

**BEHAVIOR ANALYSIS: THE DISCIPLINE ITS
FOLLOWERS DON'T WANT: A REACTION TO
GROTE, JOHNSTON, RAKOS, AND WULFERT**

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ABSTRACT: The previous issue of this journal included my article entitled "An Academic Home for a Natural Science," followed by the critical reactions of four prominent authors. In this article I respond to a number of the issues that they raised and further explicate my thesis.

Key words: Behavior analysis, behaviorology, discipline, epistemology, natural science, psychology, Skinner.

Articles that I submit for publication may refer to my basic discipline as *behaviorology*, a term that occasionally draws critical reactions from editors and reviewers who complain that it has a divisive effect on the behavioral community. On one such recent occasion, I responded in writing to the editor. Below is a slightly edited portion of that letter:

Dear [Editor],

About the damn "behaviorology" thing. . . . I find it difficult to discuss, because I have trouble finding a starting point. During my own early professional phase many years ago, as I went through my personal episode of the shaping of a behaviorist, I accepted, uncritically, the seemingly worthwhile and necessary mission toward which the leaders of my profession had directed its energies-namely, to "make over" (Skinner's phrase) the organized discipline of psychology by investing it with a solid natural science of the phenomena on which it focused.

Over the next 15 to 20 years I diligently served that mission as a good soldier, sitting in a long series of discussions at one ABA convention after another, . . . reiterating the same old list of suggestions about how to persuade psychology colleagues to adopt the more powerful and effective philosophy and science that we advocated. Across that extended period, I was focusing my own disciplinary analytical skills on a variety of behavioral phenomena mostly in the areas of education, corrections, thanatology, and verbal behavior-and on philosophical domains such as ethics and the

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analysis of determinism. During that lengthy period, I did not, however, fully apply my analytical attention to the disciplinary "war" for the "hearts and minds" of psychologists in which I had been soldiering. I had simply "enlisted" and gone off to participate in that action, and in doing so, I had accepted implicitly the correctness of its cause and the efficacy of the campaign.

Eventually, the evidence mounted that the psychology community was not changing—at least not much. The fraction of psychologists who identified themselves under behavioral labels remained about ten percent (usually less, depending on whose poll served as referent). In the behavioral community, leader after leader lamented the fact that psychological colleagues remained unpersuaded even by the most compelling demonstrations, in both theory and practice, of the power of the behavioral approach. Slowly, I began to turn my own analytical attentions to matters of disciplinary integrity, in particular to an analysis of the task of converting traditional psychology into a natural science discipline. By that time, the movement to accomplish that outcome presented as such a well-failed experiment that I had to question the assumptions with which it had been undertaken.

The twentieth century had opened to an emerging new field under the label of psychology. However, that field was informed by different maturing paradigms. Psychology could become a natural science discipline, or it could merely use scientific methodology in service to mystical assumptions. While clearly the discipline of psychology has to some extent matured along both paths, the issue was essentially resolved at the recruitment table. For every new student predisposed to establish psychology as a natural science, there was a multitude of other recruits who were not predisposed toward natural science, . . . a necessary and predictable implication of open and philosophically unconditional enrollment coupled with a cultural recruitment pool in which fundamentally mystical people have vastly out-numbered naturalists. That lopsided ratio still prevails. While today few miraculous creationists, water dowers, and similar appealers to mysticism seek admission to geology training programs, which shelter themselves under an epistemological tent called natural science, psychology classrooms, in contrast, are packed with welcome students predisposed toward explanatory reliance on mystical variables—variables such as selves that drive bodies, or minds that originate thoughts.

Space is not available here to share the extensive and often troublesome thinking through which I wrestled with this issue. I eventually concluded that the traditional psychology community was scientistic, not scientific, . . . that it did not derive its fundamental assumptions as grand inductions from its scientific explorations, but rather that it brought to science, for validation, its own kind of assumptions derived in ways entirely apart from science. Counter-implications from science were troublesome side effects not to be tolerated, and any facets of science that would give rise to such implications were to be avoided. Traditional psychologists were using science to explore and explicate the implications

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of the postulates that they brought with them from nonscientific origins. They seemed to be seeking confirmations of their assumptions, but only in ways that did not challenge those postulates.

For many traditional psychologists, the assumptions about behavior that they brought to their science were the same assumptions upon which they based many of their deepest convictions, including, especially, those of a religious nature, which is the second most pervasive strand of mysticism in our culture (belief in the agential self being the most pervasive). It slowly dawned on me that I had been fighting a long battle that could only succeed were my side to talk vast numbers of people out of their fundamental (and very comfortable) basic assumptions. For most of those people, the behaviorists' mission objective implied reversing the substantial intrusion of mystical religious beliefs into their interpretations and treatments of the behavior-related subject matter-and for just about all of them, in a more secular vein, the behaviorists' mission implied their abandonment of warm, fuzzy, but nonsensical, humanistic ideas. No scientific arguments make dents in that kind of armor. As one of my religious graduate students once put it during a classroom discussion of this issue, "faith is impervious to reason."

I next turned my attention to the organization of my own discipline, which was represented by the Association for Behavior Analysis (ABA). That, too, was a long process. But in the end, I concluded that ABA was too committed to the quixotical remake of psychology to lead an effective march toward an independent natural science discipline for the study of behavior-a new discipline that could take its place at the roundtable of the basic natural sciences along with physics, chemistry, and biology. At that point I began to contribute part of my efforts to the fledgling behaviorology movement, whose members wanted to create such an independent natural science discipline for the study of behavior/environment relations.

Like almost all behaviorologists, I remain a behavior analyst, and a loyal member of ABA, which I regard as an important organization, both valuable and necessary. It is a grand forum, and, while I support its redirection, I do not wish to see it impaired. There are, of course, some differences in how I do things that follow as implications of my behaviorological investment. For instance, I do not structure my writing in ways contrived to persuade psychologists to abandon their fundamental assumptions and become natural scientists, because I construe such efforts to be exercises in futility. If I harbor any such underlying persuasive motive, it pertains to the members of the behavioral community, whom I would like to see unified around their own disciplinary mission instead of preoccupied with a quest to infiltrate and usurp the discipline of another community.

All of this said, I must acknowledge that, in matters this complex, many of my thoughtful colleagues around the circuit have also entered into the same kinds of analytical deliberations of this disciplinary mess, and they have often come to the opposite conclusion. I, myself, think that we are all edging toward a time when ABA will have to revisit this issue in an

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open and explicit way. While the threat of divisiveness is always present, I would hope for a constructive outcome. However, in support of its arguably untenable mission to forge a natural science discipline within organized psychology, ABA has strategically absorbed many people who can never abandon the fundamental mystical assumptions of the world of agential minds to enter with full commitment into the seemingly alien milieu of the natural sciences.

I think that the key to ultimate success with disciplinary independence will be to focus for a long time on the development of effective science, quality-controlled by a strict natural science philosophy, while promulgating that evolving science only under its explicitly independent and natural science label-working to establish our own academic departments while avoiding costly sociopolitical conflict with organized psychology to the extent that that is possible and strategically appropriate. On that sociopolitical front, ABA should allocate a significant and fixed fraction of its income to the endowment of faculty chairs in any academic department explicitly named for, and devoted to, a natural science of behavior-provided that the recipient department is, in all cases, established apart from any preexisting or coexisting psychology department housed elsewhere within the host institution.

Sincerely,
Lawrence E. Fraley

The Qualitative Analysis of Paradigms

The Nature and Function of an Intellectual Paradigm

As the term is commonly interpreted in scholarly communities, a paradigm is (roughly) a way of thinking based on a set of postulates, although the explicit relation of those postulates to the practical activity that respects them may go undescribed by the behaving parties. Sometimes the term seems interchangeable with "discipline," but is more often construed to be the intellectual foundation around which an organized discipline emerges. Paradigms can be graded for effectiveness in terms of the extent to which they control (lead to) effective practice, where "effective practice" is defined as behaviors that yield reinforcing environmental effects (outcomes). Thus, a paradigm, in the context of an intellectual approach to the interpretation of data, consists of verbal behavior and usually connotes one or more basic assumptions or postulates, along with behaviors respecting their implications when a person is interpreting evidence.

Natural scientists who work among traditional psychologists have at times insisted, as an aspect of accommodation, that there are no "correct" ultimate

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definitions of science, truth, and causation that can be applied to distinguish among competing conceptualizations. In contrast, my position is that paradigms, including their epistemological foundations, can be sorted according to the relative effectiveness of the practical behavior that they inform. The relevant criterion is not correctness, but effectiveness, and that is measurable. Paradigms can be graded according to their capacities to share in the antecedent control of effective behavior. It also follows logically that no two paradigms can be equally effective and efficient in leading to the behavior that solves a given problem.

In fact, it is only because of that kind of functional capacity that paradigms matter to us. Depending on the kinds of outcomes toward which people are working, one paradigm will always be more helpful than any other. Communities that focus on a given class of outcomes will work to identify and consolidate the elements of a paradigm that maximizes the production of such outcomes. If a scholarly community seems to respect an unnecessarily ineffective paradigm, perhaps the reviewer's assumptions about what is most important to that community are mistaken.

The Natural Science Paradigm

A maturing natural science of behavior develops through a progressive sequence of steps (Skinner, 1953, p. 6). Those steps are (a) the discovery and description of behavioral effects, (b) a search for orderly relations among the definitive environmental and behavioral variables pertinent to those effects, (c) the rendering of reliable predictions based on those relations, and finally (and most importantly), (d) the controlled production of predictable effects. The paradigm common to the natural sciences provides a facilitating intellectual (verbal behavioral) support for this sequential maturation and, according to natural scientists, does so to a greater extent than can other paradigms. Among the fundamental principles of the natural behavior science paradigm are these: The world is a natural product of evolutionary selection; life forms have been selected by contingencies of survival, operant behaviors have been selected by contingencies of reinforcement, and cultural practices have been selected repeatedly, by many different individuals, to be taught (i.e., commonly selected for inclusion in the cultural training curriculum). Perhaps the most basic postulate is that all events, including behavioral events, have a natural history. That is, they are presumed to be determined by prior events featuring measurable variables, and those events, in turn, likewise. Such

histories can be described as chains of functional relations, and the essential assumption is that no link is broken to allow for intrusions of a mystical or supernatural nature.

Relevant Training

Few members of a paradigmatic community class themselves as such because they have reviewed the capacities of different available paradigms to share in controlling behavior and adopted the one that affords the most contact with their reinforcers. The emergence of a person within a particular intellectual community is more often the result of accident, not its selection on merit.

I have concluded that every natural science training program should include a course in comparative analytical epistemology. Different paradigms would be examined for their functional contributions to classes of outcomes deemed important within the communities in which those paradigms respectively prevail. A student should know how the paradigm of one's discipline leads to effective practice within that discipline. The student should be able to trace the function of a prevailing paradigm to the practices characteristic of a discipline and to do that not only for his or her own discipline, but for others as well.

The student should be prepared to account for the origins of the elements of paradigms. For example, beyond merely reciting the postulate that any behavioral event is totally and naturally controlled by potentially discoverable functional relations, the student would also have to suggest a rational origin for such a postulate—that is, specify contingencies under which we may anticipate such an inductive leap beyond the limits of evidence. The student should then proceed to describe the implications of respecting it in practice.

It seems to me that too few behavior analysts, and even too few behaviorologists, are prepared to defend, either intrinsically or comparatively, the paradigm to which their discipline claims commitment. Paradigms, by their nature, inevitably determine critical functional capacity and are therefore too important to be left to largely unanalyzed accidents—or to be treated cavalierly and ignored in science training programs under some misplaced code of social propriety. Would-be scientists should first study the science of science. A few such courses may exist, but, in general, such courses are not likely to emerge in typical psychology departments, where, for reasons that should be obvious, such qualitative comparisons of intellectual armaments are

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likely to be deemed divisive, and the explicit revelation to students of even the basis for performance rankings among alternative paradigms is typically deemed impolite.

Intellectual Paradigms Define Disciplines

Although paradigms tolerant of mystical variables have predominated in the field of psychology, psychology has always been home to a paradigmatic mix. As Thomas Hardy Leahey, a prominent contemporary historian of psychology, has concluded about paradigmatic unity in that field: "I now believe that there never has been a paradigm in psychology, and to think so obliterates vital differences between thinkers lumped together in a supposed shared 'paradigm'" (Leahey, 1997, p. xvii). I would put it this way: The psychology community has been a professional coalition of different intellectual factions whose activities have been informed by different and often antithetical paradigms. The natural science faction, always relatively small, has been stymied in its proselytization efforts, because the scientific repertoire of the nonnaturalists (often antinaturalists) is quality-controlled by paradigms featuring postulates that preclude the very knowing that is the theme of the behaviorists' proselytizing. The functional capacity of their mystical postulates endures safely beneath the penetrative capacity of adduced scientific evidence, leaving those individuals unsusceptible to the kind of persuasions proffered from the natural science sector.

Reaction to the Article by Irene Grote, (Grote, 1997)

The Current Debate: Its Nature and Issues

Grote's interpretation of my position notwithstanding, now is not the historical moment for a precipitate behavioral exodus from psychology in the manner of some grand ostentation. The long years devoted to coexisting with psychology and the protracted effort to change it into a natural science discipline have left the behavior analysis community somewhat unfocused and in disarray with respect to its disciplinary objectives. There is no imminent revolution to be undertaken by a cross-purposed, under-trained, politically weak, and intellectually compromised community.

The current debate is about where this community should be, and *what* it should be, in thirty to fifty years. The first order of business is for the

professional organizations of this discipline-especially The Association for Behavior Analysis (ABA)-to review and recast the long-term evolutionary mission for the emergence and consolidation of this discipline-to promote a professional coalescence around the discipline's own paradigmatic integrity as one of the basic natural sciences-and then to start making sure that every current action comports in some way with that long-range objective.

A few valuable experiments are now under way-small approximations that test important features of what seemingly must be done. Examples include the new and exclusively behavior analytic department, independent of psychology, at University of North Texas; a new small science-focused organization that denotes the independent integrity of our natural science paradigm and its focus on behavior/environment relations by operating under a new name free of all associations with antithetical paradigms-The International Behaviorology Society (Vieitez, 1998); and a new organization focused on the development of appropriate curricula and training opportunities-The International Behaviorology Institute and its related Association (Ledoux, 1998). These small exploratory steps in the right direction partially fill the vacuum left by ABA in the wake of its historical preoccupation with organized psychology. These are timely and worthwhile experiments, and the behavior analytic community at large should support them, draw upon their findings, and model after them as appropriate.

The Analytical Capacity of Our Discipline

The natural science of behavior identifies and describes the functional relations under which behavior occurs. The first term in a three-term contingency of reinforcement reveals, at the level of proximal cause, why a behavior happens on a given occasion, and a review of the conditioning history of the organism reveals how that function came to be capacitated in the first place-an explanatory reach to more remote causation. The descriptions of such relations and events, to which a large part of our science is devoted, explain why behaviors occur.

Seemingly more troublesome have been the "should"-questions (see, e.g., Vargas, 1982). But, insofar as "ought" statements comport with the speaker's values, and the speaker's values are the speaker's reinforcers, we have only to review the speaker's phylogenic and conditioning histories to account for the speaker's "ought" statements. We can, in theory, also arrange for the speaker to undergo some new conditioning that will result in that person endorsing

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anything that we care to prescribe. At this stage in the maturation of our discipline not much about the *whys* and the *oughts* remains beyond the reach of our philosophically-informed science and the behavior technology that it supports (Fraley, 1998, chap. 23).

A "Good" Home in These Times

Grote described her academic home in the Department of Human Development at the University of Kansas as a good place to work. As a behavior analyst, she reported that her scholarly activity (and implicitly that of other behavioral people there) is "tolerated" by her cognitive psychology colleagues, which implicitly she tallied as a plus. She reported that she herself was trained "in the best of all possible worlds for a science of behavior..." – the unadulterated behavior analysis program at University of North Texas. She argued, however, that good scientific work is primarily a function of the self-management practices by which individual scientists stay focused on their own kind of science. It seems to me that Grote's conclusions about the quality of her present home do not comport with her own assessment of the model program at University of North Texas that she selected for her own training.

Notwithstanding Grote's expressed satisfaction with the cognitive/behavioral mix at Kansas, I do not believe that having to rely almost entirely on self-management practices to sustain one's work in an unhelpful environment is good enough for our discipline. A strong discipline requires its own academic departments in which to work—a small community of scholars that can exert substantial control over both the integrity of their discipline and the preparation of both themselves and their students for the kinds of careers to which that discipline affords access—departments that potentially can offer far more appropriated resources and more direct support from proximal colleagues than benign neglect, tolerance, or even distanced respect.

At the training level, a comprehensive and worthwhile disciplinary curriculum is a kind of synergistic product of the faculty. Behavioral training opportunities of that kind are not possible when behaviorists operate in bubbles of isolation—scattered among a psychology majority and allowed only a limited minority voice in setting the training curricula—with each piece of course work that they are permitted to offer to their students representing a compromise with followers of an antithetical discipline. Under such conditions, the training of new behavioral students must occur in the absence of a balanced and comprehensive natural science curriculum and

occurs instead largely by some osmotic process occasioned by closeness to a mentor-a kind of training that leaves the student narrowly focused along the strengths of the mentor and deficient in other dimensions of the discipline.

True, attainment of the best for which one can realistically hope tends to get a glowing account. But the more important issue resides with the implications of our disciplinary leaders having led us down a path that leaves scholars like Grote with professional opportunities limited to the kind of compromised academic homemaking for which she has had to reserve her praise.

Reaction to the Article by James Johnston (Johnston, 1997)

Framing our Disciplinary Problem

I share Johnston's preference that our discipline had emerged in association with biology instead of psychology. However, Johnston's proposition that we cannot revise history and must find ways to live with the consequences of what has actually occurred is a truism that avoids what I believe is a more relevant observation, namely, that we can now begin to construct a new historical episode that, in thirty to fifty years, will have become the history that established the integrity and endurance of our discipline.

Describing Disciplinary Differences

Johnston reiterated the often mentioned assertion that the objective for nonbehavioral social scientists is to understand and predict (but implicitly not control) behavior. However, job offerings in the field of behavior seldom list high paying jobs for persons whose duty consists of standing around and, on the basis of their understanding, predicting what is going to occur. If some behaviorists are reluctant to grade the capacity of their discipline to support effective action in relation to the corresponding capacities of alternative disciplines, others unwittingly, in conducting the practical affairs of the market place, have already established that kind of differential in worth along which disciplines can be graded. The available jobs are all for people who can intervene and effect some kind of *control* over the phenomena of concern.

The general name for that is technology-in our case a technology of behavior-and the *only* way to practice such a technology, without altering the body that behaves, is to manipulate (i.e., change in a controlled way) those

independent variables that bear a functional relation to the dependent behavioral variables of concern. No evidence exists for a parallel universe in which practical changes in behavior can be effected in any other way.

The public at large wants behavior controlled, not just predicted, but distinguishing among disciplines is difficult for most people—a confusion that is only exacerbated when behavior analysis remains integrated with traditional psychology. Traditional psychologists proclaim that their cognitively or mentalistically informed practices are as effective as those of the behaviorists for intervening to "induce" changes in behavior. The truth (as behaviorists know it) is that behavior can be changed in only one general kind of way—namely, through the kind of functional processes upon which behavior analysts focus. In reality, for a psychological practitioner to effect a behavior change requires exactly the same kind of function-respecting operations as would be performed by a function-sensitive practitioner of natural science. The only difference is that one party speaks of what is being done in ways that have little or nothing to do with either the essential aspects of those practices or the functional relations through which the outcomes manifest, while the other party's descriptive rhetoric pertains precisely to those matters and therefore supplements the controls on the practitioner's effective behavior.

The difference between effective and ineffective water dowsers was that the practices of the good ones were largely controlled by the natural contingencies prevailing in their work environments and much less by the silly mystical rhetoric with which dowsers described their activities. The ones that reliably found water were intuitive ground water geologists who exercised control over their functionally passive forked sticks (perhaps without concomitant self-notice of their doing so), while the ones that made frequent mistakes behaved largely under control of their own functionally irrelevant rhetoric plus accidental stimulation *from* their forked sticks. Notably, water dowsers, whether ineffective or intuitively effective, have steadily lost market share to an alternative and independently organized natural science community of ground water geologists in whom the intellectual component more fully, functionally, and directly supports the practical behavior of the practitioners.

Hypocrisy

If behavior analysts feel compelled to insist that they are just one kind of psychologist, they must somehow rationalize their personal view that their own epistemology and the science that it supports is more effective than the

corresponding intellectual practices of their cognitive counterparts. (To surrender even private claim to a better disciplinary approach would imply irrationality in their stubborn adherence to the natural science paradigm.) To escape that dilemma, some behavior analysts suggest that the cognitivists and they entertain different goals: The behavior analysts say that they are pragmatically interested in producing practical outcomes-interested in intervention technologies that yield planned and measurable changes in behavior-or more generally, that they want to operate at the level of control. Being under contingencies to gain control of behavior, the behavior analysts then explain that they pursue the kind of epistemologically informed science that affords such a capacity, while the cognitivists are asserted to have only the goals of understanding and explaining behavioral phenomena in terms of the mental activity through which cognitivists assume that behavior originates-limited goals that can be met while operating in accordance with the fundamentally mystical epistemology of mentalists.

Cognitivists are categorized that way by the behaviorists because behaviorists are convinced that their own focus on behavior-controlling functions is really the *only* approach to control. It is the same approach to control that characterizes all of the natural sciences, of which the natural science of behavior is but one representative. From the natural science perspective that prevails in physics, chemistry, biology, and behaviorology, the control of any variable can be attained *only* through interventions among the antecedent variables in the functional relations that yield the phenomenon of interest. To the extent that the paradigm of the cognitivists neglects functional analyses (especially analyses of relations between behavior and accessible environmental events) and for many mentalists does not require that functions among physically defined variables account *exclusively* for the occurrence of behavior, it follows that cognitivists could get little help from their paradigm in their efforts to "influence" behavior (as they typically describe their paradigmatically moderated attempts to control it). That implies, of course, that when cognitivists assert that they too can develop effective behavioral technologies *on the basis of their paradigm* (as nearly all do), they must be deluding themselves. They do, of course, develop many effective behavioral practices, but where are the behavior analysts who really believe that that occurs as a functional result of, instead of in spite of, any paradigm that would shun an analysis of the relevant functional relations?

The dismissal of such paradigmatic differences merely as reflections of differing interests (goals) may be based wishfully on the behaviorists' own

assessment of the relative capacities of the two paradigms. But insofar as the mentalistic psychologists do not share that view of their own way of thinking, it is a condescending ploy, the transparency of which raises the question of which audience is the target. That argument cleverly obscures the fact that the cognitivist paradigm, in failing to adhere strictly to functional analyses involving both accessible and measurable variables, is proportionally deficient in its capacity to solve practical behavior-related problems effectively. Furthermore, the differing goals idea would be acceptable only to those cognitivists who would let the behaviorists specify their goals for them and who would accept being cut out of the effective intervention business by being assigned goals that stop short of the level of control. Where are the cognitivists who are supposedly willing to let others consign them to a class of goals for which there is no market and who agree that their own paradigm cannot support effective intervention technologies?

Objections by mentalists aside, the disciplinary distinction being discussed here *is* valid with respect to capacities to support effective interventions but not with respect to goals. This "differing goals" argument looks more like one of those propagandistic explanations, constructed and promulgated by savvy behavioral thinkers, who themselves are not seduced by it, and disseminated to the rank and file of their own paradigmatic community to help calm the intellectual turmoil of a conflicted population. It has a calming effect insofar as it seems to provide a kind of answer to the perplexing question of how a scattering of natural scientists are supposed to relate to their surroundings in the midst of a scientific community. This reassuring pseudoanswer suggests that we merely have two sets of cooperating colleagues, equally well equipped with respectable if suitably differing intellectual armamentarium, who happen to be pursuing different interests. Ah..., that feels good to the behavioral ear on which it falls! Delicately cast, it may lull a few mentalists, too. (For a good example of an article incidentally asserting that behavior analysts are psychologists and gratuitously accounting for the intellectual differences between behavioral and cognitive psychologists on the basis of putative differences in interests and goals, see Hayes and Hayes, 1992.)

According to that argument, those antithetical epistemologies and sciences have been selected by the differing contingencies under which their respective adherents were placed by their own adoption of differing goals. However, such capacity-limiting goals as "understanding" and "explaining" were not selected by early psychologists. Those putative goals were only later

pinned on the cognitivists by the behaviorists, because behaviorists believe that, without strict functional analyses, the cognitive paradigm, like others that fail to penetrate effectively to the level of functional causation, cannot support activity at the level of control (i.e., the level of practical intervention). The "differing goals" spin, if issued with serious academic countenance, is perhaps a little less likely to offend the traditional psychologists than to assert more directly that the mentalistic paradigm entertained by most psychologists offers little paradigmatic support for an effective behavior technology thus leaving the mentalists to whatever level of effective practice the prevailing and often unanalyzed natural contingencies can support.

This "differing goals" thesis posits a fundamentally dishonest explanation, because it misrepresents the motives of cognitive psychologists who, in general, mistakenly believe that their paradigm *is* as functionally applicable to practical interventions as any other. They do *not* accept that their paradigm is functionally inadequate to support what they construe to be good works. *As* behavior analysts well know, natural contingencies almost always exert substantial power, and a lot of worthwhile practical work can be accomplished under such contingencies, in spite of any nonsensical paradigmatic rhetoric that may accompany the effort. Furthermore, the natural contingencies that prevail in practical arenas tend to go undescribed and unanalyzed unless one is trained explicitly to look for them and analyze their effects. The truth is that much of what is accomplished by practitioners of any disciplinary persuasion, including the behaviorists, occurs under prevailing natural contingencies and requires little if any contribution from concurrent paradigmatic rhetoric, public or private. For example, the above-chance success rates that some of the "healers" of yore attained, perhaps with herbal medicines, certainly had nothing to do with the inane hocus-pocus that often accompanied their healing practices, even when they themselves sincerely believed otherwise.

Cognitivists and behaviorists do not differ paradigmatically because they originally developed different interests and set different goals. The original theorists in both camps were interested in finding conceptual paths to effective technologies. Who wanted to create a discipline that would leave its followers wise but ineffective? While it is true that some people may now avoid wasting their time pursuing objectives that are rendered unrealistic by the limitations of their own respective sciences, cognitivists simply do not acknowledge that their science imposes limits that the behavioral paradigm allows its followers to exceed. Contemporary cognitivists believe passionately

in the efficacy of their science, especially in its capacity to support practical behavior technologies in what they uncritically presume to be a functional way, and their paradigm simply does not equip them for coming to know better. American education is an example of an applied field that has always been informed almost exclusively by scientific foundations consisting of traditional cognitive and humanistic psychology. The capacity of those paradigmatic foundations to support an effective behavioral technology of teaching can be measured by the current condition of American education, which is widely proclaimed to be a national tragedy.

Identifying the Real Issues

Personal respect for behaviorists is nice, but it is not the point in this debate. We are talking about how best to protect and develop the integrity of our discipline-and about the kind of setting in which the most effective training can be provided. Perhaps some behavior analysts really believe that remaining in psychology is necessary for contact with the subject matter upon which psychologists focus. However, the literature of any discipline is open and available to anyone, and so is most of the public marketplace. Regardless of where one class of scholars reside organizationally, they can access the literature of all others. Nor does an independent organizational territory necessarily imply economic isolation. Economic contingencies will eventually compel consumers to seek out the most effective people. Even legislated rights to exclusive access to markets through certification are putatively based on scientific qualification and therefore remain subject to challenge.

Psychologists are not going to permit themselves to be put out of business by losing contests of efficacy to behavior analysts-especially not in their own departments, in front of their own students, in their own field, nor in their own politically organized discipline. Yet, to the extent that natural science is more effective in gaining control of the environment than its alternatives (which most behavior analysts, for good reasons, profess to believe), that will happen if the playing field is level. When it really matters in practical ways, people evince little trust in outcomes produced in a manner that disrespects natural science. For most people, production informed by natural science principles and postulates is an expectation, and a demand, before they will board an airplane, schedule surgery, or drink the city water. Such public vigilance notwithstanding, the contingencies of accountability enforced by the public have not been particularly stringent with respect to the *behavioral*

outcomes of scientifically informed practices. Behavioral practitioners, whether teachers, counselors, or whatever, have always found ways to evade personal responsibility for any inadequate outcomes of their behavior technologies, usually by relying on their evaluators' mentalistic concepts of a person as an autonomous or semiautonomous behavior generating agent that is susceptible only to so much external influence and is therefore personally responsible—a view that weakens the contingencies of accountability imposed on the behavioral practitioner.

The organized community of mentalistic psychology must do what is necessary to prevent the cancer of a natural science developing to full-blown mature effectiveness within its own innards. It is irrelevant whether behavior analysts who work under the umbrella of organized psychology are arrogant or humbly respectful. Humble respect buys one a little more neglect and hence elbow room to do one's own work—and for many, that is good enough. It is all that they can realistically expect: Keep quiet, do not cause trouble, and you can pursue your specific scientific interests on a limited basis—the kind of placating offer long extended to the subjugated minorities of the world.

However, except in a limited way before their implications become apparent, here are actions that you may *not* take in that situation: You may not recruit your own kind to attain a political majority. You may not join others of your kind to construct a comprehensive, multitiered, integral training curriculum in your discipline independent of intruding influences from—and required training in—traditional psychology. You may not teach verbal behavior and its recent extensions in such a thorough and comprehensive way that students abandon reliance on cognitive theories thereby rendered redundant. You may not teach comparative epistemology to the extent that the psychological paradigm suffers invidious comparisons of potential efficacy relative to the natural sciences.

In many psychology departments the containment of behaviorism goes beyond restrictions on proactive behavioral activities to include imposed servitude supportive of the basic mystical paradigm of the hosts-cum-masters: I have continually been surprised to hear behavior analysts—often persons whom I have known casually for years—finally admit that they themselves have long been compelled to teach traditional psychology in which they personally do not believe. Few are prepared to bear the consequences of saying no when handed a text book and assigned to teach such a course, especially if that demand is echoed by the band of psychological colleagues in whose midst that behaviorist must work. Regardless of what a behavior analyst may believe

(and in spite of the First Amendment to the United States Constitution), these individuals are tenable to refuse to teach, affirmatively, traditional psychology courses, especially if they expect permission to teach occasional behavioral courses. Under the conditions wrought by the organizational architects of their discipline, many behavior analysts now toil shamefully in that kind of intellectual slavery, and their rationalizations reverberate with a familiar pathos.

Johnston also pointed to certain advances in therapy made possible by contributions from cognitive psychology and concluded that this represents a step in the cognitive revolution that, to some degree, validates the efficacy of cognitive psychology. He suggested that those advances filled some gaps left when less sophisticated and outmoded behavioral techniques fell short and that the cognitive psychologists deserve some respect for having won that contest of efficacy. Behavior analysts who aspire to operate on that frontier are now left to review the relevant cognitive principles and the practices that those principles have spawned. True, the behavioral people, as always, must translate and reinterpret those principles and practices in terms of their own functional science, but the behaviorists are now left to work with what survives that process of translation/reinterpretation from the cognitivists' archives. Contemporary behaviorists must follow in the wake of the cognitivists, because their own discipline, although providing the foundations for the necessary reinterpretations, has failed to produce a parallel and competitive alternative therapeutic technology.

These are valuable observations on Johnston's part, but they support a more important kind of review and interpretation than he provided. The cognition-related phenomena posited as underlying those therapeutic advances has traditionally been a centerpiece of psychological study for reasons that can be gleaned from the history of psychology (Leahey, 1997). Because the workings of an agential mind have always held a special fascination for psychologists, to outsiders traditional psychology has often appeared as little else than the study of mental activity. On the other hand, the functional analysis of verbal behavior in our discipline, although recognized as a very important area of study, has generally been regarded as focusing on just one of different classes of the behavior/environment relations that comprise our field. However, a mature functional analysis of verbal behavior, incorporating the extensions of analytical power from stimulus equivalence (Sidman, 1994) and perhaps the relational frame theories of Hayes and Hayes (1992), leaves a mind without an implicit requirement that it have any

capacity for spontaneous origination (an essential nexus of mental agency) and diminishes the practical importance of physiologically-based behavior-mediating functions upon which cognitive psychologists dwell in extensive preoccupation.

Three relevant points can be made about concluding that psychological accomplishments, unmatched by the behavioral camp, imply some approximation of equality in the capacity of the two paradigms to support effective action: First, the competing teams have not been equal in size. Psychologists pursuing the implications of cognitive theories—for example, as those theories may apply to practical matters in therapy—have outnumbered the behavioral people who have pursued the correspondingly applied implications of a functional analysis of verbal behavior.

Second, the natural contingencies in any field, when they become sufficiently stringent, will tend to shape increasingly effective practices regardless of how little of the functional origin of those practices can be traced to the contributions of the practitioners' theoretical frameworks. Also, given that theories relying on the fictitious force of suction can lead to the development of pumps that actually work in certain environments, I do not take the position that cognitive theories, however fallacy laden, can never contribute to an effective behavioral practice. Therefore, when large numbers of cognitivists set to work in any area, it is not surprising that we find some effective practices emerging, nor that one of their applied specializations seems to have gotten ahead of the parallel field defined by the work of their few behavioral counterparts.

Third, the conceptual sophistication of Skinner's treatment of verbal behavior (Skinner, 1957), and its more recent analytical extensions, has required a level of training that few behavioral faculty members have been prepared to provide—in part, because the fragmented and spotty behavioral training possible in the midst of other peoples' academic departments prepares too few potential teachers of that challenging facet of the behavioral subject matter. Given that cognition is at the central theoretical core of the mental domain that psychologists see as the essence of their subject matter, why would psychologists tolerate the emergence, in their midst, of a subdiscipline as antithetical to their perspective as the functional analysis of verbal behavior? It is one thing to tolerate the teaching of some applied behaviorally-derived practices; it is another thing entirely to permit the emergence of a major theoretical challenge to the premises upon which rests the core of their discipline.

When Johnston stops his interpretation (of some behavior therapists

losing a professional contest to some cognitive therapists) at the point of giving the cognitivists a deserved pat on the butt for handing us a temporary loss in the best-therapy contest, he hardly makes the most of his opportunity. That kind of tragic setback for our discipline, to the extent that it is real, stands as compelling evidence of our need to reorganize our training mission in far more effective ways than will ever be possible for a band of academic vagrants dispersed across the academic countryside as guests in the homes of others-guests who too often must fine-tune their tolerability at the expense of the integrity of their discipline.

Johnston argued that the benefits from critiques by colleagues in other disciplines, in whose departments we are scattered, maintain our civility and challenge us in scientifically useful ways-another trivial truth. But it is both misinterpreted and overrated: People are usually civil when they walk in a community in which everyone carries weapons (of whatever kind). That kind of Civility does not reflect a virtue; it is a conditioned necessity. Also, the critiques of intellectual miscreants in our *own* field strike even closer to the essence of important disciplinary issues and challenge us in even more annoying ways, for whatever benefit that kind of provocation is worth (see Falk, 1996, for a discussion of the contribution of adversity to creative behavior). Before positing access to collegial critiques as a compelling reason for natural scientists not to operate out of their own academic departments, I suggest that that argument first be floated to physicists, chemists, biologists, and geologists for their evaluation of its relative merit. The frequently touted benefits of cross-fertilization and expressions of deserved respect can still be realized across a hallway-a small separation that makes big differences-insuring not only a faculty's determination of its own academic affairs, but also freedom from the stifling obligation to find merit where none exists as a form of rent-paying for one's space in the home of persons who do not share one's confidence in a natural science of the subject matter.

Smoke

In the end, Johnston retreated to reliance on one of the most well-failed propositions in behavior analysis: He said, "I ... believe that behavior analysis has lost its influence in mainstream psychology because ... we have relied on arrogant rhetoric rather than data to demonstrate the superiority of our approach." Mistaking traditional psychology for a scientific community, whose members are persuadable by data produced through practices informed

by a natural science epistemology, is the primordial disciplinary mistake in behavior analysis. He apparently knows better, because earlier in his own paper Johnston himself explained at one level why "neither behavior analysts nor traditional psychologists are persuaded by each other's data." Just as most American citizens are more or less aware of the social admonition not to talk to strangers about religion or politics (because antagonism is probable and persuasion is impossible), scientific scholars are supposed to know that faith is impervious to reason and that it is therefore not productive to layout data-based scientific arguments contradicting the basic assumptions of pseudoscientists who use scientific methodology mainly in their quest to validate various aspects of their fundamentally mystical postulates.

To behavior analysts who continue, stubbornly, to insist that a way can someday be found to present behavior-analytically-produced data to traditional psychologists that will result in their adopting the natural science that many in our community still call behavior analysis, I propose a relevant practice exercise: Select a fundamentalistic religious group in your neighborhood, whose proclaimed public mission is to change immoral behavior to righteous behavior-a group whose approach to knowing, and whose beliefs, you regard as invalid. Go explain to the members of that group, with appropriate subtle delicacy, how and why their assumptions are wrong and what is wrong with their general way of knowing. At the same time, provide them with your demonstrably better alternative, *building a carefully constructed data-based case for the superior level of practical control that your proffered paradigm makes possible*. Be especially polite and humble, and avoid arrogant rhetoric.

You pass the test when significant numbers of those people abandon their former postulates, adopt both your alternative assumptions and your epistemological approach, and, on the basis of those new fundamentals, begin acting along the putatively more effective lines to which you have pointed them. (Should you fail, you can always insist later that those people were not really interested in the practical control of their environment and wanted only to understand it.) Such a blatant exercise in futility sounds like a silly suggestion. Yet those few individuals in the behavioral community who suggest that behavior analysts should know better than to waste their time that way have always been dismissed as arrogant, and those in the behavioral community who further suggest that at least the more savvy behavior analysts *do* know better have always been dismissed as damned arrogant.

The Nature and Context of the Real Debate

There is a time and place for the members of a discipline to analyze and distinguish clearly between the efficacy of different disciplines—a time and place for debate about the relative merits of differing epistemologies and the divergent technologies that those disciplines can spawn—a time and place for an expose of any fallacies in the foundations of respective disciplines. That time and place is during the consideration, by members of a given discipline, of potential actions that may be taken, relative to other disciplines, to guide the development of their own discipline along the best course to maturity. We engage in such a debate, not to pursue a pointless diminution of another people's discipline, but to focus on what is best for ours in relation to theirs. Although it is necessarily an exercise in explicit comparisons, it is part of a largely intradisciplinary discussion of our own future conducted by our own people at our own roundtable.

During this important debate, to accuse those who raise the relevant issues and adduce the relevant facts of "leading a crusade against nonbehaviorists" appears as little more than a diversionary tactic in behalf of those who, in the comfort of their personal accommodations, do not want such a debate to occur and are hoping that, once again, disparaging those who come first to the table will buy another postponement. I concede that what is relevant during this important debate within our community may go unappreciated and even resented among the devotees of other disciplines should their paradigms suffer invidious comparisons during our debate of our own disciplinary future. But this is *our* debate, and those members of our own community who are so invested in other peoples' disciplines that they cannot objectively enter into discussions about the best course for our own discipline have some private soul-searching to do before they come to the table.

Though some of the disciplinary organizational issues to which we should attend pertain to interdisciplinary social phenomena, the most important discipline-related problem to be solved here is not a social problem pertaining to the personal demeanor of some of our behavioral people. The important issue is how best, organizationally, to develop and maintain the integrity of our natural science discipline of behavior/environment relations so that it may best serve our contemporary culture. Labeling activists in this quest as social misfits diverts attention from how some behavioral people have compromised their own way to comfortable nests in various psychology departments—perches in trees that they would apparently prefer others not shake in behalf

of a more catholic objective.

To them I say "don't worry." Events in these matters unfold at a pace too slow to have much effect on the career to which a given individual has already committed. Those behaviorists who are comfortably and perhaps necessarily snuggled into psychology departments are obviously going to remain there for the duration of their careers. This debate poses little threat to their job security. Realistically, most of them are trapped, and it is too late to rescue them. At issue is the question of what our organized profession is doing either to continue forcing future behavior analysts into the milieu of psychology or, alternatively, to create working environments for natural scientists of behavior that are more conducive to intellectual and professional vitality—environments that can support the kind of multifaceted and multitiered training curricula befitting a complex natural science.

To each of those behavioral people now working in psychology departments, I would also point out that the Association for Behavior Analysis (ABA) is not your psychology department. ABA is our own grand forum where we all come together, not to defend the personal accommodations and compromises that many of our members have had to make in their individual professional lives. Rather, ABA is where we all come together to map the disciplinary strategies that will make similar accommodations and compromises less necessary in the lives of future generations of behaviorologists. That important work can begin just as soon as the leaders of ABA stop steering the organization along a meandering course of reflective justifications for the kinds of self-accommodating career moves into which they were forced and instead focus that organization on the development, within this culture, of a suitable and well established home for a natural science of behavior by the year 2050.

Reaction to the Article by Richard Rakos (Rakos, 1997)

The Ceiling Effect

Rakos has presented a useful survey of the progress that the natural scientists of behavior, operating under various labels, have made in changing psychology into a worthwhile science-based field. The progress he reports on various fronts is, in a few instances, more than some may have supposed. Overall, however, it is not much for fifty years of demonstrations of efficacy presented in ways that, if directed to the members of a natural science

discipline, would have proven much more readily persuasive.

As we have seen, even in the series of papers following and rebutting my lead article (Fraley, 1998), most competent behavior analysts can mount some version of the general argument that psychologists and natural scientists of behavior operate with incompatible epistemologies and therefore do not interpret evidence in compatible ways-which means that they cannot persuade one another of the correctness of their respective intellectual modes nor effectively promote acceptance of their respective products by the other kind of thinkers. At this late date, nearly all parties are convinced that this is true even though many behavior analysts who agree with that conclusion nevertheless continue publicly to endorse continuance of the persuasive approach. In my lead article (Fraley, 1997), I described some obvious characteristics of our culture that explain in more detail the nature of the resistance that behaviorists have encountered within psychology. That resistance was predictable and has come as a surprise to behaviorists only to the extent that they have misunderstood those upon whom they have wasted their proselytizing: An intradisciplinary debate and an *interdisciplinary* debate are two different things. For fifty years, behavior analysts, whose eyes were taking the measure of their adversaries' resource cache instead of taking the measure of their adversaries, have been trying to use scientifically adduced data to talk psychologists out of an epistemological framework intuitively crafted to accommodate mystical postulates. We are supposed to know better than that, and I assume most behavior analysts do. Many who continue to endorse that approach apparently pursue that quest for reasons other than its potential success.

The progress in "making over" psychology that Rakos describes falls into two classes based on the kinds of people within psychology who are being affected. First, there has always been a small subcommunity of persons operating within psychology who, though untrained in the functional analysis of behavior, are predisposed toward the basic philosophy of the natural sciences and who therefore require only education in the details of a natural science of behavior. We have only to take such a person to school, and that individual will learn. The behavior analysts have always had success in appealing to such people, and one mistake has been to assume uncritically that psychology is populated largely by persons of that nature. It is not.

Second, psychologists could always be found who, when facing difficult practical problems, will adopt effective practices quietly offered to them by behaviorists, provided those behaviorists do not emphasize the disciplinary

implications connoted by those practices. Operating at the intellectually superficial level of "here, try this" or "here, try that," behavior analysts have slipped many effective practices to psychological colleagues. The mere adoption of such practices has in some cases tempted the behavioral evangelists to chalk up another conversion, but that is unwarranted. The appropriation of a behavioral practice by cognitivists who shun the philosophy and science that spawned that practice should not be construed as anything more than an instance of expedient adaptation without endorsement. No strongly conditioned philosophy is going to be challenged merely by the efficacy of a practice that was developed under the influence of an antithetical philosophy. When implications of that history surface, the meaning of the practice is easily reinterpreted. Also, lacking the quality control of the philosophy from which the practice is then severed, that practice is subject to a drift in form that eventually renders it ineffective. When it is then supplanted, typically by something drawn from the arsenal of the individual's own discipline, the discredited practice may be reassociated with its behavioral heritage for the discard ceremony so that the failure of what is left of that corrupted practice can be associated publicly with the behavioral paradigm.

The successful intrusion of the behavior analytic paradigm into the field of organized psychology has an upper limit defined by the size of the subclasses of behaviorally susceptible people within psychology, because only with those people will efforts to proselytize have some potential for success. In my lead article I have set forth the reasons why that potentially convertible subset is relatively small, and will remain small, within psychology. Thus, the long-term primary strategy of the behavior analytic community for the conversion of psychologists to the paradigm of the natural sciences is limited in its potential by this kind of ceiling effect. That is, only a modest fraction of the psychology community—far too few to support a political or scientific revolution—is potentially susceptible to the only legitimate kind of conversion activity available to the behavioral camp. The progress that Rakos has described represents the approach of the behavioral invasion of psychology to its high water mark. Hopefully, future behaviorological monuments to this historical episode do not have to include among the proclaimed laments a devastating loss of behavior science momentum within this culture. We do not have to play out this futile surge to exhaustion in our own final Pickett's charge.

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Bad Contingencies Produce Bad Behavior

Rakos, after providing a useful delineation of effects that our discipline has had on that of traditional psychology, argues that establishing an independent discipline for the natural science of behavior would cost our disciplinary community both "psychology's resources and social acceptance." I believe that current resource accumulations and current circumstances of social acceptance are not the most important factors to be considered when charting the course of an emerging discipline. Threats both to resources and public palatability have always been faced by any minority that seeks to establish a new and better way to operate under its own tenets.

In the small deep-southern towns of my youth, the very few African-Americans who worked actively to end racial segregation were ostracized both economically and socially, but few among us would argue today that, for the good of both their cause and the long term prosperity of their people, they should have toiled instead to remain "good niggers" (the phrase reserved in that culture for subjugated individuals of any race who tried to salvage a modicum of self respect by making a virtue out of their subservient status). Perhaps some behavior analysts, who today pretend to be fighting the good behavioral fight mainly to stay close to the copious resources of the psychology establishment and to enjoy the pitiful kind of "social acceptance" reserved for "good behaviorists" in that community, could indeed be classed as intellectual whores (the amusing phrase that Rakos invoked when interpreting my argument). I remember the many African-Americans in my languid Dixie community half a century ago who did speak piously, to similar effect, of the propriety in keeping to their proper subordinate place in the social order. But such pathetic spectacles-their kind then and our kind now-simply reflect rational behavior occurring under some distasteful social contingencies that inhere, in our case, in the strategy long ago adopted by behavior analysts for the development of their discipline.

Propitious Realities of the Intellectual Market Place

Both natural contingencies to be effective and social contingencies to be accountable impel the evolution of practice toward behavioral technologies of the kind that are supported by our science. Practitioners who find themselves under sufficiently stringent contingencies of those kinds will seek out those who can show them how to be effective, regardless of where they have to go

to find that help. Rakos has pointed to several examples where behavior technology, with a clear behavioral label, is preferred and now gets the nod. But why assume that those in need would continue to look for effective behavioral technologies only under the banner of psychology? If those technologies were to become available only across the hall in another department based on a clearly demarcated discipline, that is where those in real need would go, like it or not. We have made many gains in the efficacy and efficiency of our practices while working under the umbrella of organized psychology, but those skills will remain with us through any reorganization of our discipline.

Proper Respect

Natural scientists of behavior have identified themselves as such on the basis of their convictions about the superiority of their way of thinking insofar as their paradigm supports what to them are demonstrably more effective practices than those informed by alternative paradigms. Perhaps there are people who identify themselves with what they construe to be their *second* best option (in paradigms or anything else), but I cannot recall meeting any of them. So when behavior analysts insist that we should all pretend to psychologists that all paradigms are worthwhile, which of the behavior analysts believe that? In their quest to infiltrate psychology, behavior analysts have apparently put themselves under contingencies to be less than honest about such matters.

When one's clan is dispersed among a huge herd of Wildebeests, one has to behave kindly toward Wildebeests-and respect their ways of doing things-lest the herd be provoked into some kind of clan-bashing stampede. There is a much better alternative available to behavior analysts. They can move across the hall and, from the protective political insularity of their own departments, if it still remains important to do so (although, it probably won't), engage their colleagues of other epistemological persuasions in proper academic and scholarly debate before the public audience-in this case, debate about the relative efficacy of the differing paradigms.

In any such debate, one should accord respect for the equal status of one's cross-corridor colleagues as one's fellow citizens of the academy, but not for their ill-conceived ideas about how best to think. The university exists to provide a grand forum for the comparative sifting of often conflicting philosophies and ideas in behalf of its sponsoring culture. The various

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disciplines represented within the university reflect the views, interests, and concerns of segments of a total population that taxes itself to create such an institution. Within that university there is no equality in effective function among the paradigms by which people operate intellectually nor among comparable products that they produce. The institution exists precisely to put those unequal paradigms, ideas, and products to comparative tests of efficacy.

Within the university milieu, the respect of persons in one intellectual community for the people in another is properly based on respect for the university as an institutional forum for that well-tested mechanism by which to cull intellect and product. One must respect others' rights to be there—the right of all parties to enter into that grand contest of efficacy on a level playing field. One must be fair, and, depending on one's disposition, one may also be kind and gentle and tactful, while some may even be arrogant. But one does not turn oneself into a pathetic hypocrite, compulsively babbling about merit where none exists when an opponent confronts one with a demonstrably inferior proffer. You respectfully layout the arguments, and if the issue pertains to paradigms, you demonstrate both (a) the functional relations between the postulates and the ultimate products of the relevant paradigms and (b) the relative utility of respective products derived through the applications of different paradigms. As Johnston, 1997, (and just about everybody else) has noted, you will not persuade your opponents, **but—to** inject a noteworthy observation—once you are operating across the hall from them instead of from within their own community, those disciplinary competitors no longer constitute your critical audience.

Skinner on Disciplinary Independence

Rakos, early in his article, asserted that Skinner never abandoned his hope that psychology and behavior analysis would eventually become the same discipline. Actually, Skinner oscillated back and forth on that issue, especially in his later years. A somewhat comprehensive review of Skinner's troubled history with this issue is available in a section entitled "Skinner's Oscillations" (Fraleay & Ledoux, 1997, pp. 118-125). Also, an eloquent set of letters exchanged between Jerome Ulman and B. F. Skinner (Ulman, 1993) affords a special insight into Skinner's personal struggle in determining the best organizational structure for the continued development of our discipline.

In Skinner (1993) we find Skinner declaring that:

FRALEY

... we have been accused of building our own ghetto, or refusing to make contact with other kinds of psychology. Rather than break out of the ghetto, I think we should strengthen its wall. No field of science has ever been more clearly defined than this world of ours. (p. 5)

Skinner, in his major address at the close of the 1989 ABA convention (Skinner, 1989), reviewed his own history of changing attitudes on the issue of disciplinary independence. After referring to his earlier acceptance of psychology as the academic home for his discipline, Skinner noted that although he had accepted some principles portending a separate discipline, he nevertheless continued to say:

... 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.

Here are some of Skinner's additional conclusions reported elsewhere in that same 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 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.

On August 17, 1990, the day before he died of leukemia-facilitated pneumonia, Skinner put the finishing touches on a paper (Skinner, 1990) based largely on a talk that he had delivered at the convention of the American Psychological Association (APA) only a week earlier-where he had told the assembled membership of the APA that selection by consequences left no role for a creator or for an initiating self or mind. He called cognitive science "the creationism of psychology" (Holland, 1990). In the subsequent article (Skinner, 1990) based on that talk, Skinner referred to his selection-based science as "behavior analysis," and in the last sentence of that article (apparently the last published sentence of textual material that Skinner wrote) he ended with this: "...but whether behavior analysis will be called psychology is a matter for the future to decide" (p. 1210).

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Reaction to the Article by Edelgard Wulfert (Wulfert, 1997)

Contrasting the Disciplines

Psychology, as Wulfert reminds us, is a collection of subdisciplines. Actually, they can be clustered, and only two major classes are relevant to this discussion. One is based on the assumption that the world is natural and that all aspects of it function through natural processes. According to that basic paradigm, science is focused on the discovery, analysis, and control of those functions. The other general way of thinking is based on the assumption that the physical world exists more or less encapsulated within the envelope of a mystical world. Science under that view is confined to studies of the phenomena that define just the physical part of the world—investigations that may also include the study of any aspects of the physical world that form a communicative interface with the mystical domain (e.g., minds or agential selves). The former view generally informs the natural sciences, the basic cultural expression of which has appeared as physics, chemistry, biology, and more recently, behaviorology. The latter view, prevailing in the remainder of the culture, has informed much of what has occurred under the banner of traditional psychology where the assumed spiritual self is subjected to empirically-based scientific study (focused largely on the nervous system) both for indirect confirmation and for explication of any contributions from the physical world to the behavior-related operations of that mystical internal entity.

Being Scientific and Being Scientistic

Wulfert notes that scholars of both of these epistemological persuasions adduce empirical data to support their arguments, and she claims that doing so meets the criteria for science. In contrast, I have long reserved the term science for such activity only when it is informed by assumptions that preclude any explanatory reliance on mystical variables. I have characterized those who employ scientific methodology to confirm and explicate the implications of their mystical assumptions as being scientistic rather than scientific (Fraley, 1997)—an important and necessary distinction for any emerging discipline whose members would have it mature among the natural sciences. Some people want to insist that worthwhile science, or at least tolerably adequate science, is being practiced by those who marshal the full armamentarium of the scientific research laboratory to seek empirical data.

that may resolve such questions as how many angels can dance on the head of a pin. It may be only a semantic issue. However, I am reluctant to diminish the concept of science in that way, especially in service to the political and social objectives of those who, with the counterfeit coinage of undeserved respect, would purchase home sites in other people's mystical but wealthy disciplinary neighborhoods. "Science" has always implied more than methodology. At least to some of us it implies, additionally, a functional philosophy of science that prevents the waste of powerful scientific methods on nonsensical problems, especially those featuring mystical variables (like autonomous selves manifesting through the cognition of agential minds).

Different people, who respectively employ scientific methods while operating on the basis of different epistemologies, cannot successfully proselytize one another, as Wulfert and most philosophically sophisticated behavior analysts occasionally explain (although that contradicts the usual adherence of behavior analysts to the persuasion bandwagon). To those who committed themselves to the course of proselytization and who have greeted my own similar observations on the futility of proselytizing with rejection, I can only suggest that they start listening to some of the prestigious and well recognized leaders within our own behavior analysis community who know that such a conversion will not occur and, like Wulfert and Johnston, are prepared to explain why, often with supporting citations to the literature, as in this exchange.

Differences that Matter in Important ways

Wulfert implies without being explicit about it, that, in the disciplinary mix of psychology, behavior analysts can coexist and prosper. It is benignly implicit in her opening remarks that the differences among those disciplines are matters of preference having few important implications: They seek their kind of truth; we seek ours; so what? I will not here mount yet another defense of the superior efficacy of the modern natural sciences, which the past three hundred years of human history have so convincingly demonstrated.

The scientific alternative to a natural science discipline of behavior is maintained in this culture, not because it tends to win the inevitable contests of efficacy, but because it provides apparent "scientific" support for certain basic assumptions about the nature of human beings and their behavior—postulates that have been made to seem important to many people for reasons having nothing whatsoever to do with science. When Wulfert observes that

there is no "correct" ultimate definition of what she prefers to call "science," that logically derived truism merely distracts attention from the important way that disciplines *are* to be compared-namely, on the basis of their relative capacities to support effective control of the environment. The proposition that classes of outcomes other than the capacity for control can be of equal or greater general worth is a potentially harmful bromide that incurs a heavy explanatory obligation the discharge of which should be demanded more impatiently.

A More Valid Analysis of the Immature Disciplinary Fixation in Psychology

Both Wulfert and Johnston explicitly reiterated the frequently offered assertion that the kind of science prevailing in psychology supports only the maturation level of understanding (incorporating description and prediction), while, in contrast, the paradigm of the natural sciences (into which behaviorology fits), supports the next level generally connoted by "control" (i.e., the level of effective behavior technology)-an old distinction, long employed by behavior analysts when comparing paradigms. My response to Johnston's casting of it was to ask, rhetorically, why, in our technologically advanced modern culture, any community of well educated and putatively intellectual scholars would cling to a scientific discipline so inferior in its capacity that it supports only the paradigmatic maturation level of "understanding"-especially when, in today's culture, we must go to the fringes to find anyone who cares about a "science" of anything that lacks the potential to support the level of control. Why would psychologists opt for such a impotent discipline, especially after their community has had half a century or more of direct contact with an alternative paradigm that putatively excels in affording precisely the capability for control?

But that is a behavior analyst's framing of the question. The psychologists have persuaded themselves that their mentalistic science does lead, in general, to effective practical actions-persuaded themselves that, bolstered by what are essentially mystical postulates, their psyche-centered discipline does support useful behavior technologies. No one likes to think of themselves as peddling an ineffective product. Nevertheless, Wulfert and Johnston (and large numbers of other behavior analysts) in general, implicitly disagree with the psychologists' view of their own discipline when they argue that it is only to the level of "understanding," and not control, to which the discipline of psychology can mature. If that is true, as they claim, those behavior analysts

should carry their explication a step further to the most obvious implication of that assertion, namely, that any claim by psychologists to practical *science-supported* intervention capabilities must be invalid in spite of what the cognitivists claim. As behavior analysts know so well, merely "to understand" is not "to bring under control."

If it is true that (a) effective interventions can occur only to the extent that the independent variables in the relevant functional relations are brought under control, (b) that traditional psychologists do not rigorously respect that reality in practice, and (c) that analyses of the kind practiced within psychology tend not to lead to explicit identifications of the prevailing relevant functional relations so that the independent variables can be identified and manipulated, then it follows logically that any effective interventions produced through psychological applications must occur mainly under natural contingencies that may go largely unanalyzed as such and which occur in spite of concurrent paradigmatic rhetoric to which any success is then gratuitously attributed. This does not describe just another discipline at the roundtable of disciplines, as these behavior analytic authors seem to imply; it describes a fake discipline.

Furthermore, what of the touted capacity of the psychological paradigm to support the kind of intellectual exercises that lead even to an "understanding," which many behavior analysts seem eager to stipulate? I know of no reputable behavior analyst who would certify as worthwhile what is typically adduced as a psychological "understanding" of a behavioral phenomenon. From the behavior analytic perspective, such psychologically derived understandings are almost always so fallacy laden that behavior analysts have to subject the database to a reinterpretation and derive new and different "understandings" just to arrive at a product that makes sense and is therefore of practical use. Furthermore, those necessary reinterpretations are far more sophisticated scientific exercises than is implied by the disarmingly euphemistic phrases, such as "translation into behaviorese," typically used to describe them. When we advance to the level of behavior technology, which, for practical purposes, is always the point, we find that only the kind of "understandings" (a.k.a. descriptions of functional relations and how to exploit them) that derive from the natural science paradigm will reliably facilitate the control that is the essence of useful behavior technologies.

Behavior analytic authors, discussing their use of psychological understandings, frequently mention this need to "reinterpret" those findings and conclusions "in behavioral terms." Those of us who can interpret English

prose recognize that such language is a euphemistic way of suggesting that the psychological paradigm represents the quest to do good work under what is implicitly the handicap of an intellectual impairment-if we are to judge by the worth of the "understandings" that the psychological paradigm can provide, given that those products have to undergo such behavioral recasting before they can fulfill the only worthwhile role of understandings, namely, to support a capacity for effective intervention (i.e., control). (For a recent exemplary episode see Bower, 1997, and Watson, 1997, and the responses to those articles by Lattal & Shahan, 1997, Marr, 1997, and Cigales, 1997.) When someone tells you that a particular paradigm provides an understanding that, though satisfying to a particular community, cannot contribute support to efforts by the members of that community to intervene effectively-and that any such understanding has to be interpretively recast according to the principles and postulates of another paradigm before that "understanding" has functional worth-that person is implicitly telling you that the original form of the understanding was not worth the paper on which it was described.

Expectations of Disciplinary Change

Two schools of anticipation about disciplinary change seem to contribute to the behavior analysts' hopeful attachment to the psychology enterprise. One view has the behavior analysts waiting out a long interval required for certain influential psychologists, whose seminal works tend to define their discipline, to come so strongly under the natural contingencies in behavior-related circumstances that their work, and the thinking that drives it, will be forced inevitably toward the analysis of functional relations among the real variables that define their subject matter. According to that theory, when those influential psychologists finally come in some natural way to appreciate the functional realities of their own work, that will prove persuasive to them. They, themselves, in response to their own natural effectiveness, will increasingly recognize their own manipulations of functional relations and conclude that the functional basis of behavior is the most important aspect upon which to focus.

Unfortunately, that is not a way in which psychology can change. The impediment inheres in how data are interpreted, regardless of the nature of their sources. Individuals cannot be persuaded by cumulative interpretations of kinds that their epistemological foundations do not permit in the first

place. Internal self-agents are not passively controlled; they *exert* control-an immutable postulate of mentalistic psychology. So environment/behavior relations, no matter how clearly displayed under the person's nose, remain invisible to the person in whom such seeing has never been capacitated-and now cannot feasibly be capacitated. The prevailing functional relations as well as the individual's own *practical* respect for them will go unrecognized by that individual-and should a nearby behavior analyst try helpfully to point to the tightness of the prevailing functionality, it will simply be denied as is necessary to maintain operating room for the self-agent.

Scientific persuasiveness works only on the scientific, so first a scientific person would have to be conditioned. However, the change effects of science do not penetrate to the epistemological level. Thus, science cannot change mystics into scientists. Scientifically adduced data changes the rhetoric and practices of persons who are already scientists, but such a change does not imply change in their scienceness. The prescription both for the conditioning of new mystics or new scientists involves essentially the same kind of conditioning processes (although featuring different subject matter). The natural scientists of behavior enjoy the advantage of being able to describe those processes by which that special class of verbal behavior (a.k.a. personal philosophy) is conditioned. The mystical community, operating intuitively to condition its young people, readily succeeds in rendering most of its young people mystical for life by the age of ten or twelve-a kind of rendering that cannot be altered by scientific evidence, because it pertains to the repertoire by which evidence is interpreted in the first place.

True, after a lifetime, traditional psychologists are replaced by new individuals who, as a result of having spent their formative years in a more modern culture, are initially a little different-perhaps a little less superstitious. However, the pace of that evolutionary process within the discipline of psychology is unlikely to exceed the pace with which mysticism is abandoned by our culture in general. Members of the natural science community certainly hope and expect that two or three hundred years from now the culture will indulge in far less superstition than is now the case. Cultural evolution seems to be on such a track, but it is a slow trend that transcends the current confrontation between the psychology and behavior analysis communities. Furthermore, if our natural science discipline is to lead the evolution of our culture rather than developing only in its wake, then the emergence of an independent natural science of behavior must occur sooner rather than later.

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With respect to how, most effectively, to produce a culture-wide impact, the conceptual expulsion from organisms of what is a fundamentally mystical body-driving agent-like the relegation to redundancy of nature-creating deities-is a task suitable for the independently organized disciplinary agencies of modern natural science, not for a quixotical scattering of data waving infiltrators dispersed within the scientistic enterprise of the mystical majority. While organized psychology may not be subject to evidence-driven change in its postulates, it is subject to supersession from without; and that is done from across the hall by a competitor that, under its own label, provides a reluctant public with a greater capacity for problem solving control of critically important aspects of the environment-the same path to establishment within the culture earlier followed by the other natural science disciplines.

Psychology, as a scientistic disciplinary representative of a way of thinking, is, above all else, constructed to accommodate, or at least to tolerate, the essential mysticism of the ambient culture. That *is* not merely a fundamental feature of psychology, it is a *requisite* feature of psychology. That is the kind of disciplinary representative that members of its sponsoring public want, and the kind that they have long supported. Neither a conspicuous replacement nor an internally instigated remake of that discipline will be tolerated by that large and vigilant subcultural majority merely for the sake of what certain others insist is greater effectiveness in behavior technology. Nor is effectiveness in gaining control over the behavioral environment the most important criterion should a choice of epistemologies ever be forced, although such an antistrategic revelation, especially in public view, is generally avoided at all costs by representatives of organized psychology. They do not admit to sacrificing effectiveness. Rather, they stubbornly deny the existence of a paradigmatic difference in the capacity to support effective interventions. While such a contention, directed to a public generally trained at a high school level, may not succeed if it pertains to physics, chemistry, or biology, public schooling does not include corresponding training in the natural science of behavior, which leaves the public much less discriminating with respect to what is and is not an effective behavior science.

A second view about disciplinary change that seems to sustain the behavior analysts' attachment to psychology relies on the belief that, under increasing cultural pressure to solve behavior problems, the traditional discipline of psychology will eventually fold under the weight of its inherent ineffectiveness. However, if behavior analysts think that in the long run they

can sit in the home of what they privately think is a fake discipline—a position in which they are prohibited from proffering a stark alternative—as that host discipline slowly self-destructs on the shoals of problems that it cannot solve, while they, with their status as boarders and a demeanor of insidious politeness and subtle condescension, progressively replace the flagging science of their hosts, I think it is time for a new look at that prospect. Organized traditional psychology does two things especially well: First, it affects pretenses of substantial effectiveness that are credible to a public that wants to see that kind of pseudoscientific community prosper. Those pretenses are rendered even more convincing when the natural science competitors are dispersed to latency within psychology's own vast ranks—a fact well respected by contemporary psychologists, whether intuitively or explicitly, through their disciplinary management practices (especially their hiring practices). Second, the traditional psychology community propagates at a rate far exceeding that of its natural science intruders and strives to keep it that way.

The only feasible strategy for the behaviorists is to leave organized psychology as alone as possible so that psychology can slowly extinguish on the basis of its own demerits in the face of competition from a starkly divergent and independently well-organized representative of the natural sciences. To facilitate that outcome, we need especially to withdraw the powerful scientific experimental component that behaviorists have bestowed on traditional psychology as wasted tribute and reinstall it within our own discipline where it belongs, and where it can serve the organized natural science enterprise as it should.

Through its early development stages the fledgling natural science community of behaviorists may even profit from the continued existence of the scientific psychology entry into the behavior science arena merely by its being there to absorb the public preoccupation with co-opting the image of science in support of mystical postulates. Organized psychology, huge and with its cultural accoutrement of politico-economic bells and whistles, could afford the small natural science community an opportunity for some benign neglect as the natural scientists step quietly aside (at the glacial pace of such things) to concentrate on the development of their own discipline as one of the basic natural sciences.

The ambient culture is technologically extended well beyond any option to retreat and must now rely on a level of technological productivity that can be supported only by the natural sciences. Most members of the public at

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large confront a private conflict about how to resolve their personal blatant mysticism with the kind of natural science on which their culture is now dependent for quality of life and even for survival. With psychology as the general public's scientific champion strongly entrenched in academia, from which its influences can be propagated to a number of culturally relevant applied fields-and with psychology occupying a salient and predominant place in clinical practice across the face of the culture-the public is somewhat placated on this issue. That is what psychology does, and that is why a scientized psychology thrives. As long as that basic reason for its broad base of cultural support remains in place, traditional psychology will be maintained and its ranks replenished. A small number of behavior analysts, useful as contributors to that basic scientific purpose of psychology, will also be accommodated under its organizational umbrella if they consent to be used in that way in exchange for access to some personally important resources. (Incidentally, that inequitable kind of interaction relation is no more accurately described as "peaceful coexistence" than was that between the dominant white leaders of the old South and the African-Americans who had been constrained to "get along" with them. It is not really a "co-" kind of relation).

Interpreting the Disciplinary History of the Past Half Century

Wulfert seems to foresee a new behavioral revolution in psychology when behavioral clinicians develop new and more effective practices. That line of wishful thinking foretells the retaking of psychological territory lost to cognitively informed practitioners (whose practices worked in the first place only to the extent that they respected behavior-related functional realities, some of which the behavioral community itself is only now discovering and describing as behavioral principles). Wulfert implied that "translating those cognitive practices into behaviorese" can reveal how and why they work, since presumably that would not be clear from the cognitive theories from which those practices putatively arose.

Wulfert attributes the midcentury cognitive revolution to the mere emergence of some clinical practices that seemed to get past some impediments being faced by the behavioral clinicians of those times. Although Wulfert was not explicit about this, traditional psychologists, of many professional specializations, were discovering more of the world of verbal behavior-and putting their usual spin on it to accommodate what to them

is an essential feature, namely, an agential mind. In their excitement about that domain of inquiry, they were producing a multitude of new cognitive theories that were rushed to the front line of clinical application. Some of those new practices resulted in patients making progress. As the smaller behavioral community now brings new science to bear and develops a more powerful behaviorally-based clinical practice, Wulfert seems to hope for a reversal of the cognitive revolution. Here again is faith in the proposition that psychology can be changed merely by demonstrations of greater effectiveness based on a better basic science (although Wulfert herself, like Johnston, had earlier in her paper explained why that can not, and will not happen). It is true that eventually a patient may seek out a behavioral clinician if that kind of clinician appears ready to do a better job, but that represents a change in where clients turn for help, not a change in the discipline of psychology.

The cognitive revolution represented a profound revulsion directed toward a natural science epistemology, the implications of which were slowly but surely coming to be understood by the traditional psychologists. That rejection of the behavioral approach ranged far more broadly across the discipline of psychology than the direct and limited contribution from clinical practice, which perhaps did serve as a catalyst. The spirit of that "revolution" was proclaimed in every facet of psychology, as practitioners in all branches of psychology found ways to announce, with hopeful relief, the death of behaviorism. (See, for example, the typical twist put on the cognitive revolution in relation to behaviorism, as written for student consumption in Biehler & Snowman, 1990, pp. 376-377, in a section entitled "The Origins of Information Processing Theory.") In general, the cognitive revolution represented a somewhat propagandistic repudiation of the natural science of behavior (often dismissed pejoratively as "the *behavioral* view"). However, the transparency of such a *revolution* was obvious to a historian of psychology like Thomas Leahey, whose twitting title for his book section in which he analyzed that period was "the strange death of radical behaviorism" (Leahey, 1997, pp. 454-457).

Wheat and Chaff

The enduring argument of Wulfert and others that only by remaining within the organizational structure of psychology can behavior analysts engage in valuable intellectual cooperation and scholarly cross-fertilization implies incorrectly that that cannot be done from across the hall. Within universities

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we are all free to follow the activities occurring in other disciplines, to read the literature of other disciplines and to attend their meetings-and, in general, to work collaboratively with the members of other disciplines. That sort of interdisciplinary activity is heavily encouraged within universities. The potential loss that many behavior analysts fear is probably not critical collegial interactions but the share of organized psychology's physical and monetary resources that, by proactively allowing themselves to be called "psychologists," behavior analysts are permitted to claim. If those putatively worthwhile collegial interactions decrease with disciplinary independence as Wulfert seems to fear, that will happen because they are not really so worthwhile after all, because those relations are easy to maintain if anyone really cares about them.

Some of us recognize that an independent discipline is required for the right to organize our own comprehensive training programs. We realize that pretending to take seriously an epistemological mode that cannot adequately support the ultimate maturation level of a science can adversely affect one's own intellect. We also question the motives of those who, for arguable reasons, would have us purchase our disciplinary resources with the currency of our scientific integrity and capacity. Our concerns may be dismissed with the invidious implication that we are merely distraught because, as the socially unskilled individuals that we are alleged to be, we have not played the infiltration game as successfully as others and consequently have gotten punished. Resort to that diversionary smoke screen has worked well in the past to distract our community from the important issues in this debate. True, many behavioral people have suffered injustices and career setbacks, and their behavior often reflects that history. But pointing vigorously away toward that relatively less important aspect each time attention is focused on the precarious status of our discipline-with the expectation that doing so will work once again-is to gamble once again that the audience is that easily distracted, a wager that earns a relevant paraphrase: You can distract all of the behavior analysts some of the time, and some of the behavior analysts all the time, but you can't distract all of the behavior analysts all of the time.

Discussion

The organized discipline of behavior analysis is a house of cards precariously organized around an intrusion into the homeland of an antithetical community-and around the lies, deceit, and hypocrisy

necessarily attendant to such a proselytical infiltration. We occupy settlements on territory rightfully belonging to another people whose basic assumptions are incompatible with those of behaviorists in particular and natural scientists in general. To castigate those of us who take note of the susceptibility to collapse inhering in such a construct is superficial foolishness that only distracts the behavior analytic community from a tragically belated analysis of its historical mistakes.

The long proffered rationale positing a behaviorally inspired epistemological metamorphosis of that host community features an impossible goal—a fictional screen deceiving both self and others behind which multitudes of behaviorists have disguised their self-serving professional accommodations and hidden the cost that they have paid in the currency of their servitude. The collapse of something that precarious is inevitable. It can be allowed to occur as a destructive calamity or it can occur as a constructively managed reorientation of our cultural mission.

Of necessity, the culture has had to create a niche for the natural sciences, and it is there that our discipline fits. We weaken the natural science establishment by remaining organizationally apart from it. That is where the home base for our discipline can and should be developed across the time spans required for such things. Like physics, chemistry, and biology, behaviorology has its own level of analysis and its own class of natural phenomena upon which to focus its studies. By its nature, and certainly by its importance to the human condition, it is one of the four basic natural science disciplines—and the move to install our discipline among the natural sciences has already begun. How long the Association for Behavior Analysis will continue to waste its place in history on its monumental false start remains an unresolved question.

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FRALEY

Editor's Note

The following email message, received shortly before this issue went to press, highlights the relevance of this discussion:

From: "Lawrence E. Fraley" <lfraley2@WVU.edu>
To: Janet Ellis <ellis@scs.unt.edu>
Date: Tue, Aug 18, 1998 3:02 PM
Subject: Follow-up

Dear Janet,

[Two paragraphs dealing with other matters have been omitted.]

... I was distressed to hear reports that a new Provost at Western Michigan University has decided to correct the substantial behavioral skew in the membership of that university's psychology department and reportedly has begun the long readjustment process by refusing approval for the employment of any more behavioral faculty members in that department. (I was told that former Western Michigan graduates are being contacted with requests to mount a letter writing campaign in support of the current configuration of the faculty and training programs of that department.)

I was reminded. . . of the substance of my July 8 cover letter to you in which I reviewed the perilous situation of even an independent department such as yours. Yet, in spite of the vulnerability inhering in your experiment with independence at [University of North Texas], a behaviorally predominated psychology department, as in Western Michigan's case, is far more vulnerable. It is tragic that the behavioral faculty members at Western Michigan have apparently ignored the adverse implications of their own strongly advocated position: Their steadfast insistence over many years that the discipline of behavior analysis is just a part of psychology implies logically that their psychology department should reflect the proper balance in its faculty that must characterize any general university department of psychology. Such a department should reflect in its faculty ratios the same sub-discipline ratios that prevail in the field of psychology at large. That is the proper position that any astute university administrator should take with respect to any of the fields represented among the academic units of the university. Because explicitly behavioral people constitute only about five to ten percent of psychologists at large, it follows that a properly balanced thirty-member psychology faculty should have no more than three behavioral members.

What the Western Michigan University administrators see at their institution is a grossly distorted psychology department that somehow has been politically captured by one of the smaller and therefore implicitly less important factions within organized psychology. The job of the administration in any large general service university is to prevent precisely that sort of extreme concentration in any of its general service academic departments. What has been allowed to go unchecked at Western Michigan represents a lapse in what most administrators would regard as

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proper management of the university by its central administration-a lapse that may now be getting some remedial attention.

Seldom has there been a group more damned by its own rhetoric than the behavior analysts who have long insisted that they belong in psychology. Within psychology, the proper place for them, according to the criteria by which university administrators must make such determinations, is small and of limited importance. Perhaps the threat to psychology-adhering behavior analysts of having their flagship department dismantled for reasons against which they will have difficulty arguing-coupled with the current round of articles that address the issue of disciplinary independence-will stimulate some new and more carefully considered debate of how best to insure the integrity of our natural science discipline.

Best regards,

Larry