available for efforts that benefit the culture in more direct ways. The separation of competing epistemologies into their own academic disciplinary homes should reduce such battles for control of a shared disciplinary infrastructure.

Three additional examples of similar suppression were provided in detail by E.A. Vargas, Spangler, Stone, and Wishon (1988).

Summary of disciplinary infrastructures. These kinds of battles for control of the disciplinary infrastructure—especially that of organized psychology—helped propel some behaviorists toward the incipient behaviorology movement and its political solution of organizing the discipline separately.

Summary of Chapter Three

Different classes of contingencies, each class affecting individuals to varying degrees, controlled personal commitments to the behaviorology movement. Among the kinds most frequently identified as having been important in individual cases were the five discussed in this chapter. These pertained to paradigmatic incompatibility (mainly with psychology), control of arrangements to train future behaviorologists, potentially better approaches to professional job markets, improvements in one's capacity to make scientific contributions to the culture, and control over the organizational infrastructure of scientific verbal communities.

The next chapter, Chapter Four ("The Transition Period: Organizing the Discipline and Developing its Infrastructure") will present a comprehensive review of the activities to establish the organized discipline of behaviorology, and will examine the cultural engineering by which the newly named discipline was formalized and debuted in the scientific community. ↻