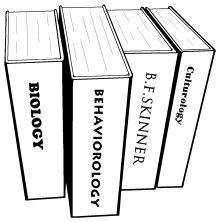

Behaviorology Today

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Behaviorology *Conductologia* *Xingwei Xue* *Gedragsologie* *Behaviourology*

About Behaviorology

Behaviorology is an independently organized discipline featuring the natural science of behavior. Behaviorologists study the functional relations between behavior and its independent variables in the behavior–determining environment. Behaviorological accounts are based on the behavioral capacity of the species, the personal history of the behaving organism, and the current physical and social environment in which behavior occurs. Behaviorologists discover the natural laws governing behavior. They then develop beneficial behavior–engineering technologies applicable to behavior related concerns in all fields including child rearing, education, employment, entertainment, government, law, marketing, medicine, and self–management.

Behaviorology features strictly natural accounts for behavioral events. In this way behaviorology differs from disciplines that entertain fundamentally superstitious assumptions about humans and their behavior. Behaviorology excludes the mystical notion of a rather spontaneous origination of behavior by the willful action of ethereal, body–dwelling agents connoted by such terms as *mind, psyche, self, muse,* or even pronouns like *I, me, and you.*

Among behavior scientists who respect the philosophy of naturalism, two major strategies have emerged through which their respective proponents would have the natural science of behavior contribute to the culture. One strategy is to work in basic non–natural science units and demonstrate to the other members the kind of effective science that natural philosophy can inform. In contrast, behaviorologists are organizing an entirely independent discipline for the study of behavior that can take its place as one of the recognized basic natural sciences.

Volume 11, Number 1 **Spring 2008**

Contents

Note: This issue does not contain any TIBI online course syllabus. In some future issues, new syllabi or updates of previous syllabi will appear. (See the *Syllabus Directory* near the back of each issue.)—Ed.

Volume 11 Number 2 Contents Plan

Here are some of the featured items planned for the next issue (Fall 2008) of *Behaviorology Today,* although these plans may change:

- *Natural Science, Superstition, & Academic Institutions Part I (of II)* (Lawrence E. Fraley).
- The last two (of seven) chapters of "Origins, Status, and Mission of Behaviorology" (Lawrence E. Fraley & Stephen F. Ledoux).
- *Coercion: The Real Parent Trap Part 2 (of 2)* (Glenn I. Latham).
- An article or two from among those that may be in process from various guest authors. *When will* **your** *article arrive?* (Staff writers can maintain the publication schedule with worthy contributions, but worthy articles from guest authors make even more valuable disciplinary literature contributions.)—Ed.

As part of the organizational structure of the independent natural science of behavior, *The International Behaviorology Institute* (TIBI), a non–profit professional organization, exists to focus behaviorological philosophy and science on a broad range of cultural problems. TIBI sponsors an association (the TIBI Association, or TIBIA) for interested people to join, supporting the mission of TIBI and participating in its activities. And *Behaviorology Today* is the magazine/newsletter of the Institute. The guest and staff writers of *Behaviorology Today* provide at least minimally peer–reviewed articles as well as, on occasion and with explicit designation, fully peer–reviewed articles. They write on the full range of disciplinary topics including historical, philosophical, conceptual, educational, experimental, and technological (applied) considerations. Please join us—if you have not already done so—and support bringing the benefits of behaviorology to humanity. (Contributions to TIBI or TIBIA are tax–deductible.)

Origins, Status, and Mission of Behaviorology Chapter 5 (of 7)

Lawrence E. Fraley
Stephen F. Ledoux

Editor's Notes: Nearly 20 years have passed since the official organizing of behaviorology as a separate and independent natural science of behavior, and today the authors would phrase some of the points of this paper differently, or at least more clearly, as well as make additional points (see Fraley, L.E. [in press] *General Behaviorology: The Natural Science of Human Behavior.* Canton, NY: ABCs). Still, this multi–chapter paper, written early in this period by participant–observers of those events, reviews the contingencies compelling—both then and now—these organizational directions. The seven chapters of this work appear, one or two at a time, in consecutive issues beginning with the Fall 2006 issue (Volume 9, Number 2). Chapters 1–5 end with only the references cited, although these appear exactly as in the full reference set which follows Chapters 6–7.

The five main parts of this paper are Chapters Two through Six. Chapter Two (**The Evolution of the Concept 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. Chapter Three (**Issues Driving the Independence Movement**) explores the increasing strength, in five different classes of contingencies, to incur the high costs of organizing a separate and independent discipline. Chapter Four (**The Transition Period: Organizing the Discipline and Developing its Infrastructure**) presents a comprehensive review of the subsequent activities to organize the behaviorology discipline and considers the cultural engineering by which the newly named discipline was formalized, rendered operational, and installed in the scientific community. Chapter Five (**The Continuing Debate: Reactions from the Behavioral Community at Large**) reviews the prevailing cultural milieu and analyzes the support for, and the opposition to, the behaviorology movement, as well as some self–management problems facing those who were taking the lead in formalizing the behaviorology discipline. Chapter Six (**Interdisciplinary Context: A Cultural Role for the New Discipline**) emphasizes the prevailing views of the early behaviorologists on where their discipline fit both among the community of natural science disciplines extant in the culture and in the cultural marketplace. It also comparatively explores the different levels of analysis characteristic of the existing behavior–related natural science disciplines, and examines the cultural basis of resistance to behaviorology.

In early 1987 Ledoux began this paper to analyze the variables leading to the independent development of behaviorological science. As the necessity of the behaviorology movement, and the significance of behaviorology's contributions to the culture, became more apparent, Ledoux invited Fraley to collaborate. More than five years of countless exchanges produced this paper (originally: Fraley & Ledoux, 1997) with each exchange extending and improving the work, and with Fraley's contribution becoming the greater—hence his listing as primary author.—Ed.

Chapter 5:

The Continuing Debate: Reactions from the Behavioral Community at Large

After the introductory Chapter One, Chapter Two of this account of the emergence of behaviorology examined the nature and origins of the behaviorology concept. Chapter Three examined contingencies supporting individual commitments to a disciplinary independence movement. Chapter Four presented a comprehensive review of the activities to organize the behaviorology discipline and examined the cultural engineering by which the behaviorology discipline was formalized and installed in the community of natural sciences. This chapter, Chapter Five, reviews the prevailing cultural milieu and analyzes the support for, and opposition to, the behaviorology movement.

The behaviorologists were refining the concept of behaviorology and organizing a scientific verbal community to protect and extend this discipline. Meanwhile, the general behavioral community continued an increasingly moot debate on the concept of a behaviorology disciplinary movement.

Continuing Debate on the Movement

The International Behaviorology Association (TIBA) was not affiliated with the Association for Behavior Analysis (ABA). TIBA had been established independent of any other existing organization, discipline, or applied field. However, during the 1987 ABA convention, which began the day following the original founding meeting of the behaviorologists, both informal discussion and scheduled formal discussion focused on the behaviorology

movement. This was partly a result of recent articles in *The Behavior Analyst* (ABA's journal) plus some ABA convention sessions mentioning behaviorology.

The question of change strategies. At that convention the title of one well–attended symposium, organized and chaired by Guy Bruce (at the time, a graduate student advisee of Ernest Vargas), was "Behaviorologist, Behavior Analyst, or Psychologist? A Question for the Young Professional." This symposium featured B.F. Skinner as a discussant. Numerous members of the audience supported an independence movement, but others raised objections, including Skinner whose objections went beyond his dislike for the name. But Robert Epstein, who also objected to the name, rose to offer several reasons, with which the behaviorologists agreed, to reorganize the discipline concerned with behavior.

Among those present various arguments arose. Some objections to a separate discipline came from persons who apparently viewed psychology as too big, too powerful, and too well established to be affected by what they saw as splinter movements. Behaviorologists deemed this concern to be irrelevant because they had abandoned the objective of changing psychology (scientifically and organizationally) into a suitable disciplinary home for behaviorology. Instead psychology would be circumvented. Another argument along pragmatic lines resisted the behaviorology movement because organized psychology currently controlled access to critical resources such as jobs, grants, licensing, and reimbursement through insurance. However, behaviorologists did not deem such established control to be immutable. And they saw such concerns as relevant only to those professional opportunities directly controlled by organized psychology, not to professional opportunities in the multitude of other behavior–related fields also in need of an effective basic science. In those diverse fields, the applied practitioners seldom claim expertise in any particular behavioral foundation science and are more open to whatever foundations best support their applied work. Furthermore, within their organized professions they tend to rely less on legally codified prescriptions for traditional cognitive psychology than is done in professions directly controlled by, and generally construed to be parts of, organized psychology.

To many of those attending the 1987 ABA convention, the possibility of a behavioral science separate from psychology seemed remote. So did a mission affecting the culture more broadly than was implicit in those few applied areas in which formal psychology has attained a controlling monopoly in the employment market (e.g., clinical psychology). For some of those people, the struggle for the science of behavior would remain, as it has always been, one of trying to reconstruct and redirect psychology. So, at the Bruce symposium, some speakers again suggested the traditional and well–failed change strategies: (a) attack cognitive/mentalistic psychology in publicly visible ways, especially in print and debate; (b) gain control of conventional courses and then teach them behaviorally both in method and content; (c) write and publish new textbooks reflecting the behavioral perspective on all of the conventional aspects of psychological subject matter; and (d) continue to insist that behaviorally oriented individuals are not only psychologists, but are, in fact, the best ones. And, of course, *try not to lose your job as a result of doing these sorts of things,* especially when operating within units formally organized under the psychology banner. (That is realistic advice; to people's use of these old strategies, psychologists often respond with actions threatening their jobs. For example, see "Court Battle," 1991; Vargas, *et al.,* 1988; and Giles, 1984, discussed in an earlier chapter.)

However, the concerted 60–year effort to pursue those well–worn change strategies has not succeeded. That may seem a short time in the historical context of the evolution of science. But the growing need for effective behavioral science was accelerating exponentially and, in the view of most behaviorologists, was becoming too urgent to allow that struggle to have its protracted play. This was especially so because the contingencies prevailing in psychology rendered demonstrations of the relative effectiveness of behaviorological *science* somewhat irrelevant. A subsequent survey of undergraduate "learning" courses in psychology departments, a traditional source of new radical behaviorist students, exemplifies the scope of the failure of those change strategies. In reporting that survey, Lattal, McFarland, and Joyce (1990) bewail the lack of coverage of even introductory "behavior analytic" material in these courses. The reasons they cite for this situation include: (a) few radical behaviorist faculty are available in psychology departments, (b) those that are available seldom have control of learning courses, (c) few radical behaviorist textbooks and laboratory manuals are available, and (d) those departments require what they call a greater "breadth" and "diversity of viewpoints" in these courses than behavioral psychologists see as justified. Lattal *et al.* reiterated the behavioral psychologist's standard rebuttal to this requirement for "diversity":

> Departments of Biology do not present evolution and creationism as alternative accounts of life nor do Physics Departments present relativity and astrology as alternatives for understanding cosmology. (p. 128)

The place reserved for behaviorists in psychology. Lattal *et al.* (1990), as is characteristic of behavioral psychologists, did not address certain relevant questions: Why should a majority that practices one discipline relinquish control over its learning theory courses to a group that entertains an entirely different discipline? Why

should that majority even allow the views of the other group to consume space in a learning foundations course meant to serve *its* discipline? After all, one could argue that behavioral "psychologists" do not represent a legitimate minority within psychology because their discipline is *not* psychology. Mainline psychologists, in squeezing out behavioral science and philosophy in practical if not yet official ways, tacitly respect that argument.

An example more explicitly addressing respect for separate disciplines arose in 1988 in the educational psychology department at West Virginia University where behaviorologists Lawrence Fraley, Ernest Vargas, and Julie Vargas had been employed for more than 20 years. The traditional educational psychologists, who were in the majority, suddenly had a brief opportunity to design, unilaterally, the department's new doctoral program, an unusual circumstance arising in the college out of some arcane internal politics. Their program design excluded from its requirements *any* course taught either by Fraley, Vargas, or Vargas on the grounds that the behaviorological science taught by those three people (under "behavior analysis" and other non–behaviorology course titles) did *not* represent psychology nor merit a place in the core of an educational *psychology* program. The three behaviorologists subsequently took successful steps at the university level to compel a reversal of their psychology colleagues' snubbing action directed at their discipline. They did this in order to secure professional operating room. But they nevertheless agreed with the psychologists that those psychologists should keep their discipline to themselves. The three took the position that such problems of disciplinary incompatibility require organizational solutions at the college and university administrative levels. The problem is that, with very few exceptions, American institutions of higher education, like those world–wide, have no academic departments devoted exclusively to the natural science of behavior, especially human behavior. (For further discussion of graduate level training in radical behaviorism being crowded out by required diversity in psychology programs, see Michael, 1980.)

Following the 1987 ABA convention, one behavior–analytic psychologist, Peter Harzem, who had been chairing a partly behavioral psychology department at Auburn University, joined the debate by publishing an article on the virtues of being a psychologist (Harzem, 1987). He offered a restatement of Skinner's earlier but still relevant thesis that, because modern technology has multiplied the dire implications of human errors in judgment, a commensurate behavioral technology must be developed to regain control in the culture (Skinner, 1971, Ch. 1). Harzem mapped out a cultural mission for such a science similar to that previously specified by Fraley (1987). But instead of endowing a separate discipline with that mission as Fraley had done, Harzem again argued, notwithstanding half a dozen decades of accumulated evidence to the contrary, that it could and should all be accomplished by the minority behavioral community based within organized psychology. Harzem also appeared to write as if the discipline that he described, and the mission that he delineated for it, represented psychology *per se.* This was an implication that behaviorologists (as well as most psychologists) see as a distortion of reality because a vast majority of psychologists (the 95% or more who are non–behavioral) do *not* and probably *cannot* accept such a behavior–analytic definition of that discipline.

In the fall 1988 issue of *The Behavior Analyst,* two spokespersons for the supposedly eclectic (but generally cognitive/mentalistic) psychology mainstream (Proctor & Weeks, 1988) replied to Harzem in a "wrist slapping" rebuttal critical of what they construed to be Harzem's myopic scientific elitism. Proctor and Weeks touted the "cognitive revolution" in psychology. They reviewed some of its putative research accomplishments. They noted what they considered to be its positive impact on a number of different fields. And they presumably refuted Harzem's premise that psychology was failing in its cultural mission—a mission to which Proctor and Weeks allowed that behavioral psychologists could contribute, but which certainly did *not* necessitate an overhaul of psychology along more behavioral lines as Harzem had advocated. They characterized Harzem as uninformed about the significant movements in his own discipline (psychology) and arrogant in his implications about the superior effectiveness of behavior analysis.

Proctor and Weeks pointedly directed three items of advice (1988, pp. 138–139) to Harzem and his fellow behavior–analytic psychologists: (a) "Behavior analysts should cease denying the reality of the cognitive revolution and familiarize themselves with the major advances that have occurred in psychological research…." (b) Behavior analysts should quit talking to themselves in "in–house" journals about things of which they are already quite convinced and instead should publish in mainstream psychology journals. (Ironically the refusal of those journals, since the 1950s, to accept behavioral articles had contributed significantly to the initial movement of many behavioral people from mainstream psychology in the 1970s. Also, in an article rebutting Proctor and Weeks, Lee [1989] appealed to the principle that journal specialization follows, and merely reflects, the increasingly discriminative characteristics of a maturing science.) And (c) "Behavior analysts should join the rest of the psychological community in the ongoing pursuit of scientific knowledge, without having the predetermined agenda of 'supplanting cognitive explanations'."

In short, Proctor and Weeks added their endorsements to the similar demands of other representatives of

organized psychology. They laid out, once again, the ground rules by which behavior analysts are expected to conduct themselves if they are to be citizens of the psychology verbal community. Their message and its tone exemplified the social punishment that mainstream psychologists inflict on behavioral psychologists who resist the norms imposed by the overwhelming cognitive/mentalistic majority.

Invalid attacks on radical behaviorism. A year after the Proctor and Weeks article, and along the same lines, Michael Mahoney published, in *The American Psychologist*, a more concerted attack on radical behaviorism in psychology (Mahoney, 1989). Like many semi–behavioral psychologists, Mahoney typically misrepresented radical behaviorism. Essentially he implied wrongly that the mediating internal processes with which cognitive, humanistic, and psychotherapeutic psychologists preoccupy themselves fall beyond any adequate conceptual and analytical reach of the functionalist umbrella of radical behaviorism. He implied that those processes require a more comprehensively founded "scientific psychology." Behaviorologists disagree. The sciences of both verbal behavior and emotional behavior (including related respondent behavioral components)—each informed by the philosophy of radical behaviorism and now being organized outside of psychology within the discipline of behaviorology—deal effectively with any real and relevant evidence adduced by the cognitive, humanistic, and psychotherapeutic schools of psychology.

In the first place, behaviorologists recognize that all *real* events in those domains of concern are variables in functional relations. On the other hand, ontological criteria deny existence to some of what those psychologists seek to understand (a classic example is subconscious mental activity invented to explain behavior). Next, with all of those mental and emotional constructions purged of their residual mysticism and recognized as *behavior,* cognition becomes verbal behavior, and the humanistic impetus becomes emotional behavior. And the behaviorologists *have* powerful sciences of *those* phenomena. As behaviorologists then develop effective behavioral technologies involving events in those domains of concern, they see the many substantial preoccupations of psychologists as either basically irrelevant or unimportant to those domains. So, in those contexts, behaviorologists neglect those preoccupations. That neglect is not because behaviorology cannot deal with such things, but because it *can* deal with them—and has sorted them into appropriate places in the grand scheme of things.

However, Mahoney's article, if weak as a comparative review of paradigms, was much more important as propaganda. His perspective did not reach beyond psychology, and he wrote as if everything "behavioral" was implicitly under the psychology umbrella. Appealing to his own interpretations of history and citing various authoritative sources, Mahoney (1989) developed positions to these effects: (a) Radical behaviorists in psychology, in their extremist zeal to disavow personal agency, have clung to anachronistic science. That group "isolated itself from and came to lag behind changing perspectives on the nature and practice of optimal scientific inquiry. …radical behaviorists… in turning their backs on the lessons of philosophy, have found themselves unnecessarily tethered by their own first assumptions" (p. 1373). (b) Radical behaviorists are self–righteous and intolerant, and have thus become "scientistic" followers of an unworthy approach to science, which renders them poor citizens of the psychological scientific community. (c) "Respected scientists" have had to revise radical behaviorist accounts of learning to take into account new complexities in cognition. (d) Only those who abandon the extremism of radical behaviorism can share in the disciplinary progress of psychology so saliently manifested by the "cognitive revolution." Worthwhile behavioral contributions to psychology have come from methodological and "metaphysical" behaviorism, and continue to come from methodological or "liberal" behaviorists who incorporate, or take into account, the explicitly "mediational" concepts of behavior from cognitivist theory. Few important psychologists any longer care about radical behaviorist views. And (e) today relatively few genuine radical behaviorists can even be found. The integrity of radical behaviorism [in psychology] is weakening in the face of dramatic revisions compelled by the views on other fronts elsewhere in paradigmatically balanced psychology. So speaks Mahoney.

Mahoney's article, appearing in the prestigious and widely read *The American Psychologist,* jarred the behavioral psychology community. In implying to behavioral camp followers that their fortunes would be more promising elsewhere, Mahoney's article perhaps influenced disaffiliations among persons who were insecure in their commitments to radical behaviorism and to the strict natural science of behavior that it informs. Mahoney implied that remaining a radical behaviorist within psychology is (a) surely a matter of embarrassment and (b) certainly not the path to any viable professional future in that disciplinary coalition.

Some behaviorologists found themselves in agreement with *both* of Mahoney's implications, though not for his reasons. The article obviously stung many behavioral psychologists who set about organizing rebuttals (e.g., Catania, 1991). Perhaps as well as anything that behaviorologists have said, articles like those of Proctor and Weeks (1988) and of Mahoney (1989) indirectly suggest the impracticality of behavioral people trying either to maintain their scientific integrity within organized psychology or to change psychology into an effective science for

the kind of cultural mission that most behaviorologists deemed important.

Some organizational and intellectual realities. The behaviorologists were confronting an organized psychology discipline populated by intelligent, articulate individuals. These persons had a lifetime of credible training and experience implying to them that their discipline was valid and effective. Furthermore, their ethical training produced a strong belief in the kind of scientific and philosophical eclecticism necessary to construe psychology as an integral discipline. Mainstream psychologists, of whom Mahoney and Proctor and Weeks were typical examples, accepted little of behavior analysis (especially in its radical behavioristic expression) and even less of behaviorology. They revealed little comprehension of the complexity and sophistication of what they called behavior analysis or of its fundamental philosophical, scientific, and technological differences with the discipline that they knew as psychology. They did not seem to understand that behavioral people cannot merge their concepts with those of traditional psychology while, at the same time, maintaining their own behavioristic scientific probity because those propositions are incompatible. Not only was the scientific integrity of behavioral individuals eroded in the psychology community, but the behavioral people were not, and could not have been, understood there.

In addition, behavioral programs, operating merely as options, specializations, or areas of emphasis *within* departments of psychology, are more vulnerable than would be true of departments devoted exclusively to a behaviorological discipline. Harzem (1987) did not address this point. Yet Harzem's own program, like any behavioral specialization existing as but one epistemological face in a larger multifaceted program, has no guarantee of security beyond the next faculty meeting, the next change in chairperson, or the next new dean. Chairpersons easily redirect academic programs that displease them simply by reassigning the courses to politically reliable faculty members who can be expected to make desired changes. A simple faculty vote within a psychology department to de–emphasize an internal behavioral movement, or an administrative decision to shift resources to other specializations, remained easily arranged dispositions of minority movements that have no formal status beyond mere areas of interest in much "broader" psychology programs. This can happen even when the minority is correct about the greater effectiveness of its science. On the other hand, to eliminate a whole discipline, including its established academic department, from a higher education institution is more difficult. A group of faculty members, or their administrative leaders, might easily dispose of some individual or minority faction at the department level. But another order of difficulty is encountered when an institution attempts to declare that an integral discipline is no longer to be part of its curriculum.

Perhaps Harzem's position reflects contingencies affecting individuals who have achieved *personal* success as members of epistemological minorities in various disciplines. But inherent in such a strategy is the peril of attenuating revolutionary fervor that often accompanies personal success. Behaviorologists have lost faith in reliance on personal success as the mechanism by which to protect their science within the psychology milieu.

An example from theology parallels the plight of behaviorists in organized psychology (Basil, 1987/88). A number of prominent Catholic theologians, religious educators, and church officials, attending a forum for secular humanists and liberal Catholics, successively criticized basic principles, policies, and practices of organized Roman Catholicism. Observers questioned why the speakers had not left the church. As individuals, they had not only "let go of the Pope, the Bible, and church tradition" (p. 6) but had also convincingly justified their having done so. In response, Dan Maguire, a professor at Catholic University, is reported to have insisted that the "essential moral creed" of Christianity, which he respects, has never been "incarnated" in historical Christianity. But as Basil (1987/88) noted,

> It was not clear to many in Washington, however, why Maguire and some of the other speakers thought that it was worth belonging to a religious institution that had never "incarnated" its "essential moral creed." Perhaps, being born Catholic, they want to remain attached to their family's roots or to the admittedly fascinating rituals of their church—regardless of its dogma. Perhaps they hope to overturn two thousand years of authoritarian religious rule "from the inside," as it were; then they would have a ready–made and powerful institution to promote humanistic ideals on a world–wide scale. …But it should be pointed out that this strategy could backfire. By remaining within a church whose very foundations they attack, these dissident Catholics could be making traditional authoritarian Catholicism stronger. After all, one could argue that the church has been able to co–opt the voices of some of its harshest critics. No matter how free–minded these fringe Catholics are, their primary message will always be that they support the church. (p. 6)

From the perspective of the behaviorology movement, a self–analysis similar to the one for liberal Catho-

lics would seem to be in order for behavioral psychologists. They are persons who retain disciplinary allegiance to psychology, but who (a) have let go of mind, (b) regard as irrelevant the physiological mechanisms included under cognition, and (c) have turned away from many traditions of allegiance to organized psychology. Perhaps they should consider the supportive implications of even their opposition if expressed *from within,* and *as members of,* the institution that they oppose. Catania (1991), chastising Mahoney (1989) for his misrepresentations of radical behaviorism, wrote that if Mahoney's views:

> …*really* represented the position of contemporary psychology, it should come as no surprise that some radical behaviorists have moved to more congenial environments… (p. 68; emphasis added)

(Catania immediately added "but intellectual ghettoes are as objectionable as racial or ethnic ones." Behaviorologists might agree with Catania's observation about intellectual ghettoes, but not about who, implicitly, is trapped in one.)

This issue continued to maintain tensions even among the early behaviorologists who differed with one another about both the degree and the appropriate kinds of supportive cooperation in which to engage with majority psychologists. Often, to gain access to students so that they could provide behaviorological training, behaviorologists had to participate in a psychology program. That program not only would claim credit for the products of that training but also would prosper in various ways through the professional involvements of the behaviorologists. Some argued that this amounted to organized psychology exploiting behaviorologists in ways that maintained organized psychology at the expense of independence for behaviorology. Others counseled patience, and argued that the preliminary work being done under the psychology umbrella was crucial for the future of behaviorology and so should not be jeopardized by premature and costly rebellion. Without resolution, some raised rhetorical questions about indulgences in self–serving rationalization while others countered with concerns that unwise emotionalism might be getting too intense.

A public separatist review. The 60–year effort begun by Skinner to make–over psychology had failed, and conditions aversive to behavioral people were continuing in force. But as Ernest Vargas observed at the conclusion of Bruce's 1987 ABA symposium, those same contingencies had previously forced the issue of separation and independence on radical behaviorists, and their experiments with separation on those occasions had worked well. He added that the only way to determine whether separation will continue to work was to continue to use it.

At the symposium Vargas also briefly reviewed the historical highlights of those earlier occasions for separatist activity: Behaviorists had experienced difficulties getting their articles accepted in the traditional psychology journals which usually required the group designs and the related statistical significance tests typical of mainline psychology. (To understand the kinds of studies that the behavioral people were doing, see the approaches to research explained in Sidman, 1960.) So radical behaviorists successfully founded separate journals independent of the psychology establishment, including the *Journal of the Experimental Analysis of Behavior* (JEAB) and the *Journal of Applied Behavior Analysis* (JABA). When the professional interests of behaviorists were no longer adequately met by any of the existing divisions of the American Psychological Association (APA), behaviorists began a separatist movement within APA by founding a new division of their own, Division 25, titled "The Experimental Analysis of Behavior." When later the APA and its regional organizations were not adequately meeting their convention and other needs, even with their own division, behavior analysts gradually pursued a larger scale separatist course by eventually founding ABA.

Finally, when, among other things, the lack of training and application facilities (which normally come as part of a disciplinary home) threatened the contributions and even the continuation of their scientific verbal community, and weakened the quality controls needed to preserve it, behaviorologists acted. They moved to complete and refine the definition of an effective and natural science of behavior, and to organize an independent verbal community around it.

A comparison with the Cultural Materialism movement. A comparison between this separatist trend from psychology and the course pursued by cultural materialists and others within anthropology (Harris, 1979) may be instructive. Cultural materialists, like behaviorists, have entertained an unusual philosophical and analytical perspective within their "parent" discipline—one sharing some basic principles and assumptions with behaviorology (see E.A. Vargas, 1985). However, the coalescence of their discriminable integrity within anthropology does not appear to have precipitated the degree of conflict that has surrounded the behaviorists in psychology, nor have the cultural materialists sponsored an emergent independence movement. Cultural materialists as a group have not professed difficulties in publishing in the traditional anthropology journals nor of being denied adequate convention participation by the professional organizations of anthropology. The cultural materialists have been able to remain within anthropology and have continued working to revise and improve that discipline. Perhaps this is because more traditional anthropologists have not widely construed them to be operating with a different basic science or philosophy, but only to have reached some unusual insights and conclusions. This approximates the

relation between the behavioral and mainline psychology communities in the 1940s and 1950s.

The discipline of behaviorology is heavily focused on controlling behavior. Its members base its rights claims to intellectual property on the effectiveness of products manifested at the level of control. In comparison, the science based on cultural materialism has not yet fully advanced to the level of control with respect to *its* subject matter. It remains largely interpretative and analytical, having passed through descriptive stages and having attained the capacity for accurate predicting and some control. As yet, however, cultural materialism does not exhibit effective technologies, based on its own principles, for designing and developing new cultures, or even new cultural practices. (See Fraley, 1988c, for a more analytically detailed examination of this point; see Beach, 1988, for hints of progress.)

Perhaps cultural materialism has not yet demonstrated control because the culture has not yet imposed much of a necessity for doing so within the scope of applied concerns entertained by anthropologists. The predictions of cultural materialists might be more accurate, and their analyses more parsimonious, than those of more traditional anthropologists. However, the implications of any such contrasts have not been sufficiently adverse to provoke the traditional majority to opt for political and economic countercontrols within the discipline. Not enough is yet at stake. But what if a major lucrative market were to develop for such skills? What if the cultural materialists within anthropology no longer merely described and retrospectively predicted the courses of old cultures? What if they were to start *producing* superior cultures and cultural practices by design? Then mainline anthropologists (just as mainline psychologists) could not be expected to acquiesce calmly to their own eclipse.

One early attempt at anthropological control involved the Vicos Hacienda project in the 1950s (see Holmberg, 1967). In that project anthropologists actually did design and implement new cultural practices. They assisted the residents of the hacienda, who had been vassals of the land, to funnel the fruits of their labors more efficiently back into their society. The project was deemed controversial within the anthropology community. Colleagues criticized project leaders for using their scientific powers to impose their own cultural values on the hacienda community.

Such rifts might presage similar rifts from efforts in the significant area of designing the cultural aspects of off–Earth settlements. Space settlements will require cultural practices respecting new kinds of values that will also have to be conditioned by design—in some cases values not shared by the designers themselves. The new practices would enable people to thrive under the alien physical and socio–cultural conditions in space, on moons, or on planets (see Beach, 1991a, 1991b; BACD, 1991).

In the 1960s, at the height of the debate about Vicos Hacienda, the anthropology community was unable to deal effectively with the issue of the putative sanctity of values. Since then, developments in behaviorological science have placed the management of values in a technology based on natural science. That is because values are behavioral effects of the contingencies under which people operate. Specifically, values are reinforcers, and most are selectively conditioned by the consequences of behavior and functionally facilitated by the concomitant emotional respondents called *feelings.* And behaviorological science and technology have worked these areas well. (For more detail, see Skinner, 1953, 1971, 1974; Krapfl & Vargas, 1977; and E.A. Vargas, 1975, 1982.)

In general, the greatest cultural needs are felt at the scientifically mature level of effective control. Throughout the culture the greatest value is attached to effective outcomes at that level. Thus more professional reinforcers are at stake among scientific groups competing for the professional markets defined in terms of effective *control* over behavior. For example, in just one area, consider the enormity of the resources that our culture is prepared to invest in whichever scientific discipline can develop the most effective technology of teaching (Skinner, 1968; also, see Barrett, 1991). Behaviorological scientists have repeatedly demonstrated what they construe to have been convincing wins in such implicit contests (e.g., see Johnson & Layng, 1992). No surprise occurs, then, when educational psychology is the locus of much interdisciplinary conflict centered around what some educational behaviorologists regard as attempts to suppress their more successful science. (For some details and examples on the educational issues, including the U.S. Federal government's massive "Project Follow Through," see *all* the articles in the Youth Policy Institute's July/August 1988 issue of *Youth Policy.*)

An original separatist move. Small scale separatist moves by behaviorists within psychology had actually become necessary on much earlier occasions than those Vargas included in his summary at the 1987 ABA convention symposium. For instance, Skinner's early psychology course at Harvard, Psych. 7, was too divergent for the conventional psychology faculty. Under the new name "Natural Science 114: Human Behavior," the course moved to the general education area where it proved successful with a wide range of students. These students tended to be focused on the realities and practicalities of the culture, and faculty resistance subsided. Skinner (1983a) reported:

> …it was a much more appropriate title. I had not given my students a general survey of psychology; *I had taught a very different subject.* The new title also at-

> tracted students whose major interests were in the social sciences or humanities but who needed a sophomore–level Natural Science course, and they were just the students who needed to know more about a science of behavior. (pp. 23–24; emphasis added)

Skinner (1983a, p. 44) subsequently converted that course material into his 1953 book *Science and Human Behavior.* That book features principles induced from an experimental analysis of behavior, and develops the conceptual foundations of a discipline strikingly different from that introduced in typical beginning psychology courses.

Skinner's Oscillations

For a number of years behaviorologists had been feeling an increasing disappointment at the progressive deceleration of the general ABA–centered behavioral movement toward disciplinary independence for the science of behavior. In 1987–1988, as those observers finally acted to establish an independent behaviorology discipline, they were sensing a rapidly accelerating reversal of the general behavioral movement back into the fold of organized psychology (a trend which their actions may have retarded).

Back to psychology. An incongruous event that occurred at the end of the 1987 ABA convention perhaps in part propelled this reversal back toward psychology. B.F. Skinner unexpectedly added a ten–minute addendum to his previously prepared major address—one in a series of annual presentations serving as the traditional final session of the annual ABA conventions. Skinner had spent his entire professional life building the case for a behavioral science revolution and explicitly providing the leadership for it through his research and writings. In contrast with this, Skinner told his audience that he thought a separate discipline was a mistake, that behavior analysis was a kind of psychology (and should *become* psychology), and that organized psychology was where the behavioral movement should remain. He had wanted a revolution, but now he was declaring that it was to occur *within* psychology, not as a departure from psychology.

Although Skinner subsequently oscillated on this issue (two years later he announced his reversal of this view, discussed in a later section), at this moment he seemed to declare unequivocal favor with one path away from a junction at which he had been poised throughout a lifetime of strategy deliberations. He had spent his professional life arguing alternately that psychology should be changed and that it should be abandoned. His lifelong bias was for changing it. He had explicitly taken that stand as early as 1928 when he wrote "…my fundamental interests lie in the field of Psychology, and I shall probably continue therein, even, if necessary, by making over the entire field to suit myself" (Skinner, 1979, p. 38).

Speculatively, a variety of potential sources may have raised the special prepotency of that recurring preference just at the time of the 1987 ABA talk. These sources include: (a) convention sessions (such as the previously described Bruce symposium that involved Skinner) which had focused on the issue of a separate discipline and sharpened many relevant variables; (b) a resolution to redouble a thus–far unsuccessful effort, perhaps triggered by misgivings about the appropriateness of that cause (a public declaration of recommitment postpones any impending verdict of failure and, as a self–management practice, renders oneself liable for the social punishments reserved for those who do not keep to their commitments); and (c) the pressure exerted by respected psychology colleagues whose counsel favored staying within psychology.

Regardless of possible reasons for his declaration, Skinner's remarks contributed to the already advanced erosion of the capacity of ABA to spawn an independent scientific discipline. Ironically, he had chosen to make these statements at a meeting of an organization which was originally established to demonstrate a measure of independence from psychology for the behavioral movement, and which he had for years fervently supported. Although ABA had attained its *organizational* independence from psychology, *disciplinary* independence had remained elusive. The concept of an independent behavioral discipline had always promised security for the separate existence of ABA. But Skinner's saying that the struggle can only proceed from *within* psychology implied the extent to which the behavior analysis independence movement had failed. As he urged the behavior analysts to keep trying to change psychology into a worthwhile science, part of the audience believed that Skinner was appealing on behalf of a cause already lost. In any case, Skinner's surprising and seemingly self–contradicting declaration elicited strong visceral responses from just about everybody. Depending on the histories of the affected individuals, the personal reactions represented a wide range of emotions.

As he made clear in his autobiography (1979 & 1983a), Skinner had neglected the politics of organized disciplines in favor of successful scientific pursuits. But his voice had grown influential. His recommendations about organizing the discipline, always heavy with political implications because so many people attended closely to whatever he said, constituted a weighty political resource for those whose objectives were served by whatever position he took.

Skinner was adding his powerful endorsement to the concept that "behavior analysis" *is* psychology. Most behaviorologists had already taken that position as part of their effort to discriminate clearly between behaviorology and behavioral psychology which had staked a strong claim to "behavior analysis." The APA, for its part, had

implicitly accepted that collaborationist perspective when its behavior–analytic Division 25 in 1987 briefly changed the name of the division journal to *Behavior Analysis.* Prominent behavioral psychologists with memberships in ABA were commonly referring to behavior analysis as a kind of psychology and to behavior analysts as psychologists (e.g., see "Minutes of," 1988, 1990). Edmond Fantino (1988), as editor of JEAB, explicitly described behavior analysis as an aspect of psychology. And numerous behavior analysis training programs were imbedded in psychology departments.

By the time Skinner gave his talk, most behaviorologists correspondingly saw little hope for an independent science of behavior analysis. (In his speech, Skinner did not pursue *that* implication of his position.) The trend toward a psychology–free credential by the clinical faction in ABA (Shook, 1993), with its implications for an independent behavior analysis discipline, had not yet had a significant impact on the general thinking of behavioral people. In any case, Skinner's speech encouraged psychology–oriented followers to distance themselves even farther from what some had seen, partly through his leadership, as the goal of a separate scientific discipline. Skinner's 1987 disavowal of that goal allowed any followers who might have been feeling the political heat of an independence–oriented rebellion to resume overtures to psychology that previously might have been deemed unfashionable. Some behaviorologists welcomed that self–culling insofar as it reoriented toward psychology a lot of people whose ambivalent faltering might have clogged the machinery of the behaviorology movement.

An affiliation with ABA no longer connoted one's disciplinary identity because the phrase "behavior analysis" had become so imprecise. Increasingly, ABA members felt compelled to declare their respective professional and disciplinary identities, some with and some away from psychology. And within two years, by 1989, the ABA leadership was explicitly evoking such declarations from its members by way of official questionnaires and new fill–in items on the ABA membership forms (as described in Chapter 4).

The Ulman–Skinner letters. An accidental fall at his home put Skinner in the hospital and prevented him from attending the 1988 ABA convention, though he returned in 1989. During the two years between the 1987 and 1989 conventions, a number of people interacted with Skinner on how best to preserve the science that he had developed and about the position that he was taking with respect to an organized disciplinary home for it. Skinner's reluctance to leave psychology was based on several practical considerations: Aside from his expressing a personal distaste for the name behaviorology, he was making the following points (recorded here as noted in his 23 January 1989 letter of response to Jerome Ulman's endorsement of a separate discipline): (a) Behavior analysis is already spread too thinly across too many organizations, so a new one, further fragmenting the movement, is not needed. (b) Colleges and universities are unlikely to create departments of behaviorology for some time. (c) Behaviorology would have little chance of gaining recognition from the National Science Foundation (NSF), at least for a long time. And (d) psychology can yet be redefined, and one useful approach to that end is to continue with good experimental scientific work that will yield ever more convincing scientific data in support of the behavioral perspective.

The dilemma confronting Skinner and other behaviorists who had long struggled to change psychology was underscored by an anecdote that Julie Vargas (Skinner's daughter) related in a letter (20 October 1988) to Fred Keller:

> Last week I was up in Cambridge for two days and a TV crew came to discuss shooting… for a 26 half–hour series on psychology. It was to be an introductory course, like a beginning textbook, they explained. They wanted my father in three of the series, a brief appearance in two and a major part of the one on learning. Brief means a couple of minutes and "major" means 10 minutes—maybe 15 or even 20…. Out of 26 half–hours, or 780 minutes, about 20 minutes or roughly 3% was devoted to the science of behavior.

A little farther along she added "I can't convince my father that being a little piece of 'psychology' does not bode well for our science." The letter ended with "I just wanted to let you know that TIBA is progressing well and to show you our brochure."

During the 1988 Christmas holidays, in discussing with Julie Vargas how best to preserve the science that he had developed, Skinner allowed that he might tolerate the term behaviorology. By January 1989, he was referring in various conversational contexts to "the behaviorologists" and their movement. In a letter to Ulman (6 March 1989), Skinner conceded that the name behaviorology is not as bad as "otorhinolaryngology." Ulman had said that initially he too had found the name behaviorology a bit strange and offered a remedy: "In the privacy of your bath, try saying *behaviorology* aloud at least 50 times." To this Skinner, in his 6 March 1989 letter, replied "I am grateful for the enthusiasm and loyalty of the behaviorologists. I'll try saying the word to myself in the bathtub and see how good a predictor you are." Still, Skinner had not yet completely abandoned his presumption of organized psychology as keeper of his science and philosophy. By April 1989, the American Psychological Society (APS) was pub-

lishing the solicited letter from Skinner (presented in an earlier chapter) lending endorsement to that movement.

The following passages from one of Ulman's letters to Skinner (24 February 1989) represent the kinds of views being advanced to Skinner by persons committed to disciplinary independence (subsequent to their inclusion here, the Ulman–Skinner letters were published in *Behaviorology;* see Ulman, 1993). After conceding that neither universities nor the NSF are likely to recognize behaviorology anytime soon, Ulman addressed many issues beginning with what he thought was Skinner's misplaced confidence in the new APS:

> In my view, APS is merely a protestant form of the same old religion, a splinter group from the Assembly of the Personal Agency, consummate defenders of "autonomous man." What motivates the overwhelming majority of APS, I suspect, is opposition to the exclusive guild concerns of the clinical majority in APA, not the promotion of a science of behavior as you and I understand it.
>
> Given the character of its membership, the "science" we can expect APS to advance is exactly the kind you have argued against throughout your career—hypothetical–deductive methodology, statistical inference rather than experimental analysis, the philosophy of logical positivism, and the type of theorizing that, as you so aptly stated in 1950, "appeals to events taking place somewhere else, …and measured, if at all, in different dimensions (e.g., in the real nervous system, the conceptual system, or the mind)." …Any behavioral psychologists in their [APS] ranks (if the behavioral constituency approaches even 10%, I would be greatly surprised) simply serve to give a behavioristic veneer to the ersatz science of psychology.
>
> In general, our participation in any *psychological* organization presupposes at least tacit acceptance of a common program. Because psychologists cannot agree on a radical behavioral program without themselves becoming something other than psychologists, a psychological organization must of necessity abandon radical behaviorism in favor of theorizing based on mind, cognition, and/or the conceptual nervous system. In short, the program of the psychologist is the defense of the psyche; the program of the radical behaviorist, its demise.
>
> What must at all cost be defended and nurtured are those cultural contingencies—as rare as they are exquisite—responsible for the development of the natural science of behavior. By comparison, it matters little to behaviorologists whether we will soon see behaviorology departments sprouting up around the country or receive a nod of approval from the NSF. That is, the integrity of the discipline takes precedence over the number of people who may or may not come to feel comfortable about recognizing behaviorology as an independent life science. What does matter, I am sure, is that our scientific work be carried out within that protective social environment we call the discipline of behaviorology.
>
> …I am afraid that this travail [the effort to reform psychology] is Sisyphean and will only wear us down and ultimately exhaust our extremely limited resources. We simply cannot afford to continue our scientific work as a specialty within the psychology establishment. To remain there any longer portends to be a recipe, not only for our continued servitude, but for eventual extinction of the very contingencies necessary for the survival of our science and the philosophy upon which it is based. Yes, as you advise, more active experimental work is needed, there are many issues that need to be explored, and there is much to discuss about the implications of our work. But psychologists since the time of Watson have not merely ignored us, they have contemptuously rejected our experimental research. And there is no apparent reason to suppose they will behave any differently in the future.
>
> …The founders of The International Behaviorology Association, …recognize selection by consequences as the most important causal mode in the life sciences and are committed to pursue the use of the contributions to behaviorology found in the works of B.F. Skinner for integrating the concepts, data, and technologies of the discipline's various subfields. With rigorous membership qualifications for its leadership, a newsletter already in pro-

> duction…, a journal under development, and TIBA chapters forming internationally, we are prepared to stay the course.

Skinner's reply (6 March 1989), a few lines beyond one page, was cordial and appreciative. But, after saying that he did not take the APS very seriously, Skinner, though without his usual literary vigor, let stand his previous position that *psychology* represents the study of behavior—including the behavior of the brain, not as the mysterious generative source of behavior, but as part of the thing called the behaving body. He complained that the cognitive psychologists have been doing inappropriate kinds of things *in the name of psychology.* And he lamented the fact that others, who also understand what is going wrong, too often let them get away with it. He briefly reviewed the list of his own rebuttals to the cognitive views, the views that he thought had led psychology so far afield (from a behavior science). Skinner made the point that he had no objection to legitimate interests by psychologists in how the body worked, but that psychology needed to switch its emphasis to the kind of science that makes possible practical behavior–technology applications.

Behaviorologists continued to raise the issue with Skinner in various ways. For example, taking advantage of inescapable realities, Julie Vargas from time to time reportedly brought to Skinner's attention factual data implying futility in continuing a disciplinary affiliation with psychology. On 28 April 1989, Ulman sent one more eloquent letter to Skinner, reiterating the reasons for disciplinary separation. In this letter he argued the points with new examples and facts. He countered the notion that staying with psychology protected previous gains and represented the shortest path, however bleak, to a secure home for our discipline. Ulman concluded in this way:

> …As long as we remain within the church, for whatever reason, the longer we must abide by its commandments, chief among which is an unquestioning respect for the divinity of the psyche (or any of its cognate hypostases—mind, cognition, experience, etc.). I feel that we can best move forward, not by arguing with psychologists about what may or may not be going on in the black box; writing letters to hostile, psyche–intoxicated editors; nor playing no–win political games in psychology departments. We have done all of that for over 50 years and it has gotten us nowhere. I say, psychologists made their own metaphysical bed, now let them lie in it. We have much more useful things to do; namely, to advance the independent discipline of behaviorology in all of its facets—philosophical, analytical, experimental, and technological—and, most importantly, disseminate our successes to the public.

Regardless of whatever fueled the great engine of organized psychology, it did not require a more effective science. The nature and course of organized psychology had remained little affected by the protracted proffer of a more effective basic science by its politically insignificant behavioristic minority. The radical behaviorists' long–nourished anticipations of an overhauled psychology had matured fruitlessly. No promising new change strategies arose to mask the increasingly hollow echo of old ones being reiterated. With few exceptions the leadership in organized psychology, whether in APA or APS, remained unalterably cognitive and mentalistic, and the discipline of psychology was organized and operated to reward members for following that lead.

A declaration of independence. By the time of the 1989 ABA convention in late May, Skinner had again had enough of psychology. In spite of his wavering in 1987, his estrangement had developed across a long if intermittent history. He had made it explicit as early as 1938 in *The Behavior of Organisms.* In 1972, when a colleague had asked if operant people should form a separate discipline, Skinner (1980, pp. 71–72) had noted, "I have been coming close to that view." He went on to list four major fronts of disciplinary activity that he thought had taken psychology astray.

Then, as his major address at the close of the 1989 ABA convention, Skinner presented his declaration of disciplinary independence, titled "A World of Our Own" (Skinner, 1989a). Reviewing the ancient origins of cognitivism and the more recent origins of behaviorism, he described how cognitivists increasingly have hidden their ancient and invalid assumptions by adopting descriptive language from other respected fields. He reviewed and analyzed samples of what he construed to be the current cognitive/mentalistic nonsense that pervades the literature of psychology. He defined a proper behavioral discipline to include the concerns of ethologists and dwelt on the importance to such a discipline of the mechanism of selection at all levels of analysis. He spoke of the neglect in cognitive psychology both of the concept and implications of selection. On the cognitive/behavioral dichotomy in the search for the origins of behavioral events, Skinner had this to say (quoted from a tape recording of his talk):

> Let's just imagine that all of these people who are trying to find out what is inside the skull, whether it is mind or brains (it doesn't matter), …[are] all completely successful. Let's suppose that people who introspect see the same thing…. And let's suppose that cognitive

> theories have all been meshed well together… [into a] super– or mega–theory. Everybody agrees! This is it! And let's assume that brain science has come up, not only with what happens when you slice a brain in two, or when you inject a drug into it, but [with descriptions of its nature] down to the finest detail, so that we know what is happening when someone says "hello." And let's suppose that is exactly what you see when you introspect and is exactly what cognitive theorists say is in there. What would you have? You would have a biochemical system. It would be completely orderly. It would simply do what it does because of its structure.

Here Skinner implicitly dismissed the notion of a personal autonomous agent and alluded to the controlling environment in which occur the events that subsequently stimulate (that is, functionally control) the internal neural activity. When the body behaves, that exhibited behavior is simply the only one possible in the interaction between that body as currently structured and the controlling environment as currently structured. (For elaboration, see Fraley, 1992a, 1996a.) These kinds of behavior–producing interactions feature the accessible independent variables over which the kind of control called intervention can be gained; they provide the basis for behavioral engineering. Skinner asserted that behaviorists should remain interested in behavior–related neural events within the body. But he further argued that the *behavioral* people are the best prepared to specify that for which the neural scientists could productively look.

After presenting some historical details about *his own earlier acceptance of principles portending a separate discipline,* Skinner noted that he nevertheless:

> …went on saying that the science of behavior was psychology. And [he added] I am convinced now that I was wrong. I think they are very different fields.

Perhaps alluding in part to his brief addendum after his 1987 ABA address, he confessed to having "been slow in throwing off the notion that a science of behavior is the future of psychology."

Here are some related conclusions heard elsewhere in his talk:

> …We've got an exclusive field here. No one is anywhere near us.
>
> …I believe that this field is an extraordinarily important one and has no rivals.
>
> …It seems to me perfectly evident that those of us who are thinking well in the terms of behavior analysis are miles ahead of those people.
>
> …Now I think this *is* a world of our own.

After Skinner's earlier call, made during his 1987 ABA talk, for followers of his science to stay in psychology, he did not attempt to publish a paper based on that speech (as had been his usual practice). But a couple of weeks after his 1989 "independence" talk, Skinner accepted an offer by Lawrence Fraley, tendered as editor *pro tem* of the incipient behaviorology journal, to prepare and provide a paper copy of the 1989 "independence" speech transcribed from a tape recording. Skinner would edit it and prepare a version for publication in the new TIBA journal (to be named *Behaviorology*). Talking later to a family member during the Christmas season of 1989, Skinner again noted that, while he had been slow to turn away from psychology, he thought that our discipline should have been established independently a long time ago. And he mentioned the paper on that theme which he was preparing for *Behaviorology.* He subsequently submitted the paper to *Behaviorology,* and it was included in the first issue (Skinner, 1993).

In August 1990, during a speech at the APA convention only a week before his death, Skinner reiterated the evaluation of psychology to which his science inevitably leads. Holland (1990) reported the scene:

> To the ballroom full of psychologists he said that, with the selection of consequences as the cause of our body (natural selection) and our behavior (environmental contingencies), there is no role left for a creator or for an initiating self or mind. So cognitive science "is the creationism of psychology." (p. 32)

Yet in the end Skinner would leave ambiguous his endorsement of a future course for the discipline built around the science he originated. On 17 August 1990, the day before his death, Skinner finished editing a final paper (based largely on that talk to the APA) entitled "Can Psychology be a Science of Mind" (Skinner, 1990). In it he reviewed the polyglot disciplinary topography of psychology. Then he explained one last time why psychology would have to undergo substantial paradigmatic change to fulfill its putative cultural mission. Referring to his selectionist–based science as "behavior analysis," the closing sentence of that final article brought to an end, without resolution, his life–long struggle to change psychology: "…but whether behavior analysis will be called psychology is a matter for the future to decide" (p. 1210).

But the decision had *already* been made—by the behaviorologists: The science Skinner originated would be called neither behavior analysis [as presently known—Ed.] nor psychology. It would be called behaviorology. And the behaviorologists had reached two additional conclusions: (a) Not only does psychology not represent Skinner's science but, more importantly, it is unlikely *ever*

to do so; and (b) Skinner's reasons for entering into organized psychology had had nothing to do with any appeal of the psychology paradigm, and *he had never come to behave philosophically or scientifically as a psychologist.*

The Issue of Covert Reform

Many others were proving even less able than Skinner to overcome the powerful contingencies that bound them to the professional situations in which they had struggled for most of their careers, in some cases with personal success. Apart from differences of opinion about the best approach to bring an effective science of behavior to our culture, the fact remained that psychology was professionally well organized and well established. Its group–arranged contingencies afforded many professional advantages plus a substantial protective umbrella for the professional and economic interests of the people in its different philosophical/scientific verbal subcommunities. Any radical behaviorist could stay within organized psychology, enjoying those benefits, while the perennial behavioral movement to change psychology from within afforded that person the imprimatur of an organized cause for being there. Outside observers could not effectively sort out the relative effects of the various kinds of contingencies sharing in the control of decisions to remain within, and to support, psychology.

During October 1989, Sam Deitz, Editor Elect of *The Behavior Analyst,* was speaking on educational reform at two successive regional ABA Chapter conferences. He told those audiences that behavior analysts should stop trying to attach credit for their accomplishments to their theoretical foundations and simply work to get their *practices* accepted as the normal way of doing things. After all, Deitz claimed, no educational practice is derived uniquely from only *one* theoretical position. As evidence for that conclusion, he noted that in practical situations cognitive and behavioral people make *similar* suggestions. Through that line of reasoning he arrived at this point: Offensive theory need not be touted at the expense of cooperative progress.

Certainly, the prevailing contingencies in many simple situations compel similar practical reactions from persons of differing theoretical persuasions. But that is because their differing scientific and philosophical repertoires are not evoked in such simple situations. Most people will respond cooperatively to a breach in a dike by throwing more sandbags on the leak regardless of differences in their respective scientific and philosophical repertoires or, for that matter, their religious, political, economic, or social ones as well. For that level of responding, people need neither philosophical nor scientific repertoires. But when an appropriate course of action is less obvious, those kinds of verbal repertoires become necessary to supplement other behavior–controlling stimuli. And unique practices, based on integral theoretical approaches, often *do* follow. For example, complex educational practices informed by behaviorological principles frequently differ in important and effective ways from those based upon cognitive/mentalistic principles and assumptions. To appreciate those differences one need only examine contrasting education textbooks like the J. Vargas (1988) behaviorological text, or the Fraley (1996a) behaviorological text, with a cognitive/mentalistic text such as the one by Biehler and Snowman (1990). Or consider the contested revolution precipitated in education during the 1960s and 1970s by the behaviorologically inspired emphasis on behavioral objectives. (For a representative textbook on behavioral objectives, see J. Vargas, 1972.)

The give–away of the products of a discipline, while de–emphasizing or hiding their conceptual origins (as Deitz advocated), may facilitate personal accommodations. But most behaviorologists saw in that strategy the peril of their discipline being extinguished. The expectation that at some future cultural awakening, or in some grand episode of enlightenment, the discipline will either be recognized properly, or will have become recognized to *be* psychology, requires that it retain its integrity until the time of its promised resurrection or prevalence. But in the view of most behaviorologists, the mechanisms of the preservation and survival of the discipline are precisely what gets eroded, suppressed, and precluded in the Deitzian prescription for its interim latency.

Adjustment Problems of Individuals Contemplating the Separatist Movement

Organized professions within one cultural agency often engage the sanctions of another cultural agency, namely law, to extend or maintain their privileged access to professional service markets. In contemplating their professional and disciplinary options, individuals outside such a legally fixated profession sometimes can find themselves confronting legally codified job classifications, legally restricted access to credentials, and legally mandated requirements for specified training.

Members of the Florida Association for Behavior Analysis (FABA, an affiliate of ABA) confronted this issue in 1990 (Taylor, 1990). Proposed revisions to state law would have required behavior analysts in Florida to become licensed under one of five existing groups within five years. The groups were school psychologists, psychologists, clinical social workers, marriage and family therapists, and mental health workers. These groups claimed to know and use "behavior modification" and "behavior analysis." But their members were mostly psychology trained. Their repertoires in behaviorological science and technology were minimal compared to the training expected of FABA members. So FABA members being licensed under

codes designed for any of those groups would have amounted to a mislabeling. And if not licensed, FABA members would have had to work under the supervision of someone so licensed—a situation as odious as it would have been inappropriate. FABA members pursued two courses of action: They successfully obtained an exemption for "behavior analysts" from the new law, and they began drafting their own licensing law. Such actions are to be expected when separate disciplines are involved.

For those behavioral individuals who have not been compelled by law to call themselves "psychologists," an alternative basic discipline, behaviorology, and the disciplinary coalition called behavior analysis have both become available. But other behaviorists have continued to have vested interests in professions for which organized psychology remains the *legal* keeper of the intellectual property rights. Often deemed necessary to protect the public from quackery, that legal mechanism carries the risk of a blindly enforced respect for outmoded ideas. Efforts to win scientific and philosophical arguments with assistance from the strong arm of the law require great sensitivity to that risk. Verbal fixation, even if legally sanctioned, is antithetical to science. The implications of bolstering the relations between scientific ideas and attractive opportunities for economic gain through alliance with the agencies of government and law deserve careful study, especially with respect to long term effects. (For another examination of these perils, see Booth, 1989.)

Some individuals turned away from both the risks and the difficulties of disciplinary independence. Among them some persisted in arguing defensively that radical behaviorists simply entertain a *different point of view* within the more securely and comfortably established discipline of psychology. They seemed not to differentiate, to the extent others did, between (a) separate disciplines and (b) differing points of view within one discipline. For example, when geologists argued over the existence of continental drift, it was an argument *about how best to interpret their incomplete data.* But both sides operated out of the same analytical framework founded in physics, chemistry, biology, and mathematics—with a particular collective conceptual integrity called geology. And because they shared the same philosophical and scientific foundations and examined the same tectonic phenomena, *they were all geologists.* But behavioral and cognitive individuals within organized psychology do not operate out of the same analytical framework nor do they share a common scientific and philosophical foundation (Hineline, 1984). Theirs is not merely a rift resulting from insufficient data, and therefore it cannot be healed through efforts to "get all the relevant facts." The geologists, in the presence of accumulating data, could finally agree about continental drift. But even in the presence of copious and adequate data behavioral and cognitive/mentalistic people still tend to disagree because they do not behave under the stimulus control of data in the same way.

Could cognitive/mentalistic people even *afford* to agree? Lawrence Fraley readdressed this subtle question during a brief talk at the 1996 ABA convention (Fraley, 1996b; also see Sagan, 1995):

> …the natural scientists of behavior who tried to persuade psychologists to change their science and philosophy failed to recognize that psychology, for the most part, was not a scientific discipline, but only a *scientized* discipline. Psychology began as the study of that interface between the metaphysical and physical worlds thought to reside in the mind of man, and that assumption still underlies much of contemporary psychology. Most people in our culture have been conditioned over a lifetime to view a domain beyond the natural world as real, and they deem explanatory reliance on its elements to be rational. The traditional psychology community has drawn much of its membership from that vast population, and represents a subset of it—a cadre devoted to a scientific study of how the natural and non–natural worlds conjoin in a mind that functions as a channeling device between those worlds.
>
> Many such people are devoutly religious persons with whom religious students feel a special affinity, and psychology training programs attract many such students. On their Sabbath, those psychology professors and their student followers pray to a deity… for interventions among variables in the physical world. They assume that a person's communication with that deity is channeled by way of a mini–deity functioning as a vital force within the body. In its religious context it is called the soul or human spirit….
>
> On Monday mornings when these people return to their traditional psychology classrooms, the soul is temporarily renamed; it becomes the "self," and the remainder of the week is spent on a secular scientific analysis of how it might work. Some psychologists, who take no part in the religious aspects, stay focused on what they see as a more *secular* self–agent. But they remain equally mystical.

During those kinds of studies, however, no findings are tolerated that substantially contradict the underlying metaphysical assumptions. That is why traditional psychology can be regarded as a scientized community rather than a scientific community. Those mystical assumptions are not inferred *from* scientific evidence. They are brought *to* the scientific evidence, which is then gleaned for any support that it can lend to those assumptions. This means that in the traditional psychology community, the persuasiveness of evidence, so compelling among the natural scientists, loses cogency in proportion to its threat to those mystical assumptions.

Skinner and his followers never had a chance of making over psychology by demonstrating that practices informed by their natural science were more effective. Their practices, when deemed effective, were merely co–opted and re–described in psychological language so that they seemed new rather than appropriated. (Other effective practices actually originate independently in both communities under common prevailing contingencies. In that case there is no need for either side's science, although once essentially the same practice has appeared in both camps, it is given very different descriptions and interpretations in the respective behavioral and psychological communities.)

Should accumulating evidence force a traditional psychologist to the brink of either abandoning mysticism or discounting valid and reliable evidence, the typical traditional psychologist treats the dilemma as a Hobson's choice—there is no real option. Any science that contradicts the fundamental mystical assumptions is abandoned. People who got into science in the first place in order to shed some scholarly light on the details of their deepest philosophical assumptions (including, especially, those of a religious nature) are not going to abandon those foundations if that science starts causing trouble. Instead, they abandon the science, which at that point is merely an intellectual tool that initially looked helpful, but has proven to cause more difficulties than it is worth.

Behaviorologists understand the mentalistic approach, and have reason to do so. The programs in which many behaviorologists were trained required that they engage in substantial cognitive and mentalistic study. Furthermore, the basic assumptions underlying all of psychology are part of the cultural lore familiar to most people. Psychology embodies a scientized incorporation of secular cultural lore and the metaphysics prevalent in religion—all matched as closely as possible to whatever the physiology–based neural scientists can say about how nervous systems function. Behaviorologists also understand the mentalistic approach because the analytical mechanisms of behaviorology are particularly suited to overcoming some of the riddles that, unsolved, allow mentalism to seem more reasonable. But the reverse is seldom true. People with the typical cognitivist/mentalist training usually lack comprehension of the behaviorology repertoire. Furthermore, they face an additional major repertoire deficit insofar as they construe their professional tasks to be those that behaviorologists have defined for themselves. Overcoming that deficit could take years of new study requiring substantial relearning of basics. Radical behaviorism and the science that it informs are complex and difficult to learn. Such costs are usually too great for an established professional person to bear. Denying their worth has always been easier than mastering them.

Rationally, instead of bearing those costs, a person might resist, in some cases by ignoring scientific evidence and eschewing open academic debate. Often the resistance takes the form of resort to available counter–controlling arrangements embodied in the politics of scientific verbal communities or to remedies available through appeal to other cultural agencies (e.g., law) for help and protection.

The behavioral psychologists, such as those at the 1987 ABA convention who sympathized with a continuing effort to change psychology from within, will perhaps carry on the attempt to persuade listeners who cannot readily afford the implications of the message. But behaviorologists, by organizing independently, are free of the contingencies to expend themselves on that continuing classic exercise in futility. Each behavioral individual must deal at the personal level with the question of what kind of professional contribution to make, and in which disciplinary and organizational framework to make it. One can work primarily to serve one's self, one's group, or one's culture in its best long–range interests. The cost of personal commitment in any one of those domains might be forfeiture of reinforcers available in the other two (for discussions of this issue, see Wood, 1976, and Vargas & Fraley, 1976).

At this writing, many behaviorally oriented scholars have no realistic alternatives to operating within units representing organized psychology and must find ways of

surviving there. The shaping of behavioral individuals under contingencies of accommodation with mainstream psychology can produce self–punishing compromises of scientific and professional integrity. The escape from these is sometimes a forced and often convoluted rationalization. The person might try to find some virtue in being suppressed or in being forced to pretend a devotion to eclecticism. A more aggressive defense of one's disciplinary integrity can require complex and sophisticated repertoires of academic and political conflict that few individuals have had an opportunity to acquire and refine. Without the skill to conduct an effective kind of fight (and often without much hope that attempts will yield more than a glorious career demise), persons trapped in organized psychology can be left privately resentful at what has happened to them. Among that subset some might seek relief by reciting comfortable half–lies about the cleverness of subtle infiltration or the sacrificial devotion of existence as a behavioral mole. Meanwhile, they could hope that their occasional limited victories or accomplishments would yet save their science.

Behaviorologists did not enjoy any special immunity from such self–protective contingencies of accommodation. Nor could one argue that personal interests played no part in behaviorologists' respective commitments *to* the separatist movement. However, behaviorologists simply arranged to avoid having to contact many contingencies of accommodation. The behaviorologists' approach to professional and scientific probity was to arrange not to have to share a discipline in the first place—especially with multitudes who (a) were untrained in behaviorological science, and yet (b) were organized for the political and ideological defense of the strongly conditioned reinforcers of their own discipline.

Psychologists might force unattractive compromises on some behaviorists within organized psychology, making the separation issue complex for them. But the behaviorologists tried not to forget how complex the separation issue could also be for those behaviorists unaffected by contingencies of personal self–protection or self–interest. Aside from any special strength that aversive contingencies and various self–interests might lend to rationalizations about eclecticism, other behaviorists *were* taking into account the same complex and extensive mix of relevant variables that behaviorologists had considered. However, unlike the behaviorologists, they were oppositely concluding that their discipline must develop *within* psychology. They were hoping eventually to monopolize the present disciplinary coalition that comprises psychology.

As most behaviorologists have always agreed, where best to develop this discipline *is* a difficult judgment call. Intelligent people differ. Individuals respond to such a mix of variables according to their respective personal and professional histories and current circumstances. And they behave accordingly.

The Adjustment of the At–Large Behavioral Community

As TIBA and the behaviorology movement developed within the broader behavioral community, some resistance, perhaps loosely coordinated, continued. It was generally confined to individuals whose investments and values committed them either to organized psychology or to the semi–independent "behavior analysis" movement. During and after 1989, following the earlier flurry of rather open debate, these resistors could clearly see that a certain group would seriously continue its pursuit of an independent behaviorology discipline. Some behavioral opponents then responded in ways that to the behaviorologists seemed like extinction procedures: Public mention of the behaviorology movement did not occur in some contexts that would normally have included it. Articles referring explicitly to behaviorology seemed to meet extra difficulties wending their way through the publication process. Reports surfaced about people being advised to ignore the behaviorologists (these were first–hand or second–hand reports, but they had the usual behavioral effects nonetheless). Some uncommitted individuals who had been maintaining collegial exchanges about the movement through correspondence with behaviorologists ceased their responding. A couple of people explicitly mentioned pressure from colleagues to do so, and reported that they were discouraged by the responses of some colleagues to any mention of the behaviorology movement.

Yet at the same time, other behavior analysts were pointing anew to the irreconcilability of the two disciplines. McDowell (1991) advanced the unusual argument that the differences between behavior analysts and psychologists stem from their *ontological* differences: They do not recognize the same realities. "This difference is so fundamental that I think it unlikely that the two disciplines can be reconciled" (p. 29). McDowell provided numerous examples. Nevertheless, he recommended, as have other behavior analysts, still another attempt at "enlightenment" to demonstrate to an implicitly psychological audience the kind of quality science and philosophy that should prevail.

Opportunities to answer opponents' questions did occur. For example, to encourage such exchanges, Deborah Shanley asked Lawrence Fraley, Stephen Ledoux, and Ernest Vargas to appear as panelists in a discussion session at the tenth annual conference of the Berkshire Association for Behavior Analysis and Therapy (BABAT) at the University of Massachusetts at Amherst on 14 October 1989. The topic was the interrelations among TIBA, ABA, and AABT (the Association for the Advancement of Behavior Therapy). The session, with Joseph

Morrow as moderator, attracted a crowd of about thirty people. Three rather prominent persons in the audience accounted for much of the audience participation. Their work, though in different fields, was informed by behavioral psychology, and they had professional ties and loyalties to ABA. At least two of them were explicit advocates of continuing with attempts to change organized psychology. They were enthusiastic about the kind of organizational independence ABA manifested. And ABA was where they thought their attempts to change psychology could be based. At the session they challenged the efficacy and appropriateness of the behaviorology movement. They raised questions about TIBA (a) competing with ABA, (b) enjoying little political power, and (c) having few members. Respective answers focused on organizational complementarity, the apolitical purposes of TIBA, and the value of getting right before getting big.

In general, behaviorologists would have preferred the early support of more people. But TIBA did not need the broad–based political power to which some critics pointed as a deficiency. In fact, a relatively small number of followers during this early organization and design phase had some benefits. TIBA leaders were experimentally and developmentally applying their own science to designing and establishing a new kind of scientific/professional verbal community of some complexity and sophistication. And they were doing so without benefit of comfortably sufficient prescriptive precedent. With their science under so many threatening contingencies, taking each next step correctly was more easily accomplished with a smaller–scale model. And for some people, being able to do that outweighed the importance of swelling the ranks.

One of the BABAT conference questioners did press for clarification as to whether the behaviorology movement was based on new science or simply represented a disciplinary reorganization. That was a difficult question, because the answer is complex: Through the new movement, old science *had* emerged within new organizational arrangements. While a new verbal community had been established, its founding members spoke an older scientific language. But the movement, through reorganization, relieved constraints that had been hindering the continued development of new and better science. Thus behaviorologists fully expected that in the more propitious professional atmosphere of the new disciplinary organization, the science of behaviorology *would* evolve constructively away from what prevailed in behavioral psychology, behavior analysis, and behavior therapy. Subsequently, TIBA would design certain organizational arrangements (discussed in an earlier chapter) precisely to evoke more new scientific activity by members.

During the same period, typical signs of permanence continued to accumulate with respect to behaviorology. The TIBA letterhead appeared frequently over items of official correspondence. TIBA had established the annual behaviorology convention, the TIBA newsletter, *Selections,* and *Behaviorological Commentaries,* as regular events. And ABA was asking all members to indicate on the membership form which discipline, psychology, behaviorology, or some other discipline, informed their work. At the same time, opponents of behaviorology were leaving unanswered some difficult questions implying the appropriateness of the behaviorology movement which behaviorologists had raised in the literature and at meetings.

Meanwhile, routine and often innocuous public acknowledgments of the behaviorology group gradually began to reappear in contexts where they might be expected. Typical of these was a statement in the newsletter of the Cambridge Center for Behavioral Studies reporting that a "…consortium, chaired by Barbara Etzel, has representatives from the Cambridge Center, ABA, Division 25 of the APA, and TIBA" (Recruiting diversity…, 1993).

Psychologists Tighten Control on Behavior Analysis Journals

Concerned radical behaviorists launched the behaviorology movement in part on the conviction that the behavior analysis movement could neither guarantee the integrity of radical behaviorist philosophy and the science that it informs, nor adequately support the further growth and development of these. The erosion of the radical behaviorist character of the behavior analysis movement could be seen in the changing character of behavior analysis literature.

Behaviorism. The journal *Behaviorism*, founded by Willard Day, had for years been the philosophical journal of the behavioral movement in the United States. It was the principal literary medium of the radical behaviorist philosophy of science. When Peter Killeen and George Graham replaced Willard Day as co–editors of that journal, they set forth in their first editorial the evolutionary trends that they would pursue (Graham & Killeen, 1985). After commenting on the necessity of scientific evolution, they acknowledged the production of variation through the mechanisms in organized science, declared the importance of meta–scientific reviews of disciplines by the philosophers of science, and began to mention their new directions. They would "facilitate…the generation of new variants of behaviorism, and the selection of promising variants for further development" (p. 1). This, they noted, would restrict the focus and emphasis to "measurable public behavior." Among topics specifically encouraged for authors were *"theories of learning"* and *"the role of cognitive variables"* (emphasis added). The editorial board would be continued under the old policy restricting membership to "psychologists" and "philosophers."

Given a lenient interpretation, none of this new focus would necessarily *have* to fall outside of a behavior–analytic framework, nor, as far as scientific and philosophical principles are concerned, even a behaviorological one. But to some behaviorologists and behavior analysts, these statements and characteristics seemed restrictive, peculiar, and neglectful of the verbal integrity of the radical behavioral community for which, and to which, the journal had long spoken. Those provisions would afford comfort and imply welcome both to traditional psychologists and to those in the behavioral movement anxious to nudge the movement away from a separate, independent disciplinary home for radical behaviorism and back under the umbrella of organized psychology. Early in 1989 the two editors sent out renewal notices on which the phrase "behavioral psychology" was defined to "include the experimental analysis of behavior as well as recent developments in *philosophy of mind*" (emphasis added).

At about this same time the behaviorological community was saddened by news of Willard Day's fatal heart attack. As people paused to reflect on Day's professional lifetime of support for quality behavioral science and philosophy, many regretted the apparent disciplinary regression in the journal that he had founded and edited for many years. Although Day had never taken his journal out of the psychology community, he had maintained its integrity as a journal serving the radical behavioral minority. The new editors seemed to be courting a wider audience at the expense of that integrity. Earlier, at the 1988 ABA convention, Lawrence Fraley had spoken with Willard Day about the behaviorology movement. Day listened carefully with his usual great interest. He responded that any movement dedicated to preserving the integrity of radical behaviorism appealed to him. In addition, he had asked to be kept informed.

By 1991, editor George Graham was introducing Volume 19 (1) of what had been *Behaviorism* with a prefacing editorial in which he wrote:

> Between 1985 and 1990 the editorial scope of the journal expanded rapidly under catholic editorial tastes and pressures within psychology for eclectic theorizing. So *Behaviorism* in 1990 did not appear as *Behaviorism.* It appeared under the current name of *Behavior and Philosophy.* This is how it will continue to both call and describe itself: a vehicle…for the philosophical reflections of psychologists and the psychological ruminations of philosophers. (unnumbered preface page)

In 1992 Howard Sloane, a TIBA member, became Executive Director of the Cambridge Center for Behavioral Studies which publishes *Behavior and Philosophy.* Sloane was generally known as a supporter of the original concept of this journal. With his leadership of the Center, many behaviorologists hoped to see a reversal of the trend away from radical behaviorism in *Behavior and Philosophy.*

***Journal of the Experimental Analysis of Behavior* (JEAB).** In 1987, Edmond Fantino began his tenure as editor of JEAB. In his introductory editorial (Fantino, 1988), he defined behavior analysis to be *within psychology* and behavior analysts to *be psychologists.* He described one change that he would introduce to the journal as "broadening the conceptual scope of articles that appear in the journal" (p. 1). Fantino suggested that behavior analysts have shied away from "cognitive phenomena," though he did mention some exceptions, and he declared that "behavior analysts certainly have not been at the forefront of the recent movement in cognition." This he lamented because he believed behavior analysts could contribute functional analyses of "language and cognition."

Fantino's enthusiasm to reassociate the behavior analysis movement with mainline psychology could easily be inferred (a) from his implication (the matter of justification aside) that a valid movement in the field of cognition had been in progress, and (b) by his statement to the effect that, by returning to the psychology fold, behavior analysts would find those cognition studies worthwhile and could begin to get a piece of the contributory action. However, when Fantino described the potential nature of those behavior analytic contributions, he suggested that they could offer alternative analyses of the data adduced in cognitive psychology. Thus, hidden beneath the overtures of disciplinary balance and cooperation were implications that (a) cognitivists lack a science adequate to interpret validly their own data, and that (b) the behavior analysts could follow with better analyses of language and cognition—as if, perhaps, to co–opt the "cognitive revolution."

By 1989 the journal was clearly reflecting Fantino's concept of a cognitive–behavioral blend in which behavior analysis had been relegated to a still important though quite insufficient aspect of an implicit psychology discipline (White, McCarthy, & Fantino, 1989). In 1991, the radical behaviorist Charles Catania (1991), in an open rebuttal to Michael Mahoney (1989), noted that JEAB included papers "…even on those research lines that you regard as forcing the revision of and challenging radical behaviorist accounts (p. 67)" (though his examples predated Fantino's tenure as editor). Catania, a self–identified behavioral *psychologist* and skilled career–long miner of the psychology literature, implicitly accepted such a mix of contents as worthwhile. Behaviorologists grant the possibility of discovering worthwhile fragments in the psychology literature. They object only on what are fundamentally economic bases to the inefficiency of a literature laced with irrelevant and often invalid paradig-

matic influences (see J. Vargas, 1990). Unfortunately, Fantino did not address the ultimate fate of behavior analysis and its literature as an insecure enclave within the larger verbal community of psychology.

Division 25 Recorder. The journal of APA Division 25, *Division 25 Recorder* (and briefly renamed *Behavior Analysis*) was always strictly a psychology journal. When the rift in the American Psychological Association (APA) resulted in the formation of the independent American Psychological Society (APS) in 1988, APS leader and advocate Linda Parrot was editing the *Recorder*. Parrot's APS advocacy was shared with her spouse Steven Hayes, another APS leader and once Division 25 president. In APA Division 25 Executive Committee meetings Hayes, against opposition, supported moving that Division from APA to APS. Presumably this would have moved the *Recorder* to APS as one of the transferred assets ("Minutes of," 1988). In the meantime, within the new APS, plans were announced to begin a new journal modeled after *Science* magazine (Salzinger, 1988, p. 118). Yet the fact remained that control over both *Behavior Analysis* (a.k.a. *Division 25 Recorder*) and any new journals of APS would apparently remain within the discipline of psychology regardless of which organization owned them. This was because both APA Division 25 and the new APS were strictly psychology–related organizations. They were populated by persons who saw themselves as psychologists and who regarded behavior analysis merely as a facet of psychology. Publications subsequently emerging from APS reflected the mainline psychology perspective of the vast majority of its members. Readers found these to be less behavioral than materials that continued to be published by the "behavioral" division (Division 25) of APA.

Conclusion on journal control. The general drift toward a cognitive–behavioral blend in the behavioral psychology literature seemed an inevitable aspect of the strategy to show the psychologists some better science by pretending merely to represent a part of their own psychology discipline. The change in character of these various journals was also rational for economic reasons, because minority "behavioral" journals sell poorly in the vast psychology marketplace. The changes were also probably an inevitable aspect of the effort to merge the selectionist and transformational paradigms and thus consolidate the psychology verbal community. But this drift in the character of the traditional behavioral literature left many authors, especially the behaviorologists, looking for alternative publication outlets. The effort within TIBA to publish *Behaviorology* seemed timely.

Internal Issues Debated

Early participants in the behaviorology movement were much in agreement. Yet among themselves they debated several issues, some of which are described in this section. Some pertained to the nature with which individuals wanted to endow the movement. But debated more frequently, and often more vigorously, were the *strategies* by which the movement should develop.

Contest? To what extent or in what ways, if any, were behaviorology and psychology in a real contest, and should their relation be characterized as such? This question about the behaviorology movement continued to preoccupy the early behaviorologists. (It would subsequently become a main theme in the lead article of the first issue of *Behaviorological Commentaries* [Fraley, 1991].)

People in the culture at large have presumed that psychology is the discipline to fill the cultural niche reserved for whichever discipline can best support effective behavioral technologies. Behaviorologists regard that as a mistake, because it is behaviorological science that has most demonstrated effective technological applications. So organized behaviorology *is* left to compete with organized psychology for what should be behaviorology's appropriate stall space in the cultural marketplace.

If psychology and behaviorology could *divide* the subject matter—with behaviorology focused on environment–behavior relations and psychology concerning itself with internal mediations of behaviors—then any adversarial relation could give way to a more complementary or supplementary relation. But some behaviorologists did not want to abandon the study of relations between bodies and behavior to psychology. A few early behaviorologists, Carl Cheney for example, were actively researching relations between *physiological* and behavioral variables. In general, whenever alternating the level of analysis—between behavior/environment functional relations and the internal workings of the body that mediate the production of behavior—becomes necessary, behaviorologists prefer interdisciplinary transfer between behaviorology and physiology, which keeps the analyses within the realm of the natural sciences.

Nevertheless, such a division along subject matter lines appealed to many early behaviorologists as a gesture of political appeasement. To the extent that psychologists would accept it, behaviorologists would avoid provoking a territorial fight with a politically and economically well established giant. The behaviorologists actually have had little scientific respect either for the rampant cognitive theorizing of psychologists or for their efforts to correlate those theories with real physiological events adduced by the physiology–based neural scientists. This is mainly because behaviorological science provides parsimonious alternative explanations that render many of those cognitive/mentalistic theories improbable in the first place (see Skinner, 1953).

In addition, behaviorologists sought to leave behind with the psychologists more than just a domain of subject matter irrelevant to behaviorological concerns. Behavior-

ologists have always objected to the *non–natural* aspect of psychology, that is, the fundamental metaphysical concepts of life, behavior, and the human species that no person or group has ever succeeded in effectively disabusing within psychology and which have been conducive to the psychologists' prevailing mentalism.

Some behaviorologists remained unwilling to settle for any resolution of that territorial dispute in ways implying that the behaviorology community accepted psychology as an equal, worthwhile, and respectable partner at the roundtable of the natural sciences—a partner that merely focused on subject matters different from those studied by the behaviorologists. Of course, the behaviorologists did not include in that concept of organized psychology the scientific contributions of those behavioral psychologists whom they thought belonged in behaviorology. But many behaviorologists believed that in the long run such respect for psychology, whether believed to be true or feigned for strategic reasons, would evoke an adverse judgment from the grand community of natural sciences at large. That community would simply lump behaviorology together with what would eventually become discredited behavioral pseudo–science. Though some behaviorologists appeared not to share this concern, in general they offered little articulation of their view.

The fear that the behaviorology movement could easily become preoccupied in premature fights with the much larger psychology community was not without basis. Critical organizational resources would be consumed and a hostile organizational giant would be aroused. The opposing views in open contests with organized psychology represented the age old tension between prudence and integrity (some saw it as the shrewd versus foolish variety). Perhaps some behaviorologists covertly anticipated that the psychology verbal community would, in fact, *eventually* develop a better scientific paradigm, apply it to its own internal disciplinary house–keeping, settle the implicit territorial disputes both with physiology and behaviorology, and claim a legitimate place beside behaviorology at the natural science roundtable. Others merely seemed reluctant for personal reasons to suggest that some important, capable, and very nice people had been wasting their scientific time with psychology.

Would behaviorology, its followers eschewing mentalism as they did, be *allowed* to gain scientific prominence within the culture? That is (the issue of logical subject matter divisions aside) would organized psychology in conjunction with a mentalistic culture at large (bolstered by some current legal codes) move to prevent the prominence of any integral and entirely natural behavior science? Would psychology instead act to preserve a metaphysical interface wherever behavior is studied? The purely natural science of Darwinian biology had faced a similar challenge and, with continuing vigilance, has prevailed. But could behaviorology duplicate that accomplishment? Could behaviorology affect a quiet and peaceful emergence as the independent natural science of its own culturally important subject matter? Or should it prepare vigorously to defend, more fundamentally, the very *concept* of applying natural science to behavior within this culture—a course that might produce conflict with organized psychology? Skinner had anticipated a contest at this fundamental level and had described the issues attendant to it in some detail (see Skinner, 1953, pp. 17–22).

Discipline/field/status relations. The emergence of the behaviorology discipline stimulated debate among the behaviorologists about the definition of "discipline." Difficulties arose because peoples' discipline–related concepts differed. Effective communication depends on terms defined in common, but agreements about concepts of discipline proved elusive. Some people (e.g., Fraley 1988d, 1990a) thought that they could distinguish a mega–repertoire consisting of an extensively shaped answer to the general question of how best to know, and called, simply, a "discipline" (…of something). Such a repertoire was construed to include several components. Two verbal components were (a) a philosophy of science and (b) a set of scientific principles. A third component (c) consisted of an accumulated set of prescriptions or practices of the kind usually implied by a term such as *behaviorological technology.* Through the technological practices, the principles of the science were related to, or applied to, a particular subject matter, and the data thereby adduced could evoke the induction of further scientific principles. Other people, though, did not clearly discriminate philosophical, scientific, and technological elements in given cases. Nor could parties to the discussion reach agreement on which aspects of such a mega–repertoire were entirely verbal. Some also questioned the asserted functional relations among philosophical, scientific, and technological behaviors.

However, the frequently appearing phrases "organized discipline" and "scientific verbal community" seemed to be synonymous. Many also tended to use the terms *field* and *discipline* interchangeably in common parlance. According to one attempted discrimination, the locus of the variables definitive of a discipline resided in the behavioral repertoire of the person (e.g., behaviorology). The variables definitive of a field resided in the environment, or more specifically, in the piece of the environment said to be the subject matter of concern (e.g., law or education). One worked *in* a field *with* the disciplinary repertoire that one had brought to it. The following comment, which appeared in the first issue of *Behaviorological Commentaries* (Fraley, 1991), is a partial elaboration of this distinction:

> Within our culture there are only a few distinctly different *major* approaches

> to analytical thought *about behavior.* Here's what I mean by that: If I am preparing myself to become a teacher (which happens to be *my* applied area), my culture offers me only a few major ways to think about the relevant behavioral events that I will encounter in the field of teaching. To name the obvious and familiar ones, I can think behaviorologically, essentially relating environmental variables to behavioral events, with a heavy explanatory reliance on selection mechanisms at several levels of analysis; alternatively, I can think psychologically, relating behavioral events to internal cognitive transformational processes, which may or may not include appeals to metaphysical influences; and I can think purely metaphysically by relating behavioral events more or less exclusively to metaphysical variables in other–world domains.
>
> Since, in my field of education, all students have long been required to study psychology in order to acquire their basic analytical approach to behavior, almost all educators are psychologists as far as their basic analytical philosophy–science paradigms are concerned. But then, so are almost all nurses, lawyers, clinicians, advertisers, and practitioners in hundreds of other applied fields. The culture offers students hundreds, maybe thousands, of *fields* in which to apply their respective basic few ways of thinking about behavior.
>
> Behaviorology is not one of those applied fields; it is a basic scientific discipline, including repertoires of science and philosophy, which can be applied to the problems in *any* of those many applied behavior–related fields. And importantly, *so is psychology.* This does not refer to those applied fields that also go unnecessarily under the expropriated title of "psychology", but rather in the sense of a basic science and philosophy as a fundamental and comprehensive way of thinking about any behavioral phenomena. (pp. 10–11)

Given that difference, a person would bring a disciplinary behavioral repertoire to bear on the problems encountered in a field. One implication, however, was that a person's complete professional description would often require two terms. One would indicate the discipline with which the person solved problems. The other would indicate the field in which the person worked and encountered those problems. The need for such labelling is implicit, for example, in efforts to talk accurately about educators. A satisfactory description often requires that a given educator be designated as a behaviorologically informed educator or as a psychologically informed educator. (For an example of material that is heavily psychological but also substantially informed by a metaphysically based discipline, see the text by Scoresby & Price [1991], two educators at Brigham Young University, an institution sponsored by an organized religion.)

On the sociology of science front (see Fraley, 1988d), these concerns gave rise to another sort of issue: At the start of the 1990s, several important and respected people, whose repertoires could be called behaviorological, had not yet endorsed the separatist approach of the behaviorology movement (though some had joined TIBA as affiliate members). They were continuing to work for change within organized psychology, and they regarded themselves as "psychologists." But what did that mean in any particular case? All of them seemed to want change in the mentalistic disciplinary tradition of psychology, but toward what definition of psychology was such a person working? In a psychology purged of mentalism, would the subject matters include the behavior–environment relations *to which behaviorology had already laid claim?* Those people would have to make clear their own positions with respect both to their disciplines and to their concepts of how subject matters were to be divided among recognized disciplines for study and practice.

Theoretically, the positions taken by those people had implications for the behaviorology movement. Separate disciplines could hardly exist with one called psychology and one called behaviorology and both defining their subject matters identically. But if the behaviorologists were correct about the futility of changing psychology, some of the potentially adverse implications of such territorial conflicts would not be realized because psychology would *not* change. Behaviorologists argued about the extent to which attempts to analyze the professional status of radical behaviorists in psychology were even necessary. Some thought such analyses were presumptuous; they intruded on those peoples' right to fashion their own careers. Others thought such analyses were important to the behaviorology movement; no matter what radical behaviorists did, their actions and the implications of those actions should be clearly understood by all parties. Discussion continued about how the organized behaviorology movement should relate to a person who was scientifically behaviorological yet who remained in psychology while wanting to maintain some tie to the behaviorology movement.

As the behaviorology movement slowly pulled away on its independent course, those in TIBA who had persisted in seeking some sort of straddling posture found that position increasingly untenable. (However, as of this writing, straddlers have tended not to drop out of TIBA, perhaps because of the Affiliate membership category available to everyone.) They had argued, in effect, that the behaviorology movement should lever from *within* organized psychology for change in the nature of psychology. But that was not to be, and ultimately each such person would probably move, however gradually, to one camp or the other.

Many such persons had *not* joined TIBA. Mutually respected colleagues looked at one another across the widening gap sometimes with annoyance, and increasingly with sympathy and occasionally regret. Among all who might have liked to share in the independence venture, pleas for understanding were cautiously exchanged between those who had subsequently committed to doing so and those who had not. Long term trends seemed to point to worsening conditions for those who stayed behind within psychology. Perhaps additional behaviorological people would yet shift to the independence movement. In the meantime, behaviorologists considered which approach would better promote or facilitate such moves: Should those potential affiliators be confronted with (a) explicit, challenging, and thought provoking analyses or (b) with a happy face that explained little but teased with the implication that professional life as an independence–oriented behaviorologist is reinforcing?

Science club versus cultural mission. The need for behaviorologists to focus on the evolution of their science became increasingly evident. As the 1990s began, the future of behaviorology as an independent discipline clearly became more secure with each extension or refinement of the science. Not only would more science be needed but it also had to evolve with its own scientific character unique to behaviorology. The more effective and unique their science became, the better would be the position of the behaviorologists to pursue the mission of providing the culture with the technologies of an independently organized natural science discipline of behavior.

However, an introspective preoccupation with the science could de–emphasize if not neglect that cultural mission. Some behaviorologists, in their enthusiasm to get on with such a science development program, appeared willing to put aside most issues pending at the interface between the discipline and the ambient culture, such as maintaining a full publication program or establishing behaviorology departments in universities. Some even appeared ready to neglect or de–emphasize the separateness of their discipline in presentations to the outside. They would seemingly have the movement take on more the character of a science club focused on science *per se*, but with little attention to how the behaviorology *movement* related organizationally to the various facets of the culture.

Other behaviorologists thought that such neglect might be imprudent, especially if carried to what they feared might be an excess. For example, they wanted to see the scientific outcomes and other products of behaviorologists function to strengthen and support their own organized behaviorology discipline and its cultural mission. They did not want to see the scientific works or other products of behaviorologists in TIBA co–opted to support or strengthen organized psychology. After all, many people found themselves under strong and mostly economic contingencies to maintain links to organized psychology. And under those circumstances people, perhaps under the guise of acting as change agents, could easily find themselves consolidating their position by allowing the benefits of their behaviorological products to accrue to the psychology establishment.

While TIBA remained a scientific organization without a pursued cultural mission, some members raised the question of what place a research–focused TIBA reserved at its scientific roundtable for those who would apply the science to the analysis of social issues. These could be as diverse as contemporary philosophy, corrections, death and dying, education, organizational behavior management, or verbal behavior, to name but a few. The implications that their work was not as welcome nor as respected within TIBA as that of data–based researchers weakened the bonds between TIBA and some of the more applied analysts of behavior. Concerns began to arise about the implicit skew in what had begun as a more balanced disciplinary verbal community in which conceptual analysts and application specialists were equally appreciated for their work in mediating the impact of the science on the culture.

In concentrating somewhat exclusively on a science development program, some TIBA leaders also wanted to keep the TIBA membership low. Each member might then know all others on a first–name basis, and at conventions, especially with intimate single–track formats, each participant would engage all others. In such ways, limited membership could improve social and scientific exchanges among members and help consolidate the scientific efforts *of that small group* of people.

However, limiting membership to gain these enhancements for so few could put severe limits on how quickly and thoroughly behaviorologists could deliver the needed depth and range of behaviorological products to the culture. For while TIBA leaders focused on developing science with only a small membership, threats to worldwide cultural survival (e.g., over–population and ozone depletion) loomed, as did many other cultural problems (e.g., control by an often costly and excessive use of punishment, and misplaced respect for non–natural con-

structs of many kinds such as the reliance of the legal system on the putative free will of "autonomous man"). Theoretically, applied behaviorology could help solve them all. And those behaviorologists who viewed neglect of the cultural interface as imprudent construed many of these concerns to be enlarging exponentially and overtaking humanity on a time scale shorter than the time scale on which *a small group* might successfully address them. Hence they argued for greater organizational efforts in the membership and cultural interface areas.

Those behaviorologists who stressed the importance of the cultural interface made suggestions. In practical terms, to support the relation of the behaviorology movement to the culture at large might require, for instance, a higher organizational priority for TIBA's publication agenda than occurs under a primary focus on scientific development. The publication priority needs to be high enough to ensure a continuous flow of high quality journal and magazine articles. These would properly display behaviorology while providing TIBA's rank–and–file members with valuable returns on their organizational investments including, reciprocally, further extension and refinement of their science. To get such a result, TIBA would probably have to appeal more proactively, rather than just by passive example, to convince additional behaviorological scientists (laboratory and applied), scholars, and students of the appropriateness of the behaviorology movement, persuading such people to join TIBA and/or to contribute to its literature. TIBA has neither sought nor required a large political base through which to have an impact on the culture. But it does need to serve as the medium of organizational expression for behaviorologists and behaviorological scientists who, if not operating under the auspices of TIBA, might remain vulnerable to having their work co–opted in service to antithetical or competing disciplines. Such suggestions have not, as of this writing, had much impact.[1]

A small organizational membership also facilitates control of the organization by its smaller cadre of leaders. But to address this or any other concern, an expanded recruitment that would bring in less committed people was not a goal. The recruitment issue pertained to other existing behaviorologists (by function if not yet by name) who thus far were neglected by organized behaviorology. Cultural impact aside, the vitality of TIBA's science agenda required a greater participation by these people.

The difficulty resolving these issues took its toll. As *Behaviorology* (with the exceptions of most of the Presidents' presentations and the B.F. Skinner Memorial Addresses) rejected otherwise reasonable manuscripts because they were not data–based research papers, its regular publication schedule became intermittent after three issues. *The International Behaviorologist,* in which such manuscripts might have been published, had not materialized primarily for lack of funding in an organization with a preference for keeping membership small. And potential submissions to it may have decreased anyway as members experienced pressure to refocus their priorities onto data–based researches (an effort that apparently was still not yet productive enough to insure the viability of a pure research journal).

David Krantz Revisited

Today [1992.—Ed.] behaviorologists look back twenty years to David Krantz's (1971) carefully constructed and well presented critique of the era in psychology when serious separatist rumblings began (reviewed in Chapter Two). The interpretations by Krantz, and others who have echoed his biases, have lost much of their cogency. The issue of commensurability has been settled despite some continued grumbling: Some psychologists study the fictional construct called "information" along with its processing by *autonomous* minds, psyches, or other behavior–originating agents thought to exist within organisms. In these efforts they study events *different* from those that concern behaviorologists and they do so from a philosophical perspective that puts them beyond the bounds of natural science. Natural science does not recognize discontinuities in any functional chain of causal events such as those implicit in concepts of free will or autonomous selves. Other psychologists dwell on emotional behavior, endowing it with special importance and often with metaphysical interpretations. To behaviorologists, feelings are respondent behavioral effects, which, like so many others, can be engineered to prescription. Still other psychologists treat behavior as a natural phenomenon but depend heavily on inferred hypothetical cognitive constructs which seem necessary given the conceptual structure of the science with which they attack problems. The latter kind of psychologists differs with behaviorologists mainly about the *appropriate* science. The former kinds differ mainly about *philosophy* of science. And both, consequently, differ with behaviorologists about what aspects of the subject matter upon which to focus. Any of these classes of difference justify organizational separation of the respective verbal communities.

One can now recast the issue of behaviorological isolationists allegedly reducing the effectiveness of their

[1] Subsequent to the considerations described in this chapter—and hence not covered in this chapter or the other chapters of this paper—further organizational developments occurred in support of the disciplinary missions of behaviorology, including the founding of TIBI (The International Behaviorology Institute). For details, see Ledoux, S.F. (2002). Afterword. In S.F. Ledoux. (2002). *Origins and Components of Behaviorology—Second Edition* (pp. 337–357). Canton, NY: ABCs.—Ed.

science by ignoring the potential contributions from other schools. Some people contemplate a kind of scientifically respectable cognitive science existing as a specialization within physiology along the theme of brain science. Scientists within that subset, whose main thrust is to seek physiological evidence for their inferred hypothetical cognitive constructs, would perhaps describe themselves using psychological labels. But, regardless of how scientifically legitimate such a seemingly over–theorized field might become, that field would not require the interest of behaviorologists—only their acknowledgment. On the other hand, a person who does not fully respect a natural science philosophy nor master the science implicit in applying selection concepts to behavior is left with scientistically rendered, traditional, non–natural assumptions. No valid basis exists for behaviorologists (or other natural scientists) to treat those assumptions as scientifically worthwhile, nor whatever disciplines they represent as scientifically qualified alternatives to behaviorology (also see Frase, 1970, p. 337, and Sidman, 1986a, pp. 214–215, concerning the lack of disciplinary integrity in psychology).

The argument that cognitivists and mentalists have novel and important things to say to behaviorologists now seems to amount to little more than the easy observation that persons under different stimulus controls might look at different things, a fact that carries no concomitant guarantee that effective reports will follow. Occasionally psychologists, looking at events to which behaviorologists have given little or no attention, report something of worth to behaviorologists. But in such cases rarely if ever has it seemed to be something that behaviorologists could not have produced were they to have addressed those same issues. Julie Vargas (1990), concluding her analytical rebuttal to Salzinger's (1990) argument that behavioral people should read cognitive literature because they would find it useful, wrote that:

> …reading cognitive studies is, for a behaviorologist, like looking through a bargain basement. You must search through many unsuitable items before finding something you can use. When reading time is limited, why not go where the density of valuable finds is higher—directly to behavioral work? (p. 201)

When some behavioral people continue to insist that culling cognitive literature is fruitful, questions arise: Do their discoveries really withstand behaviorological tests for quality scientific products, or are the discoverers being overly enthusiastic (see J. Vargas, 1990)? Are their searches, and public proclamations of delight at making discoveries, mostly tactics in a social strategy of accommodation with their host cognitive/mentalistic communities? Could the discoveries that *are* of real value to behaviorologists have resulted *only or exclusively* from applications of a cognitive or mentalistic paradigm? Or were they produced *in spite of* those repertoires (which in those instances did not happen to get in the way)? Behaviorological colleagues have little basis for criticism when an individual's culling through cognitive literature survives the critical scrutiny implicit in such questions. Many behavioral scholars, having spent much of their professional lifetimes scrutinizing psychological literature for useful items, have grown skilled at finding them. But the behaviorologists raised the question of how much more fruitful their labors would have been had the discipline of psychology been capable of producing a more uniformly worthwhile literature.

Krantz also dwelt on the role of professional courtesy in the relations between professionals in different disciplines. Professional courtesy in some appropriate form will always have its place. But psychologists, in mistakenly regarding behavioral people as a part of psychology, have rationally expected the sort of professional courtesy from behavioral people that they would expect from other psychologists, a courtesy apropos of collegial *disciplinary peer* relations (see the discussion of Proctor & Weeks, 1988, in an earlier chapter). But the view that behavioral people should have continued to express deferential professional courtesy to those who have devoted their professional lives to a different discipline—in some cases featuring philosophies that tolerate non–natural science—and to have done so in such a way as to prolong the suppression, eclipse, or neglect of the natural science of behavior, was an argument that by 1990 had worn thin for most contemporary behaviorists, and especially for behaviorologists.

Increasingly, behaviorologists appeared more comfortable and confident with their own discipline. In their own respective ways they behaved more frequently as if behaviorology–suppressing deference were not polite, but self–demeaning; as if it did not extend courtesy, but implied weakness; as if it confused and misled students; and as if it damaged natural science in general, because all natural science is compromised when it is betrayed in any one area. Increasingly, deference yielded to the recognition that long–range interests of the culture were ill–served by delays in critical scientific progress needed immediately. Part of behaviorology's coming of age was simply that behaviorologists, in exhibiting professional courtesy, would begin to avoid doing so in ways that were once a harmful but necessary aspect of the abandoned strategy to fit into, and to change, the cultural megalith of organized psychology.

Besides, for most people who were affected by this issue, normal collegial relations within universities already afforded a more appropriate paradigm for collegial courtesy. Universities exist within the culture in part to test the efficacy of grand ideas over long periods of time. No

guarantee exists, nor should exist, that all such ideas are mutually compatible. Pretenses to false compatibility by persons who represent different and antithetical disciplines do not respect the founding principles of academic institutions. Given the cultural mission of the academy on behalf of the culture that supports it, both parties have a right to be there. They can respect each other not for the quality of their respective ideas but in mutual recognition of their respective participation in fulfilling the purposes of the academy. That is the *only* valid basis for respect between disparate intellectual traditions. It conserves the integrity of each party because it is a respect based on cooperation, not integration.

In the prevailing view among behaviorologists, organized psychology has long been deemed too big and politically powerful to be changed from within by anything that behaviorologists might do at its scientific debating round–tables, especially since many non–scientific factors contribute to the organizational integrity of psychology in the first place. Those mainstream psychologists who are distressed by any perceived threat of potential competition (whether real or not) presumably would be pleased to see behaviorological revolutionaries expend themselves in that effort. And why not? Psychology is not only organized to contain or co–opt such efforts but also to train multitudes of traditional cognitivists for each behaviorally inclined person produced. Fortunately for behaviorologists, their movement has rendered that whole approach as unnecessary as it is futile.

The tiny behavioral sub–community operating within organized psychology has produced a limited number of behaviorally trained and skilled people. To distinguish between this somewhat estranged minority of behavioral scientists and the vast remainder of people working in organized psychology, authors usually denote that minority in psychology by calling them "behavioral" psychologists. Conversely, behavioral authors have attached adjectives such as "mainline," "mainstream," "traditional," "cognitive," and "mentalistic" when referring to members of the majority. But the behaviorology movement was in part born out of the conviction that remaining in psychology and calling oneself a psychologist, while denoting oneself as "behavioral," was inappropriate. Some, however, have had to continue to do that, at least temporarily, if for no other than the practical reason of remaining employed.

Those natural scientists of behavior, particularly the radical behaviorist "Skinnerians" who have struggled to maintain a disciplinary home base within organized psychology, have nevertheless maintained a literary isolation from mainstream psychology (which Krantz described in detail). That was done largely as a matter of scientific necessity. Coleman and Mehlman (1992) found that "the contrasting self–citation and mutual–citation practices that were reported by Krantz …have largely persisted in the 20 years from 1970 to 1989" (pp. 47–48). Yet, at the same time, on the political front the behaviorists in psychology had to work at cross purposes to their scientific selves by redoubling their communication efforts with the psychological establishment. As a typical example the final general session concluding the 1992 ABA convention, entitled "Science Agenda for the 1990s," featured the assembled presidents and science officers of the American Educational Research Association (an organization in which traditional psychology prevails), ABA, APA, APS, and, AABT (see the Eighteenth ABA Annual Convention Program, p. 164).

People could not validly be criticized for the histories that brought them to a behavioral view, nor for circumstances that led some to continue viewing organized psychology as the appropriate setting in which to pursue studies of behavior. But this protracted professional experiment—in the pattern of the protracted political experiment of Nationalist China—in which some behavioral psychologists isolated themselves *as psychologists* off the coast of mainland Psychology and claimed to *be* the only real psychology, appeared to the behaviorologists to be doomed. In *this* case however, behaviorologists believed that reabsorption into mainstream psychology would hurt the culture by significantly contaminating and warping their science. Not only would the extent to which that science need be taken into serious account by mainline psychologists be diminished, but the capacity of the resulting compromised residual version of that science to provide its contributions to the culture would be diminished also.

Summary of Chapter Five

Chapter Five reviewed the cultural milieu and analyzed the support for, and the opposition to, the growing behaviorology movement as the community at large witnessed its coalescence on the scientific professional scene. It examined some of the self–management problems facing those who were taking the lead in forming a verbal community around the behaviorology discipline. And it described and analyzed their actions and interactions on disciplinary issues.

As the behaviorology movement materialized as an accomplished fact, the contingencies on members of the behavioral community at large shifted subtly in important ways. The period during which hypothetical issues were posited for harmless conceptual analyses gave way to a new era featuring personal confrontations with an issue that required a career decision. The objections of resisters now focused more on *practical* issues. But with an alternative to affiliating with the verbal community of psychology now available, people found as ever more onerous their having to accept their disciplinary citizenship being policed by

mainline psychologists as the price to remain in psychology. While some behavioral people shifted to the behaviorology movement, others flirted with that movement and still others renewed their resignation to abide.

The behaviorologists eschewed intense recruitment drives and worked to consolidate and further establish their organized verbal community. Leaders debated alternatives about how best to do that and how to relate to outsiders. They fought to a draw with the behavioral psychologists for Skinner's final endorsement regarding proprietorship of the intellectual property. During this same period the literature of the "behavior analysis" movement slipped increasingly under the influence and control of organized psychology.

Increasingly, with respect to interdisciplinary relations, the behaviorologists recast old problem definitions and old perspectives: They no longer accepted some of the old challenges—and they discerned new ones. Their values were changing. Their mission was changing.

The next chapter, Chapter Six ("Interdisciplinary Context: A Cultural Role for the New Discipline," presented integrally with the conclusion Chapter Seven and the endnotes and complete references) will examine the prevailing views of the early behaviorologists on where their discipline fit both among the community of natural science disciplines extant in the culture and in the cultural marketplace. It will also comparatively explore the different levels of analysis characteristic of behavior–related natural science disciplines, and examine cultural resistance to behaviorology. ❧

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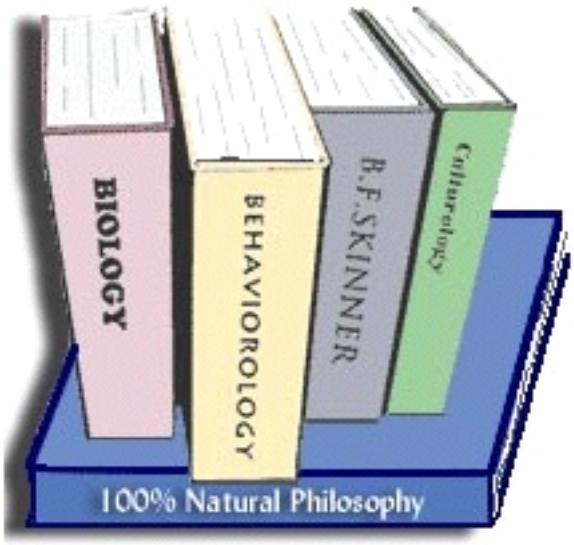

Behaviorology, Death, & Life

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[This is another topical excerpt from "Person, Life, and Culture," a later chapter of the author's book, *General Behaviorology: The Natural Science of Human Behavior* (Fraley, in press). Given its relevance to improvements in ongoing cultural concerns, readers of this journal may find it pertinent.—Ed.]

Defining *person* in terms of behavior has implications for the interpretation of dying. From the behaviorological perspective, dying is defined in terms of loss of the capacity to behave. Three kinds of dying have been delineated,[1] and will be discussed in this section.

How the Behaviorological Definition of a Person Affects Concepts of Death

First, consider the abrupt termination of behavior that is characteristic of the kind of sudden death experienced by victims of extreme trauma or acute disease (e.g., sudden death by gunshot or fatal heart attack). Abruptly, the person ends, and the body begins an irreversible differential biological dying across a relatively short and generally uninteresting interval. For convenience, an arbitrary time of death may be declared, which often coincides with the moment of the catastrophic event or with the discovery that the general physiological operations of the body can no longer be sustained (e.g., when it is recognized that the blood circulation has ceased irreversibly).

However, various parts of the body, isolated following the failure of the greater organic system upon which their vital maintenance has depended, continue independently to maintain their physiological functioning across different respective intervals. With body parts (and the individual cells of which they are composed) dying individually, *body death* is thus differential as those isolated body parts independently exhibit the dying process (the so–called *dying process* is actually a cessation of the ongoing physiological processes that define the status of living). Such a differential dying makes possible the harvesting of still viable organs from behaviorally dead persons so that those parts can be transplanted into the bodily systems of others.

Although the behaviorally defined person may be dead, the respondent behaviors necessary for a certain level of biological maintenance of the whole body may continue, as in the case of a permanently comatose indi-

vidual whose respiration, circulation, and other critical maintenance functions persist. If the respondent behaviors that drive those respective critical maintenance functions fail, at least some of those functions can be maintained by externally imposed interventions such as the use of a respirator. The behaviorally defined *person* will have been permanently ended insofar as the capacity for behaving operantly has been lost, while the body that formerly mediated the person remains biologically alive, perhaps with critical physiological functions continuing only with supplementation from external sources.

Thus, because body death involves the cessation of physiological functions, body death is different in nature from *person death,* which involves the termination of *behavioral* functions. Obviously, person death can occur without the concomitant biological death of the body. That is, the body can remain alive after its capacity to exhibit person–defining classes of behavior has been lost, and that is a common occurrence

Final and total *person death* coincides with the irreversible cessation of all *operant* behavior plus the failure of conditioned respondent behavioral relations—that is, with the loss of the capacity to exhibit the behavior that has been conditioned during the person's lifetime. In cases of slow dying, often marked by the creeping progress of an eventually fatal disease, that loss of previously conditioned behavioral capacity occurs incrementally and may span a relatively long interval. That protracted erosion defines an interval during which the former *person* is progressively diminishing.

Such an erosion of person–defining behavior can occur for two main reasons. First, the contingencies that control person–defining behavior may weaken so that the frequency of the behavior decreases, perhaps to zero. The body is still capable of producing the behavior, but certain behavioral manifestations of the person become less frequent due to changes in the contingencies to behave in those ways. This is a prime characteristic of slow dying, and social contingencies are often involved. A simple example is provided when slowly dying people who have always closely followed news reports of world and local events gradually stop doing so. As evidence mounts that the dying person will not be a part of the future that is implicit in those reports, the effect of that evidence on the dying person is a proportional weakening of the contingencies to attend to those reports.

Second, during the course of a slow–death episode, the physiological capacity of the body to produce operant behaviors may diminish incrementally as a disease progresses. These failures tend to occur differentially across different bodily systems so that the capacities of the whole body to behave in various ways are lost at different times—a progressive erosion of the behavioral repertoire that reflects progressive structural failures throughout the body.

A disease that eventually affects various parts of the bodily system often begins in a single subsystem. For example, Alzheimer's disease, which destroys brain cells, results in the differential dying of a *person* over a long period of time as the capacity to behave under partial control of neural behavioral supplements is slowly and irreversibly lost, one nerve cell at a time. Other diseases affect muscles so as to render them progressively incapable of being innervated. In some cases the slow and progressive losses of the capacity to behave in certain ways tend to go unnoticed until the person comes under contingencies to behave in some way that the relevant body parts can no longer exhibit.

The behaviorological definition of *person* also has implications that manifest in the course of normal living. Because a *person* is essentially the operant behavioral repertoire that the elements of a general environment can evoke, a normal ongoing *person* necessarily always re-

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[2] Reference to *the meaning **of*** something is such a common feature of ordinary discourse that it occasionally has been allowed to occur in some passages within this book to facilitate communication. Note, however, that *the meaning **of** something* is not actually a reference to any of its intrinsic properties but is instead a reference to how it affects behavior. For instance, to say that the word *rapid* means *quick* is to predict that a mediator will exhibit the same behavior in response to each of those verbal stimuli and to imply that a mediator would behave as if the statement *"The private's salute was both quick and rapid"* included a redundancy. However, like most pairs of synonyms, the two distinct terms remain extant because they sometimes occur under differing respective antecedent controls. While *quick* and *rapid* both signify smallness (which accounts for their synonymy), *quick* can pertain to a latency while *rapid* can pertain to a duration. To speak agentially, a listener who so interprets the statement about the salute would infer that the private, upon contact with the stimulus, did not hesitate long before saluting and then produced a swift salute. Importantly, that listener's public responding would be discriminative with respect to the terms *quick* and *rapid*, which we would then insist *did not mean the same thing to that person.*

[3] In the context of this paragraph, changes that result in more or less behavior (and hence more or less of a behaviorally defined person) do not necessarily correspond respectively to a better or worse person. For instance, in a given situation the more discriminative person may be deemed the better person, although a less discriminative person may be exhibiting a wider range of response types (most of which may be inappropriate).

mains in a state of flux, waxing and waning with gains and losses in operant behavioral capacity. Many of those gains and losses among the operant repertoire are deemed to represent the normal outcomes of ordinary experience. Normal processes such as extinction, punishment, and forgetting, which diminish behavioral capacity, thereby diminish a *person*, while the process of reinforcement, which enlarges behavioral capacity, expands a *person*. With respect to respondent behavioral processes, the generalization process broadens the range of controls on a given behavior, thus in a sense expanding the behaviorally defined person, while the discrimination process, in narrowing the range of such controls, tends, in the corresponding sense, to contract the behaviorally defined person. A behaviorological analysis of *person* thus lends new meaning[2] to the adage *"People change!"*[3]

A behaviorally defined person, being mediated by a body that while alive is a dynamic system, is therefore never fixed, whether gauged qualitatively or quantitatively. Person fixation comes only with person–death and then only at a scale value of zero.

The *quality* of a person inheres in the efficiency, effectiveness, and appropriateness of the individual's behavioral outcomes, and a person may be regarded as ffbehavior through which those outcomes are produced. From any given perspective, some behavioral outcomes are worthwhile and some are not, so from that perspective some facets of a *person* represent behavioral capacities the loss of which is deemed to be beneficial, while other facets of the person represent capacities for beneficial outcomes, an enlargement of which would be deemed advantageous.

Social death represents a third kind of dying (in addition to *body death* and *person death*). It is actually a subclass of person death, but its importance contributes to its special categorization. Social death is marked by the breakdown of the contingencies under which various classes of social relations have been maintained. It occurs in stages as the impending end of life alters the contingencies under which one behaves with respect to other people. Also, the body of a slowly dying individual may be experiencing a progressive physiological failure, and the erosion of its overall capacity to exhibit its operant repertoire will, in time, inevitably affect the social aspects of what is left of that person.

Eventually, for a slowly dying individual, these two change factors (changes in contingencies and changes in physiological structure of the body), separately or collectively result in a reduction in the behaviors that characterize each kind of social relation in which the person has been involved. Among such affected relations are business and professional relations as well as the social relations that define friendships, family ties, and one's most intimate interactions with loved ones. A time is reached beyond which each such class of social relations is no longer sustained in the traditional ways if at all. The eventual termination of *all* such relations, respectively due either to accumulating losses of physiological capacity or losses of the opportunities to exhibit the kinds of behaviors that define those relations, represents the final social death of a whole person. A slowly dying person experiences a partial social death for each such relation that, in turn, comes to an end, often to the dismay of the other people who have been involved in those progressively extinguishing relations.

Theoretically, given its nature, social death may precede person death. Thus, a slowly dying person can experience a progressive social isolation. The final stage of person–death is often a period of extreme loneliness that cannot necessarily be mitigated by the mere presence of previously close associates. Such a withering of social relations, even in the presence of those with whom they have been well established, can occur (a) due to progressive loss of effectiveness by the antecedent and consequating stimuli in the previously effective social contingencies or (b) the body's progressive loss of capacity to further mediate the social behavior. This accounts for the practicality of the widespread practice of involved parties saying their good–byes in the context of their traditional social contingencies before the progress of social death deprives the dying person of the capacity to do so.

The common allusive phrase …*as lonesome as dying* (as in "trekking cross–country alone can feel as lonesome as dying") connotes the widespread intuitive grasp of this reality. The urgency with which close others "being there at the *very* end" is touted may connote more its therapeutic worth for a mourner than for a prospective decedent, although there is normally a preceding interval during which those eroding social relations still have some functional integrity, …an interval during which the dying person can still contact at least some of the potential social reinforcers that are being provided by those in company. It is during that penultimate interval that a dying person may utter the classic plea "…please stay with me till the end" (although, as this chapter makes clear, *end* can become quite ambiguous). Across different cases, the intervals that are being discussed here may differ in duration …lasting seconds, minutes, hours, days, or even longer.

A Summary of Life and Death

Although on a microscale the distinction between matter and energy tends to disappear, on a macroscale, matter and energy manifest respectively as structure and process. In that regard, the related concepts of matter (as structure) and energy (as process) provide an analogy to the related concepts of body and life.

Matter, existing in some structural form, may receive additional energy, and its effect may be reflected as change in process exhibited by that structure. That is,

energy transferred to structure, unless stored in a potential form as more structure, can result in a new state that is detected as increased process. Process is thus something that is *happening* to structure in some relation to the energy that the structure receives or loses. If mechanical energy is transferred to a swinging pendulum, the pendulum swings through a greater arc. An input of thermal energy manifests as increased molecular agitation.

Organic bodies are structures, and what we call "life" consists of classes of processes that are exhibited by those structures. One large class among the processes that collectively represent organic "life" works mainly through nutritional biophysical and biochemical functions that maintain specific bodily structures. Body substructures (such as hearts, for example) are thereby kept within a structurally defined range that can support the kind of respondent behavioral processes (e.g., heartbeats) that contribute to one kind of definition of biological "life." The kinds of processes that maintain the structural integrity of body parts, plus the kinds of behavioral interactions of those parts that endow bodies with their systemic nature, constitute the main subject matter of physiology. Collectively, those processes are said to pertain to the internal economy of an organic body.

Another class of life processes is often described as the "behaviors" exhibited *by* that body, and, in that context, the sociocultural behaviors that concern behavior scientists are mostly operant. The behaviors in that class (a) are stimulated (i.e., triggered) by events in the behavior–controlling environment, many external to the body, (b) tend, in turn, to have effects on that environment, and (c) are finally rendered more or less probable by micro–structural changes to the body that occur in reaction to those behavior–produced environmental changes (the term *reaction* alludes to the effect of an energy transmittal back to the body from the behavior–altered environment, alterations to the environment that are categorized as the *consequences* of that behavior). Thus, the (c)–part of the overall process is known as *consequation*. While (a), (b), and (c) characterize the total process of operant conditioning, it is (c) that represents its essence via some neural microstructuring insofar as energy returning to the body from the behavior–altered environment results in some neural microrestructuring that alters the future evocability of the initial behavior.

Life (a compound process) begins when the developing structure of the body gains the capacity to respond with the kind of processes that are evidence of a state of living. In natural organic reproduction, the parentally contributed materials are already endowed with certain primitive life–defining processes, so "life" (as defined by function and process) remains continuous across episodes of organic reproduction. Although life is thus continuous, the degree and diversity of its manifestations undergo variation across successive generations.

A technological procedure to originate life from a point in time without the historical continuity of biological reproduction would require only that the appropriate structures be produced and energized within the necessary ranges. With such a structural capacity for life having been created, given contact with the kind of environmental events that control a given life process, that process would occur inevitably to that structure.

One theoretical implication is that, if, in a laboratory, we constructed a body to the precise structural specifications of a live model and endowed it with corresponding kinds and amounts of energy, the constructed copy would also be coming alive as the critical aspects of the construction were completed—meaning nothing more than that certain processes that are definitive of life could and would begin as soon as their appropriately energized capacitating structural elements were in place with respect to one another and with respect to a supportive environment. The life processes would then occur automatically in the same sense that a marble, released six feet above the floor, would automatically begin to accelerate downward. It is simply what starts to happen given the necessary conditions.

The complexity of the bodily structure of a living organism, plus the complexity of the energizing that is involved in its invigoration together have tended to discourage attempts to synthesize a living organism in a laboratory, especially a more complex type of organism that would be of interest to most people. Currently, however, certain biologists are working diligently to synthesize a simple primitive form of living matter. Most of this work seeks to duplicate in laboratories the conditions that are believed to have prevailed on this planet when the first life presumably emerged via natural processes. Presumably, if those primordial conditions can be sufficiently approximated, primitive life will emerge—this time, under controlled conditions in a laboratory. Another theory suggests that the first life on this planet was carried here in bombarding asteroids. If true, this could complicate efforts to duplicate the initiation of life functions, but it does not render the successful outcome absolutely impossible. In any case, scientists who are familiar with such lines of research now predict that primitive forms of life will soon be created in a scientifically controlled way.

Modern biotechnologists may not yet synthesize equivalents of the natural outcomes of a course of biological evolution that has steadily continued across the entire biological history of this planet. However, while the complexity of nature defines the challenges faced by scientists, no degree of naturally produced complexity rises to the level of an absolute restriction on scientific

progress toward the explication and control of the natural processes that define life.

That holds true with regard to the problem of synthesizing copies of more complex natural products, including a living and fully functional human being. After all, the definitive characteristics of any person have a structural basis. Let us imagine some relevant events that lie beyond the range of current scientific and technological capacity. Suppose, for instance, that a future team of scientists in a laboratory synthesized a fully nourished human body whose parameters fall within the range of a normal living thirty–seven year old man or woman. Because the behavioral experiences of a body are reflected in a microstructural kind of record, such a project would have involved the synthesis of the microstructuring that would correspond to a particular behavioral history that had never actually occurred to that newly constructed body.

For instance, a newly created and *exact* duplicate of you, as you are structured and energized at this instant, given the appropriate evocative stimulation, would be capable of describing the details of your seventh birthday party to the same extent that you are now prepared to do so in response to the same kind of stimulation. Such an accomplishment may remain indefinitely impracticable, impractical, or both, but impracticality does not necessarily invalidate theoretical possibility.

Death of the body as a whole (and hence, necessarily, of the person), occurs with the cessation of the interactive processes that define the systemic aspects of body life. A state of body death ensues when certain systemic processes can no longer happen just as earlier, and for equally natural reasons, the attainment of the capacities for those same processes insured the start of the processes that contributed to the *life* of that body. The concept of the start and end of a given life process gives rise to no ontological implications in the sense of requisite managerial or agential entities beyond whatever bodily structure supports the manifestation of that process. In terms of process, which is the essence of life, to die is merely for certain naturally occurring processes to cease, because the structural capacity to exhibit them has been lost or the necessary supportive energy inputs (either from internal reserves or from more remote parts of the environment) have stopped.

Given a bodily structure that is necessary to mediate a specific life process, that process of bodily life is subject only to functional control, and the proximal independent variables are the energy inputs to that body structure. Those energy inputs may be relatively minuscule and merely trigger the release of potential energy to support the life process, or the energy inputs may represent a larger part of the necessary energy for that body part to operate in way that endows it with an alive status.

The total body, considered as a structure, can *live* in the sense that (a) its structural subsystems are being maintained and (b) the interrelations among them continue to occur thus insuring the vital integrity of the body as a biological entity. Some aspects of those interrelations among bodily systems are supported by respondent kinds of behavior that directly affect only the internal economy of the organism (e.g., heartbeats, routine breathing, various endocrine functions, etc.). The evidence that such a *body* is alive need not include operant behavior that directly affects the external environment (i.e., the behaviors that characterize a *person*).

Thus, a body may remain biologically alive although it may lack the operant capacity as well as much of the respondent capacity to interact behaviorally with the outside environment (e.g., a deeply comatose individual). Such a body is biologically alive but person–dead. If, on the other hand, the structural capacity of the body for functional relations between the external environment and the body has been maintained, especially for operant kinds of responding, then the individual is prepared to interact behaviorally with its external environment in ways that define a *person*. However, if a behavior is not stimulated it does not occur even though the structure may be in place to support its manifestation. That is, behavior may fail to occur for lack of stimulation as well as for lack of the structure required for its occurrence. Person death for either kind of reason is therefore possible.

Person death that occurs merely for lack of stimulation implies the theoretical possibility of a special kind of dying. That idea has been exploited in theoretical proposals for a prison in which person–death is approximated through stimulus–starvation while body life is maintained. Procedurally, the external environment is contrived to be stimulus free, although the body is kept alive and maintained in a state that remains capable of reacting to its external environment (usually called a *conscious state*). However, in such a procedure of extreme preclusion, an external environment is created that minimizes behavior–producing energy transfers of any kind from environment to body. With the environment arranged to be as stimulus–free as possible, little if any behavior is elicited or evoked. In proportion to the extent to which the environment thus loses its definition, the behaviorally defined sociocultural *person* is precluded from manifesting as such, although the structural capacity of the body to do so is extant.

In one version, the body is suspended in darkness within a fluid having a temperature matched to that of the individual's body, while air that is at body temperature is silently supplied by tube to a face mask. The body is bound by soft restrains that gently prevent the movement of its parts relative to one another. Nutrients are

supplied intravenously, and waste is removed in similarly unstimulating ways.

Under such an arrangement, most of the behavior that defines the person cannot manifest for lack of the necessary stimulation. Some sequences of private verbal behavior may continue. However, without even occasional links to the outside, those private events would remain largely and indefinitely divorced from the reality of the external environment. That kind of isolation would leave such private neural behavior prone to the increasingly deviant forms that characterize bizarre hallucinations. For a time, this kind of person death retains the theoretical possibility of some degree of resurrection through a restoration of environmental stimulation, although unexercised body parts cannot indefinitely retain their respective capacities to mediate behavior.

The Abstract Person

The qualitative differential that inheres in the behaviorologically defined person (to which most everyone hews in proportion to the practicality of their specific situations) confronts people with the issue of the relative worth of any given person at any given time. Regardless of the validity of variance in the worth of a person, that concept tends to be fraught with disruptive implications for the kinds of social relations that have generally been conditioned within human culture.

While the powerful contingencies of practicality compel adherence behavior with respect to the relative worth of persons as behaviorologically defined, the avoidance of many troublesome social implications has been accomplished through the emergence of an abstract class into which all biologically alive human bodies are conceptually posited and declared by rule to be of equal worth. People refer to a member of that abstract class as a *person* or a *human being*, and laws are adopted that protect those abstract beings by granting them certain rights.

Practical contradictions are then handled through defined exceptions. Those whose conduct matches such a definition are transferred conceptually to an excepted class and thus rendered susceptible to treatment in an unpersonlike or inhumane way. For instance, a person whose social conduct is intolerable in certain specified ways may be categorized as a criminal, …an abstract classification that, under the auspices of the state, qualifies the individual to be stored in a cage or killed according to the ritualized ceremony of execution. Another example features the maintenance of a live body that can no longer mediate a behavioral person and is very unlikely ever again to do so. The responsibility for that maintenance may be transferred legally to the management of a party who is entitled to terminate life support for the person–dead body under what is usually a combination of economic and social contingencies.

Such conceptual devices of exception and exclusion, and the sociocultural arrangements that follow as their implications, permit people to ignore the universal classification of abstract *person* or *human being* and instead to behave toward a specific individual in relation to the behavioral worth of that individual as dictated by practical contingencies. These sociocultural devices seemingly mitigate some of the troublesome intrinsic relativity in the behaviorally established worth of a person by creating a bifurcation of status. The individual remains a person or human being in the abstract while concurrently being consigned to a practical category to which specially contrived procedures are then exclusively applicable. Thus, at the moment that a jury announces a guilty verdict, the defendant may remain a human being in the abstract while undergoing an instantaneous change in practical social status. Under the umbrella of such a conceptual bifurcation of the abstract and the practical, people can continue to speak as if a convicted murderer retains an equal "personhood" (in the abstract) yet in the practical domain treat that individual as behaviorally threatening and worth relatively little. That treatment may involve prolonged isolation through incarceration or quick and permanent riddance through the infliction of extreme trauma.

In cases where less is at stake, informal conventions accomplish the same dichotomy between the abstraction of one's equal personhood and the realities of one's relative worth in practical situations. Given two applicants for the same occupational position, the relative worth of each individual's relevant behavioral repertoire is the criterion for selection. In such practical situations no one expects their equality, as endowed through their abstract personhood, to count for anything among the selection criteria, although it may be cited as the basis for any individual's opportunity to be considered for the position. Nevertheless, the exploitation of such an opportunity is by way of exhibiting the behaviors of self–presentation, and those behaviors are subject to evaluation in terms of relative worth. An allusion to that reality inheres in statements about a person making or not making a good first impression.….☘

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Coercion: The Real Parent Trap Part 1 (of 2)

Glenn I. Latham

Utah State University Logan

[This is the first part of the first article in the first issue of Glenn Latham's *Parenting Prescriptions* magazine. As one of the four Founders of TIBI and a *Behaviorology Today* staff writer whose work has appeared in the pages of this journal before, Glenn had planned other submissions (before his unexpected death). So we are thankful to have received permission to occasionally reprint one of his helpful, science–based practical articles for parents and other child caregivers. (Readers can obtain all four issues of *Parenting Prescriptions* magazine through the "Products" section of www.parentingprescriptions.com which is the web site that Glenn established as an information resource.) The other part of this article (Part 2) will appear in the next issue (Volume 11, Number 2, Fall 2008).—Ed.]

Welcome to the first [article in] *Parenting Prescriptions,* a newsletter that provides proven formulas for parenting with love. We at *Parenting Prescriptions* have a vision. We have a clear view of where we are going, how we are going to get there, and what we hope to accomplish in the process. What we hope to accomplish is this: We want to help parents get coercion out of their homes and out of their relationships with their children.

We also want to help parents replace coercion with noncoercive parenting skills, or parenting prescriptions. Toward that end, this article explains what coercion is and gives some examples of how to replace coercion with noncoercive parenting.

Coercive Parenting: Parenting Skills that Stink

Coercive parenting is negative parenting, which attempts to compel children to act or to choose in a particular way. Coercive parenting puts children down, draws undue attention to children's weaknesses and failures, and leaves children feeling unsafe in their own home and family. Consider this example of a father using coercive parenting:

"What a terrible report card! You're no dummy. Why do you do this to yourself? Don't you get it? This is your life we are talking about. Fail in school and you fail at life. Don't be so stupid! Is this what I go to work every day for? To pay taxes to support your schooling, then have you throw it back in my face every nine weeks with a rotten report card? Get those grades up, Buddy, or you're dead meat!"

Does this tirade sound familiar? Too often, not only does it sound familiar, it is right on the mark. In fact, in this instance, it's a direct quote. (For examples of other negative parenting habits, see "Eight Common Coercive Behaviors" [the sidebar on page 38].)

Let's analyze this angry, negative, coercive outburst. Does the parent explain to the child what he was supposed to do? No. Does the parent say anything that would give the child a reason to do better? No. Does the parent say anything that would bond the parent to the child so that in the future the two can work out their differences in harmony and good will? No.

How do you think the child will respond to the parent's outburst? Is there any chance that the child will respond in the following manner?

"Thank you so much for being so direct and candid with me. Surely this is just the nudge I need to get off dead center and start moving ahead with my life. How could I have been so selfish? My, my, how I appreciate the long hours you put in at work, knowing you are doing it all for me. Well, my days of thoughtlessness and selfishness are over. And all because you care enough to take your valuable time to give me the tongue–lashing I need. What a parent. What a dear, dear parent!"

Let me ask again, is there any chance that the child would respond in this way?

In keeping with this brain–wrenching question, brace yourself for a tough multiple–choice question: What would the child most likely do?

A. Get away from his father
B. Stay away from his father
C. Get even with his father by getting worse grades next time
D. All of the above
E. None of the above

To help you answer this question, I am going to give you some important information about coercion. After all, it wouldn't be fair to ask a question about material that hasn't been covered. (Although this reminds me of the time one of my college professors told the class: "Don't worry if I don't cover all of the course material during class, because I will cover it in the exam.") This information will not only help you solve the multiple–choice question but will also help you solve more perplexing parenting problems in the future.

Coercion is like the smell of a skunk: We want to get away from it, and we want to stay away from it. But even worse, we want to get even with the animal (or person) responsible for it.

Dr. Murray Sidman has been a major contributor to basic and applied behavior analysis since 1952. ["Behavior analysis" is a name that has been used for naturalism–informed behavior science, especially during the period when such science and the non–natural science discipline of psychology shared their history; see Ledoux, 1997/2002.—Ed.] He [Sidman] is also the author of more than 100 scientific papers about behavior. In his book *Coercion and Its Fallout* [Sidman, 2001] Dr. Sidman effectively teaches that coercion makes a person want to escape (get away), avoid (stay away), and counter–coerce (get even). *Parenting Prescriptions* will teach you the skills you need to get coercion out of your homes and families because, in the long run, the two cannot coexist. Eventually, one has to go. Either coercion stays and the child goes, or coercion goes and the child stays. It's as simple as that.

Noncoercive Parenting: The Sweet Smell of Success

Now that you understand how destructive coercion can be, let's revisit the encounter between the parent and the child about the report card. This time, however, the parent uses noncoercive parenting skills.

Parent: "I'm sorry you chose to do things other than your schoolwork. What can you do to get the kind of grades you're capable of getting?"

Child: "I did the best I could. I get bad grades because my teachers don't like me. It wouldn't make any difference how hard I worked, I'd never get good grades. Besides, my classes are dumb. In fact, the only think dumber than my classes are my teachers."

Parent: "I can see that you have some strong feelings about your classes and your teachers. Still, what can you do to bring your grades up?"

Note: The parent does not respond to the child's comment about "dumb" teachers and "dumb" classes. These types of comments are called *behavioral noise,* and you should not pay any attention to such comments. (We'll discuss behavioral noise in later [articles from] *Parenting Prescriptions.*) Instead, the parent remains focused on what the child needs to do to get better grades.

Child: "I already told you, my teacher's hate me! No matter how hard I work, or how many assignments I hand in, I'll never get good grades."

Parent: "You just mentioned two really important things. Thanks. That's good. You mentioned hard work and handing in your assignments. Super. What else do you need to do to improve your grades?"

Note: Do you see how the parent focuses on what should be done, rather than on what wasn't done? This is the key to noncoercive parenting. Also, notice that the parent emphasizes what the child needs to do to improve his grades, rather than what the parent will do to the child if he doesn't improve his grades.

Child: "Look, I'll do my best. But don't hassle me if my grades don't come up. I can't make my teachers like me, you know."

Parent: "Great! I can't wait to see your next report card. Now let's go shoot some hoops."

I bet I know what you're thinking: "Yeah, fat chance, who are you trying to fool?" Well, it's no joke. This is virtually a work–for–word dialogue between a father I recently worked with and his 14–year–old son. The boy was failing all of his subjects when they had this discussion. Nine weeks later the boy's next report card was all As with one A– in algebra. More importantly, however, the boy's mother told me excitedly, "Best of all, (my son) and his dad are back together again. I would never have believed it."

When coercion is gone, when the relationship with the parent is safe, parents and their children come together. It is predictable. It is a law of human nature.…❧

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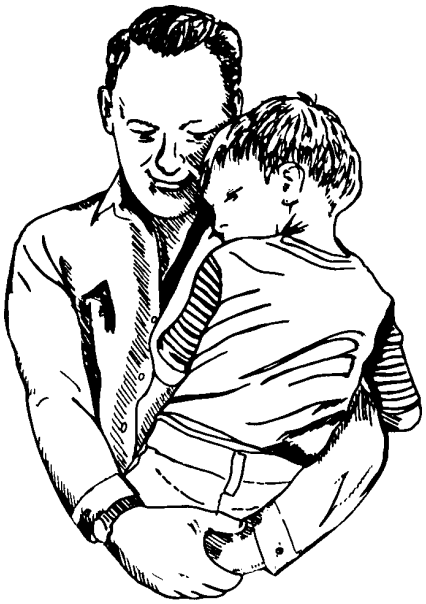


See the "Eight Common Coercive Behaviors" sidebar on the next page.…]

Eight Common Coercive Behaviors

Glenn Latham / *Parenting Prescriptions* sidebar (part of the *Coercion: The Real Parent Trap* article)❧

———— § ————

If parents use coercion to respond to children's inappropriate behavior, this coercion negatively affects the relationship between parents and children. In fact, coercive behavior divides parents from children. (By the way, these behaviors also divide students from teachers.)

Over the years, I have identified eight common coercive behaviors. I call these behaviors *parenting poison.*

❧ **Criticism** or finding fault with children

❧ **Sarcasm** or making fun of children or ridiculing children

❧ **Threats** or warning children that you will perform a hostile act if children don't quickly behave better

❧ **Questioning** or asking children to explain why they misbehaved

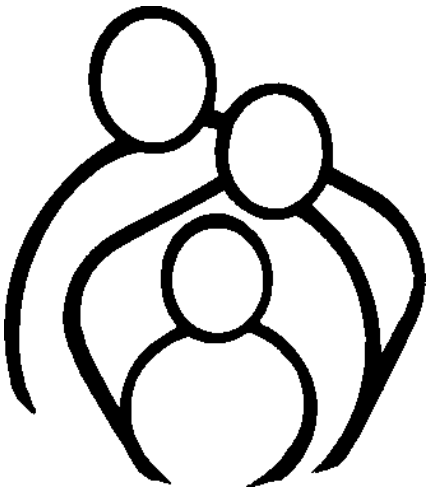
❧ **Logic** or trying to reason with children

❧ **Arguing** or trying to convince children that you are right and they are wrong

❧ **Physical or Verbal Force** or hitting or shouting at children to force them to behave

❧ **Despair** or feeling hopeless, beaten, and out of control

By avoiding these coercive behaviors, you can calmly respond to your children's inappropriate behavior. When you stay calm and understanding, your children will feel safe and will want to be close to you. In future [articles from] *Parenting Prescriptions,* I will discuss these behaviors in more detail.☙



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[See the listing in the last issue.—Ed.]☙

Syllabus Directory

Each issue of *Behaviorology Today* contains three lists. These lists show where to find only the most up–to–date versions (in title and content) of TIBI's course syllabi. The first list shows syllabi located in the current issue or past issues. The second list shows the schedule (which may change) of syllabi to appear in some future issues. The third list repeats the syllabi locations (actual or planned) but by course number rather than by issue.

Up–To–Date Syllabi in Current or Past Issues

Volume 7, Number 2 (Fall 2004): BEHG 101:
*Introduction to Behaviorology I.**
Volume 7, Number 2 (Fall 2004): BEHG 102:
*Introduction to Behaviorology II.**
Volume 7, Number 2 (Fall 2004): BEHG 201:
*Non–Coercive Child Rearing Principles and Practices.**
Volume 7, Number 2 (Fall 2004): BEHG 355:
*Verbal Behavior I.**
Volume 8, Number 1 (Spring 2005): BEHG 400:
Behaviorological Rehabilitation.
Volume 8, Number 1 (Spring 2005): BEHG 415:
*Basic Autism Intervention Methods.**
Volume 8, Number 1 (Spring 2005): BEHG 420:
*Performance Management and Preventing Workplace Violence.**
Volume 8, Number 1 (Spring 2005): BEHG 425:
*Non–Coercive Classroom Management and Preventing School Violence.**
Volume 8, Number 1 (Spring 2005): BEHG 475:
*Verbal Behavior II.**
Volume 8, Number 2 (Fall 2005): BEHG 410:
Behaviorological Thanatology and Dignified Dying.
Volume 9, Number 1 (Spring 2006): BEHG 365:
Advanced Behaviorology I.
Volume 9, Number 2 (Fall 2006): BEHG 470:
Advanced Behaviorology II.
Volume 10, Number 1 (Spring 2007): BEHG 120:
Non–Coercive Companion Animal Behavior Training.

Syllabi Planned for Future Issues

Volume ?, Number ? (Spring/Fall 20??): BEHG 250:
Educational Behaviorology for Education Consumers.
Volume ?, Number ? (Spring/Fall 20??): BEHG 340:
Educational Behaviorology for Education Providers.
Volume ?, Number ? (Spring/Fall 20??): BEHG 405:
Introduction to Instructional Practices in Educational Behaviorology.
Volume ?, Number ? (Spring/Fall 20??): BEHG 455:
Advanced Instructional Practices in Educational Behaviorology.
Volume ?, Number ? (Spring/Fall 20??): BEHG 445:
Advanced Experimental Behaviorology.

*An older version appeared in an earlier issue.

Syllabi Locations Listed by Course Number

BEHG 101: *Introduction to Behaviorology I:*
Volume 7, Number 2 (Fall 2004).
BEHG 102: *Introduction to Behaviorology II:*
Volume 7, Number 2 (Fall 2004).
BEHG 120: *Non–Coercive Companion Animal Behavior Training:*
Volume 10, Number 1 (Spring 2007).
BEHG 201: *Non–Coercive Child Rearing Principles and Practices:*
Volume 7, Number 2 (Fall 2004).
BEHG 250: *Educational Behaviorology for Education Consumers:*
Volume ?, Number ? (Spring/Fall 20??)
BEHG 340: *Educational Behaviorology for Education Providers:*
Volume ?, Number ? (Spring/Fall 20??)
BEHG 355: *Verbal Behavior I:*
Volume 7, Number 2 (Fall 2004).
BEHG 365: *Advanced Behaviorology I:*
Volume 9, Number 1 (Spring 2006).
BEHG 400: *Behaviorological Rehabilitation:*
Volume 8, Number 1 (Spring 2005).
BEHG 405: *Introduction to Instructional Practices in Educational Behaviorology:*
Volume ?, Number ? (Spring/Fall 20??)
BEHG 410: *Behaviorological Thanatology and Dignified Dying:*
Volume 8, Number 2 (Fall 2005).
BEHG 415: *Basic Autism Intervention Methods:*
Volume 8, Number 1 (Spring 2005).
BEHG 420: *Performance Management and Preventing Workplace Violence:*
Volume 8, Number 1 (Spring 2005).
BEHG 425: *Non–Coercive Classroom Management and Preventing School Violence:*
Volume 8, Number 1 (Spring 2005).
BEHG 445: *Advanced Experimental Behaviorology:*
Volume ?, Number ? (Spring/Fall 20??)
BEHG 455: *Advanced Instructional Practices in Educational Behaviorology:*
Volume ?, Number ? (Spring/Fall 20??)
BEHG 470: *Advanced Behaviorology II:*
Volume 9, Number 2 (Fall 2006).
BEHG 475: *Verbal Behavior II:*
Volume 8, Number 1 (Spring 2005).

Always More at behaviorology.org

Visit TIBI's web site (www.behaviorology.org) regularly. We are always adding and updating material.

From the *Welcome* screen, you can select the *Sample* page of our *Behaviorology Community Resources* (designed especially for first–time visitors). This page provides a wide selection of useful articles, many from *Behaviorology Today*, in *Adobe* PDF format (with a button to click for a free download of Adobe's Acrobat Reader software, although most computers already have it). The articles are organized on several topical category pages (e.g., contributions to parenting and education, book reviews, and behaviorology around the world). Other selections on the *Sample Community Resources* page feature descriptions of TIBI's certificate programs and course syllabi, and links to some very helpful related web sites.

From the *Welcome* screen or the *Sample Community Resources* page, you can also select the main page of the web site, the *Complete Behaviorology Community Resources* page. This page contains a more complete set of materials, including (a) more articles under the same selection categories as on the *Sample* page, (b) additional article selection categories (e.g., contributions to autism, natural science, outreach, and verbal behavior) each with its own range of pages and PDF materials, (c) many more links to related behavior science web sites, and (d) several new types of selections (e.g., books and magazines pages and PDFs, and upcoming activities).

Visit the web site regularly. After each new issue of *Behaviorology Today,* we link the issue's articles to the relevant selections and categories on the web site.

Explore what interests you. And tell us about your site–visit experience. Your input is welcome, and will help us make further imporvements.

As with any category of regular membership or Donor level, a paid online membership (US$5) earns and supports access to the greater amount of online material included on the *Complete Behaviorology Community Resources* page. (See *TIBIA Memberships & Benefits* in this issue.)

Subscriptions & Back Issues

People can receive copies of *Behaviorology Today* in ways other than as a member. People can subscribe without membership for US$20, and people can obtain back issues for US$10 each. Photocopy, fill out, and send in the "membership" form on a later page. As applicable, check the "subscription" box, and/or list which back issues you are ordering. *Donations/Contributions* are also welcome, and are tax–deductible as TIBI is non–profit (under 501–c–3).

While supplies last, new subscriptions—with or without a regular membership—will include a copy of each past issue of *Behaviorology Today,* beginning with Volume 5, Number 1, (Spring 2002).

TIBIA Memberships & Benefits

The levels of TIBIA membership include increasing amounts of basic benefits. Here are all the membership levels and their associated, basic benefits:

Free–online membership. Online visitors (who may or may not elect to register online as a free *member)* receive benefits that include these: (a) access to selected, general interest *Behaviorology Today* articles and links, (b) access to Institute information regarding TIBI Certificates and course syllabi, and (c) access to previews of the benefits of other membership levels.

$5 (to $19) *Basic–online membership.* Online visitors who pay the $5 online dues earn benefits that include these: *All* the benefits from the previous membership level plus (a) access to all *Behaviorology Today* articles and links online, (b) access to TIBIA member contact information online, and (c) access to special organizational activities (e.g., invitations to attend TIBI conferences, conventions, workshops, etc.).

$20 (to $39) *Subscription membership.* Those who mail in (by regular post) the $20 subscription fee and form receive benefits that include these: *All* the benefits from the previous levels plus a subscription to the paper–printed issues of *Behaviorology Today* (ISSN 1536–6669).

Contribution amounts beyond these first three levels are *Donor* levels, which are described in *TIBI Donors & Levels* in this issue. All memberships are per year. The next four membership levels (Student, Affiliate, Associate, and Advocate) were the Institute's original membership categories, and so are sometimes designated the "regular" membership levels. Here are these regular membership levels and their basic benefits:

$20 *Behaviorology Student membership* (requires paper membership application co–signed by advisor or department

chair, and dues payment—see *TIBIA Membership Criteria & Costs* in this issue). Benefits include *all* those from the previous levels plus these: Access to all organizational activities (e.g., invitations to attend and *participate* in meetings conferences, conventions, workshops, etc.).

$40 *Affiliate membership* (requires paper membership application, and dues payment—see *TIBIA Membership Criteria & Costs* in this issue). Benefits include *all* those from the previous levels plus these: Access to advanced levels for those acquiring the additional qualifications that come from pursuing a professional behaviorology track.

$60 *Associate membership* (requires paper membership application, and dues payment, and is only available to qualifying individuals—see *TIBIA Membership Criteria & Costs* in this issue). Benefits include *all* those from the previous levels plus these: TIBIA voting rights.

$80 *Advocate membership* (requires paper membership application, and dues payment, and is only available to qualifying individuals—see *TIBIA Membership Criteria & Costs* in this issue). Benefits include *all* those from the previous levels plus these: May be elected to hold TIBIA or TIBI office.

Other Benefits

Beyond the intrinsic value that TIBIA membership bestows by virtue of making the member a contributing part of an organization helping to extend and disseminate the findings and applications of the natural science of behavior for the benefit of humanity, and beyond the benefit of receiving the organization's publications, TIBIA membership benefits include the following:

- Members will have opportunities to present papers, posters, and demonstrations, etc., at the organization's meetings;
- Members paying regular dues in the last third of the calendar year will be considered as members through the end of the following calendar year;
- Members paying regular dues in the middle third of the calendar year will be allowed to pay one–half the regular dues for the following calendar year;
- A TIBIA member may request the Institute to evaluate his or her credentials to ascertain which TIBI certificate level most accurately reflects the work (and so, by implication, the repertoire) behind those credentials. The Institute will then grant that certificate to the member; as part of this evaluation, the Institute will also describe what work needs to be accomplished to reach the next certificate level. The normal processing fee for this service (US$20) will be waived for members. For the processing fee of US$20, a non–member may also request this evaluation and, should she or he ever join TIBIA, the US$20 already paid will be applied to the initial membership dues owed. (Faculty teaching behaviorology courses can encourage their students to request this evaluation.)

TIBIA continuously considers additional membership benefits. Future iterations of this column will report all new benefits upon their approval.

TIBIA Membership Criteria & Costs

TIBIA has four categories of regular membership, of which two are non–voting and two are voting. The two non–voting categories are Student and Affiliate. The two voting categories are Associate and Advocate. All new members are admitted provisionally to TIBIA at the appropriate membership level. Advocate members consider each provisional member and then vote on whether to elect each provisional member to the full status of her or his membership level or to accept the provisional member at a different membership level.

Admission to TIBIA in the Student membership category shall remain open to all persons who are undergraduate or graduate students who have not yet attained a doctoral level degree in behaviorology or in an acceptably appropriate area.

Admission to TIBIA in the Affiliate membership category shall remain open to all persons who wish to maintain contact with the organization, receive its publications, and go to its meetings, but who are not students and who may not have attained any graduate degree in behaviorology or in an acceptably appropriate area. On the basis of having earned TIBI Certificates, Affiliate members may nominate themselves, or may be invited by the TIBI Board of Directors or Faculty, to apply for an Associate membership.

Admission to TIBIA in the Associate membership category shall remain open to all persons who are not students, who document a behaviorological repertoire at or above the masters level or who have attained at least a masters level degree in behaviorology or in an acceptably appropriate area, and who maintain the good record—typical of "early–career" professionals—of professional accomplishments of a behaviorological nature that support the integrity of the organized, independent discipline of behaviorology including its organizational manifestations such as TIBI and TIBIA. On the basis either of documenting a behaviorological repertoire at the doctoral level or of completing a doctoral level degree in behaviorology or in an acceptably appropriate area, an Associate member may apply for membership as an Advocate.

Admission to TIBIA in the Advocate membership category shall remain open to all persons who are not stu-

dents, who document a behaviorological repertoire at the doctoral level or who have attained a doctoral level degree in behaviorology or in an acceptably appropriate area, who maintain a good record of professional accomplishments of a behaviorological nature, and who demonstrate a significant history—typical of experienced professionals—of work supporting the integrity of the organized, independent discipline of behaviorology including its organizational manifestations such as TIBI and TIBIA.

For all regular membership levels, prospective members need to complete the membership application form and pay the appropriate annual dues.

Establishing the annual dues structure for the different membership categories takes partially into account, by means of percentages of annual income, the differences in income levels and currency values among the world's various countries. Thus, the annual dues for each membership (or other) category are:

Category	Dues (in US dollars)*
Board of Directors member	The lesser of 0.6% of annual income, or $120.00
Faculty member	The lesser of 0.5% of annual income, or $100.00
Advocate member	The lesser of 0.4% of annual income, or $80.00
Associate member	The lesser of 0.3% of annual income, or $60.00
Affiliate member	The lesser of 0.2% of annual income, or $40.00
Student member	The lesser of 0.1% of annual income, or $20.00

*Minimums: $20 director or faculty; $10 others

TIBIA MEMBERSHIP APPLICATION FORM

(SEE THE NEXT PAGE FOR THE TIBI / TIBIA PURPOSES.)

Copy and complete this form (please type or print)—*for membership* ***or*** *contributions* ***or*** *subscriptions* ***or*** *back issues*—then send it with your check (made payable to TIBIA) to the TIBIA treasurer at this address:

Dr. Stephen Ledoux
TIBIA Treasurer
SUNY–CTC
34 Cornell Drive
Canton NY 13617 USA

Check if applies:
Contribution: ☐
Subscription:* ☐
Back issues:* ☐
❧ Vol. ___, #___
❧ Vol. ___, #___

Name: _____

Office Address: _____

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Office Phone #: _____

FAX #: _____

E-mail: _____

Degree/Institution:** _____

Member Category: ☐

Amount enclosed: US$ _____

Home Address: _____

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Home Phone #: _____

CHECK PREFERRED MAILING ADDRESS:

Office: ☐ Home: ☐

Sign & Date: _____

*Subscriptions: US$20/year; back issues: US$10 each.

**For Student Membership:
I verify that the above person is enrolled as a student at: _____

Name & Signature of Advisor or Dept. Chair: _____

*TIBI / TIBIA Purposes**

TIBI, as a non–profit educational corporation, is dedicated to many concerns. TIBI is dedicated to teaching behaviorology, especially to those who do not have university behaviorology departments or programs available to them; TIBI is a professional organization also dedicated to expanding the behaviorological literature at least through the magazine/newsletter *Behaviorology Today* (originally called *TIBI News Time*) and the *Behaviorology and Radical Behaviorism* journal;** TIBI is a professional organization also dedicated to organizing behaviorological scientists and practitioners into an association (The International Behaviorology Institute Association—TIBIA) so they can engage in coordinated activities that carry out their shared purposes. These activities include (a) encouraging and assisting members to host visiting scholars who are studying behaviorology; (b) enabling TIBI faculty to arrange or provide training for behaviorology students; and (c) providing TIBI certificates to students who successfully complete specified behaviorology curriculum requirements. And TIBI is a professional organization dedicated to representing and developing the philosophical, conceptual, analytical, experimental, and technological components of the separate, independent discipline of behaviorology, the comprehensive natural science discipline of the functional relations between behavior and independent variables including determinants from the environment, both socio–cultural and physical, as well as determinants from the biological history of the species. Therefore, recognizing that behaviorology's principles and contributions are generally relevant to all cultures and species, the purposes of TIBI are:

A. to foster the philosophy of science known as radical behaviorism;
B. to nurture experimental and applied research analyzing the effects of physical, biological, behavioral, and cultural variables on the behavior of organisms, with selection by consequences being an important causal mode relating these variables at the different levels of organization in the life sciences;
C. to extend technological application of behaviorological research results to areas of human concern;
D. to interpret, consistent with scientific foundations, complex behavioral relations;
E. to support methodologies relevant to the scientific analysis, interpretation, and change of both behavior and its relations with other events;
F. to sustain scientific study in diverse specialized areas of behaviorological phenomena;
G. to integrate the concepts, data, and technologies of the discipline's various sub–fields;
H. to develop a verbal community of behaviorologists;
I. to assist programs and departments of behaviorology to teach the philosophical foundations, scientific analyses and methodologies, and technological extensions of the discipline;
J. to promote a scientific "Behavior Literacy" graduation requirement of appropriate content and depth at all levels of educational institutions from kindergarten through university;
K. to encourage the full use of behaviorology as the essential scientific foundation for behavior related work within all fields of human affairs;
L. to cooperate on mutually important concerns with other humanistic and scientific disciplines and technological fields where their members pursue interests overlapping those of behaviorologists; and
M. to communicate to the general public the importance of the behaviorological perspective for the development, well–being, and survival of humankind.

*This statement of the TIBI / TIBIA purposes has been adapted from the TIBI by–laws.

**This journal (BARB) is under development at this time and will appear only when its implementation can be fully and properly supported.—Ed.

Periodical Information

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To submit items for publication, **contact the editor. Send items initially to the editor both by email (or disk) and by hard copy.**

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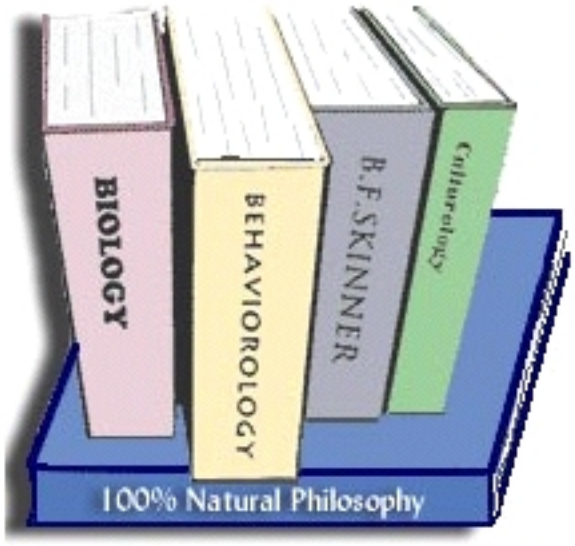

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