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Behaviorology

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ABOUT BEHAVIOROLOGY

BEHAVIOROLOGY IS AN INDEPENDENTLY ORGANIZED DISCIPLINE FEATURING THE NATURAL SCIENCE OF BEHAVIOR. BEHAVIOROLOGISTS STUDY THE FUNCTIONAL RELATIONS BETWEEN BEHAVIOR AND ITS INDEPENDENT VARIABLES IN THE BEHAVIOR-DETERMINING ENVIRONMENT. BEHAVIOROLOGICAL ACCOUNTS ARE BASED ON THE BEHAVIORAL CAPACITY OF THE SPECIES, THE PERSONAL HISTORY OF THE BEHAVING ORGANISM, AND THE CURRENT PHYSICAL AND SOCIAL ENVIRONMENT IN WHICH BEHAVIOR OCCURS. BEHAVIOROLOGISTS DISCOVER THE NATURAL LAWS GOVERNING BEHAVIOR. THEY THEN DEVELOP BENEFICIAL BEHAVIOR-ENGINEERING TECHNOLOGIES APPLICABLE TO BEHAVIOR RELATED CONCERNS IN ALL FIELDS INCLUDING CHILD REARING, EDUCATION, EMPLOYMENT, ENTERTAINMENT, GOVERNMENT, LAW, MARKETING, MEDICINE, AND SELF-MANAGEMENT.

BEHAVIOROLOGY FEATURES STRICTLY NATURAL ACCOUNTS FOR BEHAVIORAL EVENTS. IN THIS WAY BEHAVIOROLOGY DIFFERS FROM DISCIPLINES THAT ENTERTAIN FUNDAMENTALLY SUPERSTITIOUS ASSUMPTIONS ABOUT HUMANS AND THEIR BEHAVIOR. BEHAVIOROLOGY EXCLUDES THE MYSTICAL NOTION OF A RATHER SPONTANEOUS ORIGINATION OF BEHAVIOR BY THE WILLFUL ACTION OF ETHEREAL, BODY-DWELLING AGENTS CONNOTED BY SUCH TERMS AS *mind*, *psyche*, *self*, *muse*, OR EVEN PRONOUNS LIKE *I*, *me*, and *you*.

AMONG BEHAVIOR SCIENTISTS WHO RESPECT THE PHILOSOPHY OF NATURALISM, TWO MAJOR STRATEGIES HAVE EMERGED THROUGH WHICH THEIR RESPECTIVE PROPONENTS WOULD HAVE THE NATURAL SCIENCE OF BEHAVIOR CONTRIBUTE TO THE CULTURE. ONE STRATEGY IS TO WORK IN BASIC NON-NATURAL SCIENCE UNITS AND DEMONSTRATE TO THE OTHER MEMBERS THE KIND OF EFFECTIVE SCIENCE THAT NATURAL PHILOSOPHY CAN INFORM. IN CONTRAST, BEHAVIOROLOGISTS ARE ORGANIZING AN ENTIRELY INDEPENDENT DISCIPLINE FOR THE STUDY OF BEHAVIOR THAT CAN TAKE ITS PLACE AS ONE OF THE RECOGNIZED BASIC NATURAL SCIENCES.

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

Volume II Number I Contents Plan

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

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

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PROVIDE AT LEAST MINIMALLY PEER-REVIEWED ARTICLES AS WELL AS, ON OCCASION AND WITH EXPLICIT DESIGNATION, FULLY PEER-REVIEWED ARTICLES. THEY WRITE ON THE FULL RANGE OF DISCIPLINARY TOPICS INCLUDING HISTORICAL, PHILOSOPHICAL, CONCEPTUAL, EDUCATIONAL, EXPERIMENTAL, AND TECHNOLOGICAL (APPLIED) CONSIDERATIONS. PLEASE JOIN US—IF YOU HAVE NOT ALREADY DONE SO—AND SUPPORT BRINGING THE BENEFITS OF BEHAVIOROLOGY TO HUMANITY. (CONTRIBUTIONS TO TIBI OR TIBIA ARE TAX-DEDUCTIBLE.)☪

Personhood & Superstition Part IV (of IV)

Lawrence E. Fraley

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[Presented here is the fourth of four related works. These works are (a) "The Nature of Personhood," (b) "More Implications of Misconstrued Personhood," (c) "Cultural Investment in Superstition," and (d) "Behavioral Engineering to Reduce Superstition." These four pieces are all excerpts from parts of "Person, Life, and Culture," a later chapter of the author's book, *General Behaviorology: The Natural Science of Human Behavior* (Fraley, in press). The relevance of these pieces to managing improvements in ongoing cultural concerns increases their interest to readers of this journal. The four pieces are presented, one at a time, in consecutive issues beginning with the Spring 2006 issue (Volume 9, Number 1).—Ed.]

Behavioral Engineering to Reduce Superstition

A culture, recast to be relatively free of superstition, would be substantially different from our present culture, requiring a population that was both more educated and differently educated especially with respect to philosophy and science. Its people would have to be schooled explicitly in the qualitative analysis of knowing.

Clearly, much contemporary recourse to superstition is culturally fostered. However, debates about the potential efficacy of superstition often beg the question of whether superstitious behavior is also part of the biological legacy in addition to occurring as a mere artifact of cultural mismanagement. Let us critically review some of the characteristics of superstitious behavior. First, in the absence of objective accounts based on objective evidence and logic, superstitious alternatives often manifest as negatively reinforced escape behaviors. Superstitious explanations, though almost always invalid, are typically convenient. For example, such pseudo-explanations often reduce the social aversiveness of a current situation that is characterized by strong mands for answers. Furthermore, superstitious accounts may deflect the continuing press for a more detailed accounting by conceptually shifting the independent variables into a mystical realm in which logic and objectivity tend not only to be rendered useless but may be regarded as indecorous.

In a superstitious explanation the putative independent variable may, from a natural science perspective, seem to be conspicuously deficient in functional capacity, and more objective people may describe explanatory recourse to such a variable as preposterous. Nevertheless, that kind of account may persist in strength, because, as a behavior of escape from socially and perhaps biologically imposed aversers, it affords great relief. However, given that many such pseudoexplanations would be regarded as absurd when initially presented to most people with contemporary sociocultural conditioning, people require extensive programs of special conditioning to maintain a susceptibility to them. Thus, the kind of relief that such false accounts represent is typically available only to those who continue to undergo the kind of extensive and self-deceptive conditioning that can seemingly rationalize what at first blush is obviously illogical and perhaps foolishly unsophisticated or childish.

In one common class of resort to superstitious accounting, real events are presented in relation to mysterious independent variables that have been custom-conjured to seem as if they complete an accounting for the event of concern. In some cases a fictional variable is posited as a part of the external environment. Consider, for example, this statement: "The pilot of the search plane spotted the tiny and obscure crash site from seven miles away, only because *from on high a compassionate God directed the pilot's gaze precisely in that direction.*" In other cases, the fictional independent variable is cast as a mysterious internal behavior—originating source. For example, "the pilot of the search plane spotted the tiny and obscure crash site from seven miles away, *only because of that pilot's unyielding determination.*" Through scholarly academic pursuit of the former kind of pseudoexplanations one can become a *Doctor of Divinity*, while with scholarly academic pursuit of the latter kind of pseudoexplanation one can become a *Doctor of Psychology*. Both career options are currently available at reputable institutions.

In idealized practice of the kind that is governed by natural science, the absence of a reliable and valid explanation is carefully acknowledged. In a natural science community, respect is garnered by those who resist superstitious explanations and instead conspicuously delineate the problems that have not yet been solved (i.e., the dependent variables for which measurable independent variables have not yet been identified). That they exist *to* be identified is a basic element of the prevailing natural philosophy. According to the rules of citizenship for the natural science community, a scientist who cannot yet account for a detected phenomenon is not to be treated aversively merely because such verbal behavior is not forthcoming, and a good reason exists for avoiding doing so: Our goading a scientist into inventing false accounts as a negatively reinforced escape behavior leaves

us with unreliable if perhaps temporarily satisfying pseudoexplanations.

Within a scientific community the prevailing practice is to reinforce as strongly as possible the proffer of objectively rendered accounts for the phenomenon under examination. Such accounts qualify for reinforcement because, insofar as they feature measurable independent variables and are therefore formal explanations (as opposed to pseudoexplanations), they are at least potentially valid. Reinforcement that is contingent on the intrinsic quality of such a verbal product is in proportion to the degree to which specified changes to the indicated independent variables in such products have, in previous cases, led to predictable effects on the dependent variables of concern. That is, they are the kind of verbal descriptions of relations (a) that on past occasions have reliably supported effective interventions and (b) that can be subjected to practical tests. For those reasons objective accounts have value within scientific communities.

Scientific accounting is a progressive process, but at no stage is any step based on superstitiously informed assumption nor can it involve mystical variables. No matter how tentative a preliminary scientific account may be, it is based logically on objective evidence all variables of which are subject to measurement. A complex phenomenon typically manifests via many functionally related variables that comprise its properties and effects, but at the outset of its investigation, attention may be evoked by only a limited subset of those variables—perhaps by only one of them.

For instance, early in the twentieth century a geographer looking at a relief map of India and the adjacent Himalaya Mountains could have noticed that those mountains appear on the map as if they were wrinkles in the crust of the earth that could have been formed by a collision of India smashing into the Asian continent. Such an observer might then have proposed that continents can and do move, albeit on the basis of very limited but objective evidence.

The limited evidence in such a case would pertain to variables pertinent to but a single kind of effect of the hypothetical phenomenon. Among scientists such a speculation, objectively yet flimsily supported, might be called a *hypothesis*. If it is correct, there will be many other kinds of effects of the posited phenomenon plus its intrinsic variables—all subject to measurement and confirmation. With repeated confirmation the respective relations in which those variables are involved come (in what is fundamentally an economic process) to be called *facts*.

As the number and variety of such measurements increase, and the results continue to comport with the original hypothesis, the proffered explanation begins to be called a *theory*. A theory is an account that is based on

a logical, coherent, and often expanding set of facts, and supports reliable and testable predictions.

Across what is deemed to be a generally sufficient number of confirmations via measures of additional relevant variables, especially direct measurements of the *intrinsic* variables of the central phenomenon, that particular account (i.e., the theory) will come to be regarded as reliable to a correspondingly extreme degree. That particular account may then be reclassified cautiously from *theory* to something with a more absolutistic connotation (e.g., a *fact*: the fact, not theory, of continental drift). However, because that shift in classification is according to arbitrary criteria, natural scientists often continue conservatively to tact a particular account as a *theory* long after others would entertain that kind of shift in the name of its class.

Thus, through the communal practices of reinforcement among natural scientists, those who do the science are kept under contingencies to discover and describe real and relevant variables plus any relations among them. Under that kind of community management the practitioners of science tend to avoid resorting to superstitious shortcuts. They eschew the proffer of fictional “causes” (unreal independent variables) that have been crafted imaginatively and alleged to be endowed with precisely the functional capacity that real but as yet undiscovered independent variables would have to possess to produce observed effects.

In contrast with the social practices that share in defining the integrity of scientific communities, in the culture at large, as it has evolved, people have not been as careful to avoid inflicting aversive stimulation on those who have been without ready explanations for phenomena of common interest. Outside of the natural science community people, and especially leaders, to whom objective accounts for events of common interest fail to occur are much more routinely subjected to aversive treatment, often in the form of ridicule. Those who seem to lack ready explanations typically experience the kind of social treatment that is reserved for dull, uninformed, and uninteresting people. In that kind of social milieu prestige accrues to individuals in proportion to their quick production of seemingly plausible explanations, and respect for people is, in general, proportional to their apparent explanatory repertoire. Recourse to superstition is rendered much more convenient when audiences carelessly fail to distinguish between superstitious explanations and those that delineate functional relations among real (measurable) variables.

Thus, to reduce the general explanatory reliance on superstition within the culture, people should be trained to withhold punishment in the mere absence of explana-

tions, especially with respect to difficult problems.¹ People should be schooled in the nature of explanations per se, and rendered more discriminating with respect to the qualitative features of different classes of explaining. A particular class of appropriate social reinforcement could pertain to the quality of proffered accounts with the reinforcers reserved for the kind of reliable and valid explanations that feature functional relations among measurable variables.

Given pressure to produce an account, the cure for a person's ignorance is not the imposition of aversive stimuli when that person holds silent, a procedure that negatively reinforces the proffering of invalid explanatory contrivances. As typically explained in the invalid terms of purpose and agency, people tend to offer nonsensical explanations to avoid the ridicule and dismissive neglect that are reserved for those who seem not to grasp the intricacies of currently relevant issues.

Instead, science education is the valid and worthwhile intervention, especially the study of science per se (recall that there is a natural science *of* science). Cultural wisdom posits that more comprehensively and appropriately educating people prepares them to offer increasingly reliable and valid explanations. As people often express it in common parlance, "the person with the broader range of integrated knowledge is better prepared to explain things correctly." By shifting the prevailing perspective from the domain of mentalistic superstition to the world of reality, we can restate the previous social lore in more valid terms: First, through natural science studies of a wider variety of phenomena, and then through general education in the methods of natural science per se, especially

¹ Recall from earlier chapters that, in a correct technical sense, it is not the offending *person* that gets punished in such cases of social miscarry, nor is it the *absence* of behavior that gets punished. Rather, technically, the behavior that actually occurs (in these cases, as an alternative to a valid account) is the object of the punishment. Suppose that a potentially valid (functional) account is not forthcoming, and some alternative behavior occurs instead (e.g., a statement such as "I don't know how or why the event of concern has occurred"). It is that alternative behavior that gets punished if aversive stimulation is subsequently applied. We would measure the effectiveness of that punishment in terms of changes in the frequency of that alternative behavior across subsequent occasions. If the punishment has been effective, the verbalizer may simply remain more quiet, but that precludes access to the positive reinforcers reserved for seemingly knowledgeable behavior. Therefore, the verbalizer may exhibit pseudoexplanations, which, if seemingly valid, both avoid further punishment and are followed by the reinforcing consequences that community members reserve for seemingly valid accounts.

from the behaviorological perspective, people not only come to respond to a wider variety of relevant independent variables, they also become conditioned to describe with greater accuracy the behavior-controlling functional relations the establishment of which is via the interactions of those variables. Explanations that are proffered by people who are better educated in those ways tend to be more comprehensive both with respect to what is studied and to how it is studied.

The greater capacity of natural explanations for practical effectiveness leaves alternative superstitious accounts subject to relatively less nonsocial reinforcement. That is, superstitious accounting for events leaves people with no practical indication of how to control or how to improve the existing control that is exerted by those events. Thus, people are unable to produce more effective and hence more reinforcing outcomes on the basis of superstitious accounts. Functional accounts, on the other hand, indicate the variables upon which to focus interventions that will yield more practical reinforcing effects. However, people may continue to behave only intuitively in a practical situation and thereby produce intrinsic natural reinforcing consequences while at the same time misdescribing the actual functional relations in terms of irrelevant superstitious nonsense. In that case no verbally mediated improvement occurs in the control of those peoples' ongoing practical behavior (i.e., any improvement must rely exclusively on the consequences of the behavior rather than on verbal supplements to its antecedent controls). On the other hand, when people objectively produce valid functional accounts of what is occurring, that verbal behavior can share in the antecedent control of their ongoing practical behavior in ways that do enhance its effectiveness.

We may note that, with more education in the natural sciences, a person's explanations of events become less superstitious, a trend driven by the greater practical effectiveness of the outcomes to which those valid explanations contribute. That is, natural accounts tend to be more effective and hence ultimately more reinforced in natural ways. In such cases, problem solving through reliance on contrived pseudoexplanations tends to extinguish under a natural process of differential reinforcement, which selectively increases the more effective kind of accounts. Practical effectiveness increases through the manipulation of the relevant independent variables, and it is precisely those variables that are specified in valid accounts. That is why people often tend to be less superstitious in their analyses of phenomena that are critical to their survival or even to their general well-being. As it is commonly said, if it is of critical important that people get things right, they cannot afford to behave superstitiously.

Let us look more closely at practical personal effectiveness. Practical personal effectiveness tends to con-

note contact with the intrinsic natural reinforcers of one's behavior, and that contact may be improved when one more effectively manipulates the relevant independent variables under stimulus controls that are being supplemented by valid verbal accounts of the relevant functions. The phrase "being able to describe how best to do things" alludes to the verbal behavior that shares in the control of the manipulative or contriving behavior by which one's contact with critical variables is enhanced or important outcomes are produced.

At the same time, if those valid verbal accounts are publicized, other people may be providing social consequences of those publicized functional explanations. Those social consequences may either reinforce or punish those accounts, depending on the social practices of the community. Members of a natural science community will tend to reinforce accounts that feature two real variables and the functional relation between them, and so may members of the general community at large. But if the general community at large is indoctrinated with superstitious assumptions about the phenomena of concern or about the nature of human beings and their behavior with respect to those phenomena, then valid functional accounts, when publicized, may be subject to suppressive social punishment.

As a result, those punished accounts may be publicly suppressed but remain extant in private, in which case, they can continue to contribute to the improvement of the person's practical behavior. On the other hand, those valid functional accounts, when socially punished, instead of continuing in private may come to be supplanted by superstitious alternatives (in which case the person is said to have started *believing* those superstitious alternatives to the valid explanations). Natural scientists who have been fated to live in the midst of heavily superstitious cultures have in some cases continued their naturally reinforcing scientific work while its public presentations undergo a social punishment-driven suppression.

Practical effectiveness aside, if invalid superstitious accounts, when proffered, are reinforced socially with sufficient strength, the social reinforcers may exert more functional control over the superstitious accounting behavior than the effective practical outcomes exert on the alternative practical behavior that may follow from more valid accounts. Some degree of practical effectiveness with the matters at hand is thus sacrificed in favor of the enhanced social status that remains contingent on what may become ostentatious displays of superstition. That is a somewhat common occurrence, and examples may be noted when a community becomes well organized around an invalid ideology. A community member's conformance to the superstitious ideological foundations of such a community may then yield social reinforcers that are stronger than the natural reinforcers that could be

contacted through the production of better outcomes in the practical work that is being performed.

For example, the work of many classroom teachers remains informed by mistaken notions about the nature of behavior and how it can be changed most effectively. Although the practical outcomes of their misguided teaching, as manifesting in the behavior of their students, remain limited and of reduced reinforcing effect in the practical arena, those teachers, in many cases, tend not to adopt more effective alternative practices. The social reinforcers of their misguided practices that are supplied from their ideologically informed professional community are sufficiently strong to prolong the maintenance of their faulty approach. That is, the community that trains them in that faulty approach continues to supply the reinforcers that maintain their faulty practices. In common terms, such practitioners proceed happily with the comfortable knowledge that they are on the right track even though an objective review of their results would suggest otherwise.

Cultural analysts have long noted that as scientific explanations are advanced, superstitious accounts retreat. The organized natural science subcommunity has played a major role in promoting that kind of progress. However, other comparably well organized subcommunities maintain their integrity through recourse to superstition and therefore defend their investment in that approach, although in doing so they are seldom explicit about the superstitious nature of their foundations, which they tend to describe euphemistically. Arguably such subcommunities remain extant, because the culture at large has made too little progress with the comparative qualitative analysis of different ways of knowing.

Mere demonstrations that a more functional kind of accounting leads to more effective outcomes, no matter how compelling, often prove insufficient to make important headway toward a more intellectual culture. The scientific community often tends to present its products while de-emphasizing its methods and perhaps more so its philosophy. According to that prevailing social theory, the value of those products will instill respect for science, while science per se, including its natural philosophy, is still not generally understood. That approach may leave people respecting "science" but unprepared to comprehend what they are respecting. Thus, they may admire their "scientific" culture and relish the improved quality of life that it affords while attacking as intrinsically evil the tenets of naturalistic philosophy upon which science is established.

With respect to any phenomenon of concern, scientific practitioners work to account for that phenomenon by discovering the relevant measurable variables as well as the functional relations among those variables. Absent apparent independent variables, scientists assume that real ones exist to be discovered, and they tend to persist

until they find them. In that way scientists trace the functional history of the current events that define the phenomenon of concern. Ultimately, through that analytical procedure, those scientists become able to intervene among what will become the functional antecedent variables that control future events. Thus, scientists gain constructive control of future manifestations of the phenomenon of concern. Improvements in the control of that phenomenon may have practical implications of great importance to people far beyond the scientific community. Everyone, not just scientists, may benefit. Yet absent explicit instruction in the field of qualitative knowing, people in general tend not to appreciate why functional accounting should hold the promise of such an advantage over other highly touted kinds of accounting, especially the kinds that feature relations that lack an objective basis and must be maintained superstitiously.

Even people whose work serves as a paradigm for natural science can become careless in their general defense of the scientific approach beyond their own specialization. Arguably, superstition has no valid role in the methods of investigation that occur under contingencies to produce practical outcomes. Natural scientists generally tend to regard those who share their particular specialization as an unsuperstitious lot, especially with respect to their respective scientific activities. Furthermore, within a subcommunity of natural scientists who engage in a given specialization, superstitious accounts pertinent to events in that specialization earn disrespect and tend to be punished in ways apropos of bad citizenship.

However, for some people the contingencies that share in reducing the superstitious behavior when they work as natural scientists may have little effect on their superstitious behavior with respect to equally natural phenomena that lie beyond the scope of their own scientific specializations. For example, consider a corporate-employed chemist who spends a career in the laboratory developing a family of organic compounds that exhibit a special set of commercially important properties. When confronting a class of natural phenomena that differs substantially from the chemical events with which that person is scientifically familiar—for instance, the origin of the earth—that chemist may entertain somewhat primitive and simplistic superstitions, perhaps by insisting irrationally that the world was created rather abruptly in the remote fringe of the human historical record by the miraculous intervention of a powerful deity.

Geologists or cosmologists, on the other hand, could not afford the implications of such an unwarranted assumption. Within a geological or cosmological community any exhibition of that particular superstitious idea would be subject to extinction. In addition, it would probably be subject to social punishment. However, a geologist or cosmologist may openly assume that organic

bodies miraculously initiate in some creative spontaneous way at least some of the behavior that they exhibit. Such an assumption, if publicized in that subcommunity, may not be subject to reduction via aversive consequences.

Our analysis of the inconsistencies in the scientific perspective of individuals from across the natural science community in general must shift to the philosophical level of consideration. Natural philosophy informs the scientific work of natural scientists in the functional sense of imposing a kind of intellectual quality control on their work and its products. However, not all of those who pass as natural scientists have been conditioned to extrapolate that kind of quality control to problem solving *beyond the bounds of their own scientific specializations*. To observers of the broader range of their behavior, they seem intellectually inconsistent.

Perhaps because of gaps in their training, they may never have attained the abstraction of describing verbally to themselves what is called the general *philosophy of natural science* nor been led to consider explicitly the functional role of such philosophy in relation to scientific activity in general. Their philosophical behavior, to the extent that it can be identified, may occur more intuitively than explicitly. Their scientific work may be largely rule-governed, proceeding mostly according to prescribed method. Arguably, such people work more as rule-governed technicians than as philosophically informed scientists.

Furthermore, such practitioners may never have declared publicly that naturalism is their personal philosophy in general, which would have facilitated the science community's provision of more precise social consequences with respect to their intellectual consistency. They may never have analyzed the real or potential functional relations between the verbal manifestations of natural philosophy and the broad ranging remainder of their behavior. While the style of their own professional work may comport intuitively with a natural philosophy as a result of the contingencies and prescribed work habits that prevail in their own workplaces, they may remain largely unacquainted with natural philosophy in an explicit verbal way, which hinders its generalizations. Thus, they may fail to render naturalistic interpretations of the scientific activities and results from other fields in which natural scientists are at work. That is, the quality-controlling effects of natural philosophy do not come readily to bear on the many facets of their behavior that lie beyond the scope of their somewhat narrow professional specializations, including, in some cases, the methods and products of natural scientists who work in other fields.

When in situations outside of their own relatively narrow scientific purview, where the prevailing natural contingencies that support scientific purity are less stringently imposed than in their own specialization, they may behave uncritically in blatantly superstitious ways. Joining

with others who do so, whether defensively or to proselytize, such narrowed scientists may formally or informally organize sociopolitically to impose on others a respect for their superstitious conduct and its implications. Such activity may even be directed at natural scientists who work in other fields, and that superstition-based meddling may inhibit the scientific work in those fields.²

Implicit in such inconsistent activity on the part of some scientists is the need for all natural scientists to be well trained, not merely in the methodology of their own specializations, but especially in the nature of natural science per se. The subject matter of that training must include the functional role that is played in general by philosophical assumptions and then, in particular, by those of scientific naturalism. Trainees in any science specialty must be prepared to describe explicitly the functional role of the philosophy by which their work is quality-controlled and to exhibit the generalization of those qualitative principles, which characterize the natural scientific perspective, to all natural science fields. That is essential to the integrity of the broadly construed natural science community.

The behavior of both mystics and natural scientists is informed by philosophical assumptions, but as revealed across the course of the book [Fraley, L.E. *General Behaviorology: The Natural Science of Human Behavior*.—Ed.], the quality of an assumption is a function of its kind of origination. Therefore, students in any natural science would be well served by describing accurately how the philosophical assumptions of naturalism have been derived inferentially from the results of general objective practice in the first place. Scientists in training must become prepared to contrast that kind of derivation with the ways in which the superstitious alternatives have arisen. Philosophy does not manifest spontaneously. It has a history, and how a particular set of philosophical assumptions has arisen has much to do with the quality of the intellectuality that those assumptions can support.

If the general objective of scientific training is to reach beyond the methods of a particular specialization and produce a comprehensive natural scientist, one who may practice within a specialization but who also appreciates natural science in general, then the training must reflect that broader goal. Such training should begin with the nature of science in relation to its alternatives, the nature of the philosophy of naturalism in relation to its alternatives, and the general nature of the functional relation between the philosophical and scientific repertoires. Students should be able to describe the qualitative differences among different kinds of knowing in terms of both origins and implications. As a result of having been trained to that extent, a person's place in the class of natural scientists should primarily connote a high quality of intellectuality that pertains to, but also transcends, activity in the person's specialization. Under that training approach a trainee preparing for a career in a natural science field would also become a connoisseur of good science in general, and that characteristic would be utmost among discriminative qualifications for full citizenship in the natural science community.

References

- Fraley, L.E. (in press). *General Behaviorology: The Natural Science of Human Behavior*. Canton, NY: ABCs.
 Skinner, B.F. (1953). *Science and Human Behavior*. New York: The Free Press.☞☛

² For example, a person who works as a meteorologist or as a geophysicist may join a coalition that is mounting a political campaign to stop embryonic stem cell research on the grounds that such scientific programs destroy worthwhile lives. Perhaps that science-employed individual has become convinced that, because some people are good, it is good to produce as many people as possible, and therefore embryos should not be diverted for other purposes. Or perhaps it is assumed that even such an inchoate human body is sacred because it has already been possessed (or is soon to be possessed) by a soul dispatched by God to take up the stewardship of that body. Stem cell research may be curtailed when natural scientists who pursue that line of work are subsequently criminalized.

Origins, Status, and Mission of Behaviorology Chapter 4 (of 7)

Lawrence E. Fraley
Stephen F. Ledoux

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

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

the culture and in the cultural marketplace. It also comparatively explores the different levels of analysis characteristic of the existing behavior-related natural science disciplines, and examines the cultural basis of resistance to behaviorology.

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

Chapter 4:

The Transition Period: Organizing the Discipline & Developing its Infrastructure

After the introductory Chapter One, Chapter Two of this account of the emergence of behaviorology examined the nature and origins of the behaviorology concept. Chapter Three examined contingencies supporting individual commitments to a disciplinary independence movement. This chapter, Chapter Four, presents a comprehensive review of the subsequent activities to organize the behaviorology discipline. It examines the cultural engineering by which the newly named discipline was formalized, rendered operational, and debuted in the scientific community at large.

*B*y the late 1980s, on the basis of differences in stimulus controls, verbal repertoires, and reinforcers, the followers of both disciplines, psychology and behaviorology, could validly acknowledge that dual memberships in these disciplines impose unrealistic demands at least on trainees. Not only had the task of bringing one's behavior under control of both subject matters become too demanding, but the contradictory nature of those two basic analytical approaches was continuing to evoke mutual rejection of their repertoires. Behaviorologists could not include the concepts of mainstream psychologists in their work, because to behaviorologists mentalism is non-natural, and nerve functions seem both irrelevant and part of a yet another discipline. For economic as well as paradigmatic and philosophical reasons, psychologists could tolerate, in the contexts of their discipline, little work designated as behaviorological (or any name of similar connotation) regardless of whether or not they claimed that work. As Murray Sidman, responding to a

question from the floor during a panel discussion, told the 1986 convention of the Southeastern Association for Behavior Analysis, a separation of the science of behavior from psychology should have occurred some time ago.

Personal Steps toward Independence

In 1989, with Julie Vargas as president, the Association for Behavior Analysis (ABA) sent a questionnaire (see ABA, 1989) to its 3,460 members; 560 (just over 16%) responded. One item asked all members to indicate the “conceptual orientation” that best described their position. Choices included an assortment of popular terms and phrases without regard to their overlapping meanings. That item (with the number of responses to each category) included cognitive (4), cognitive-behavioral (56), interbehavioral (20), behavioral (264), radical behavioral (180), and behaviorological (15), (plus “other” and “NA”—not applicable [25]). For the first time, the already self-identified behaviorologists in ABA at that time were, as a group, afforded the opportunity to indicate their discipline publicly. More importantly, the item revealed to the ABA membership in general that a new and different disciplinary option had become available to them.

Some subsequent ABA membership forms also requested explicit self-identification of disciplinary affiliation. On the membership form used to develop the 1991 ABA Directory, 122 *non-TIBA* (The International Behaviorology Association) members of ABA selected “behaviorology” as their disciplinary identity (later forms listed options less explicitly). This large number surprised the much smaller group working to establish behaviorology as an independent discipline. One speculation held that many ABA members, preferring not to be identified with psychology as their basic discipline, were merely responding to the name without knowing its implications or connotations. To resolve that issue, a letter on TIBA stationery was sent to all 122 of those ABA members. It invited them to the next TIBA convention and included the following paragraphs:¹

According to ABA membership records you have identified yourself as a person who respects the discipline of behaviorology in your professional work. This letter comes to you from The In-

ternational Behaviorology Association (TIBA), the professional organization dedicated to preserving and developing that discipline.

The behaviorology movement is specifically focused on the evolution of behaviorology as an *independent* natural science discipline. The discipline of behaviorology is guided by a strict radical behaviorist philosophy of science. TIBA exists to organize this discipline and to focus its mission of applying this science to the diverse facets of the culture. Most TIBA members are members of The Association for Behavior Analysis, which consists of a large and broad coalition of persons who fall into the general “behavioral” category, but who represent a variety of philosophical perspectives and who entertain various notions about how best to organize behavioral science for its cultural mission.

If you are interested in pursuing your professional work (in any applied field) as a person whose professional activity is informed by the basic scientific discipline of behaviorology, you are invited to contact [...TIBA].

Following some details about membership, the letter ended in this way:

We hope to see you at the [TIBA] convention. We will also be happy to respond to your inquiries about becoming a member of this movement to establish the independent discipline of behaviorology.

The arrival of only a few responses seemed to confirm the speculations that most of the behavior analysts who did not want to be classed as psychologists were not ready to affect a personal affiliation with an explicit alternative. But the letter informed a large number of persons of the implications and connotations of the behaviorology label to which they had been attracted. Thus TIBA functioned in timely fashion to further secure its informal control over the name of the discipline that it had been organized to protect—and to preserve the technically precise meaning of that name against the drift that can accompany popularization.

Responding to a shortage of text materials that represented an independent behaviorology in name and concepts, behaviorologists teaching in universities began to organize their instructional materials for publication as explicit “behaviorology” texts. By mid-1989 several such efforts were under way. Ernest Vargas became the first to devote a sabbatical leave (fall term, 1990) to such work.

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

Bernice Stewart and Julie Vargas were completing a book (Stewart & J. Vargas, 1990) on child care written explicitly from a behaviorological perspective. Lawrence Fraley was organizing the materials for his behaviorology course into a textbook called *General Behaviorology* (current version: Fraley, 1996a). And others began inserting the name behaviorology into the titles of textual materials that they were authoring for local use in courses that they were teaching.

In 1991 TIBA formed an Academic Affairs Committee. It had three charges: (a) to explore, initiate, and coordinate activities relevant to establishing departments of behaviorology in institutions of higher education; (b) to initiate and coordinate efforts to establish programs of studies leading to degrees in behaviorology; and (c) to design, implement, and operate an evaluation mechanism by which behaviorologists in various kinds of institutional settings can be assessed with respect to the professional quality of their behaviorological productivity.

The early behaviorologists encouraged one another to identify openly their relation to behaviorology. Guy Bruce successfully did so in applying for, and obtaining, his first postdoctoral position as director of the Learning Skills Program at Monmouth College (IL). Discussion among TIBA members focused on ways to introduce behaviorological courses free of control by competing disciplines. The objective was to establish programs, and ultimately departments, of behaviorology as soon as practicable. Stephen Ledoux described potential training programs and coursework in behaviorology for the undergraduate and masters levels (see Ledoux, 1997c). He also included a certificate program that could be employed to provide behaviorology training to post-graduate professionals already dealing with behavior.

A few successive approximations to disciplinary homes had occurred, for example, when university-based training programs under the title "behavior analysis" had been established independent of existing psychology departments. Perhaps the best known one was at the University of Kansas. Another example began in 1989 at North Texas State University (NTSU). There, after seven years of effort, the training program headed by Sigrid Glenn, and founded on a strict radical behaviorist philosophy, received formal approval to award a masters degree in "behavior analysis." That program resided within the Center for Behavioral Studies, a university unit entirely separate from the college which housed that institution's traditional psychology department.

The "behavior analysis" label attached to such programs prolonged the ambiguity evoked by that name. In the NTSU case Glenn herself was soon to become active in the professional activities of the American Psychological Association (APA; see *Division 25 Recorder*, 24/25, p. 6). Still, this program increased the small list of universities at which an entering graduate student could go either to

a natural science program and study behavior in its functional context, or to a separate and different program where one could study the varieties of psyches still thought by many to have something important to do with behavior and to exist in obscure, perhaps neurological, recesses of the "mind."

The Relevance of Contemporary Professional Organizations

Persons who thought that behaviorists of any kind should affiliate with an *existing* professional organization, and not further divide the general behavioral movement, often criticized the early behaviorologists. That raised the question of how the behaviorologists, in light of their goals and values, viewed the existing organizational options confronting them during the late 1980s. This section reviews the more prominent of those available options.

Division 25, APA. Division 25 of APA (American Psychological Association) had been operating under the title "The Experimental Analysis of Behavior." It was composed of behavior-analytic psychologists too attached to organized professional psychology to sever that relation easily, or in many cases even to approximate doing so. That could be done by joining the more removed ABA (Association for Behavior Analysis) instead. Some members of Division 25 had *concurrently* joined ABA while others had joined the Association for the Advancement of Behavior Therapy (AABT). But many of those dual affiliators continued both to call themselves psychologists and to behave as if an effective behavioral science could and should ultimately develop through a continuous evolution of traditional psychology.

Some APA Division 25 loyalists, when operating within the more "separatist" organizations (ABA and AABT), have contributed to a quiet but persistent tension between (a) groups more committed to total separation from the organized discipline of psychology and (b) groups more ambivalent about separation. Efforts to neutralize revolutionary zeal in the separatist movements have succeeded to the functional extent that neither ABA nor AABT publicly, explicitly, and officially proclaims clear justification for its independent existence. Nor does either organization take decisive, effective, formal, publicly visible, irrevocable steps to further distance itself from organized psychology. However, by 1991, whether intentionally or not, ABA had approximated such a step on the level of guild interests by deciding to begin accrediting "behavior analysis" programs (see Hopkins, 1991; Shook, 1993). Still, most of the eligible programs existed in university psychology departments and were construed to be *psychology* programs.

Since the inception of the APA Division 25, its leaders have tended to concentrate efforts on describing the nature of the science and philosophy that would have to

prevail in a psychology discipline capable of fulfilling its putative cultural mission. At the same time, unlike the behaviorologists, those leaders have not inferred, because of the nature and strength of the encountered resistance, that stepping out of psychology is the most feasible approach to their long-range objectives.

Stepping out of an old discipline and recognizing a new one, while sometimes necessary to leave behind an untenable organizational morass, can exact significant professional costs from those who lead the departure. And because the time scale by which new disciplines emerge can span or exceed the remaining professional lifetimes of those who take that step, they may foresee little personal recovery of their former professional statuses. Like potential revolutionaries in any traditional setting, those in the APA ranks, in facing the quandary inherent in the option to jump to the behaviorology independence movement, had to gauge their respective capacities to be sustained by reinforcing effects inherent in promises of a future that they might help to construct but not live to experience. The contingencies of conservatism in issues like this tend to be stronger for older professionals who have little professional life remaining. This perhaps partially explains why the more established and respected professionals, who also tend to be older, often resist the appeal of such revolutions.

Further complicating matters, APA Division 25 leaders had moved to co-opt the name “behavior analysis” to imply that organized psychology had proprietorship over whatever professions, fields of study, or scientific disciplines are connoted by that phrase. (Such moves threaten the conceptual and organizational property rights that some attempt to claim under a “behavior analysis” label *independent* of nearby corresponding psychology properties, for example, the masters degree program at North Texas State University.) One move to appropriate that phrase involved changing the name of the *Division 25 Recorder to Behavior Analysis* for at least two full volumes between 1986 and 1989. Some Division 25 members floated a proposal that the division also change its name to Behavior Analysis. In the same vein, Division 25 had maintained a committee for “Behavior Analysis as a Separate Specialty [of psychology]” headed in 1988 by James Johnston. In reporting to the Division 25 Executive Committee, Johnston’s committee recommended that “the Division tie in with *other groups of psychologists* (ABA, AABT) who might be interested in this issue” thus openly implying that ABA and AABT were psychological organizations (“Minutes of,” 1988, p. 130; emphasis added).

Such efforts by the behavioral psychologists notwithstanding, the leaders of organized psychology as a whole appear to have been ambivalent about owning behavior analysis at that time. The president of Division 25 was “largely ignored” when he contacted the APA office about

listing behavior analysis as a separate specialty within psychology (“Minutes of,” 1990, p. 10).

The moves to incorporate behavior analysis into psychology might have exerted pressure, on those behavior analysts who were ambivalent about separation, to form closer affiliations with organized psychology. But these events also re-emphasized philosophical integrity as a variable of demarcation between (a) disciplinary coalitions like either psychology or behavior analysis and (b) the discipline of behaviorology.

By the late 1980s, the phrase *behavior analysis* had come to denote behavioral practitioners who operate individually with a variety of supporting philosophies of science and who also grouped together under the behavior analysis banner at least partly due to political and economic contingencies. On the other hand, behaviorologists shared radical behaviorism as their common philosophy of science. During the early years of their movement, described in this account, they did not compromise their philosophy of science to enhance a social, economic, or political position within organized science.

The Association for Behavior Analysis (ABA). Previously called the Midwestern Association of Behavior Analysis (MABA), ABA traces its origins to an eight year interval of informal and formal meetings begun in 1966. The history of that period, briefly reviewed here, comes mostly from a history article by Margaret Peterson (Peterson, 1978) who was involved in starting that movement.

ABA was begun, however gradually, by people frustrated in their attempts to operate scientifically under organizational arrangements controlled by others who were respecting a different science—a kind of motivation that would later share in driving the behaviorology movement. Significantly, like TIBA, ABA also began with a strong commitment to the philosophy of radical behaviorism and to the kind of science that radical behaviorism supports. Peterson, in her article, consistently used the terms behavior analysis, behavior analyst, behaviorist, and behavioral:

...in the restricted sense of the orientation most directly related to the work of B.F. Skinner and characterizing the work [then] currently published in the *Journal of the Experimental Analysis of Behavior* (JEAB) and the *Journal of Applied Behavior Analysis* (JABA). (p. 3)

The psychologists who had long dominated the Midwestern Psychological Association (MPA) had been rejecting submissions from behavioral people for presentations at MPA conventions (p. 3 [stand-alone page numbers like this in this chapter are from Peterson, 1978]). This snubbing precipitated an all-day meeting of concerned behaviorists during MPA’s 1971 annual convention. The agenda pertained to organizing a “behavioral group.” Peterson explained:

MPA continually proved to be an inefficient mechanism for behavior analysts to exchange and report research interests. These difficulties eventually led to the development of an independent behavioral movement within MPA. (p. 3)

This early effort was merely an attempt to organize for political leverage *within* MPA. The need for separation only gradually became apparent.

If, in emerging from these early beginnings, ABA had evolved into the organizational center of an independent scientific discipline that was quality-controlled by the philosophy of radical behaviorism, then the behaviorology movement probably would not have arisen two decades later. Aside from historic interest, why ABA did *not* follow such a course is of substantial *practical* importance in steering the evolution of organized behaviorology; hence the following degree of detail.

Within the MPA minority, consisting of concerned behaviorists, two strategies emerged: First the group would try to change the operation of MPA by taking over some of its leadership positions. Some positions were subsequently obtained, but any resulting changes in the way the organization operated were deemed insufficient. A survey among the dissatisfied behaviorists revealed “overwhelming” support for a second strategy that was subsequently adopted: The group would conduct its *own* behavior analysis conferences in conjunction with MPA conventions.

MABA held its first such conference in 1974. But costs and room scheduling problems forced the meeting to be held at the University of Chicago rather than at the MPA convention hotel elsewhere in the city. At the group’s second convention, held in 1975 concurrently with MPA (but at another hotel just across the street), one important development was the declaration of a new purpose. In addition to continuing to press MPA for more cooperation, the statement of purpose also declared that the new behavioral organization would “provide a meeting place for behavior oriented *non-psychologists*” (p. 5; emphasis added). This was in no way intended to threaten the political dominance of the psychologists who maintained a substantial majority in the break-away MABA.

However, problems with MPA continued. MPA insisted on “complete censorship rights to all sessions MABA wished to hold or schedule” (p. 7). As a result, at a critical meeting held during that second convention, the MABA Organizing Committee reviewed its relations with MPA. A survey had revealed that few of MABA’s people were actually attending sessions at both conventions. This suggested that the cost being incurred by MABA to make that possible for its members was not justified. Also, hotel accommodations for the joint meetings with MPA were difficult to secure. So the committee decided to pursue the task of effecting a *total* organizational separa-

tion of MABA from MPA. But that move was intensely debated, and the decision to do so cost the fledgling MABA organization some of its members, including one of its most energetic leaders, Gerald Mertins, who was then serving as MABA coordinator. Mertins left MABA to continue his efforts to influence MPA directly from within.

MABA held its 1976 convention in tandem with that of MPA rather than concurrently. MABA had stepped away from MPA, but only one pace. This step was a successive approximation that tested the water but did not yet pit MABA’s exclusive drawing power against that of MPA. Nor did that step completely alienate MPA people by operating with total independence as a direct competitor, though that would soon happen.

The fourth MABA convention, in 1977, saw increasing commitments by MABA leaders to growth, both national and international, as well as, significantly, to guild issues. A resolution emerged that MABA pursue licensing and professional certification—a step beyond the mere accreditation of training programs. That resolution arose not only because these “have proven economically and politically beneficial to professions” (p. 14) but also because “if MABA does not take an active role, such decisions will be made by persons in other disciplines and legislative bodies who may attempt to licence or govern behavior-analytic practices” (p. 14). (Just such an attempt occurred in Florida in 1991, and is discussed in Chapter 5.) Though probably not fully analyzed at the time, this commitment to guild issues formally committed MABA to growth in part as a *political* organization.

In planning for the 1978 convention, the MABA Council endorsed the concept of growth in various ways. It changed the organization’s name to the Association for Behavior Analysis (ABA). It extended ABA’s influence internationally. It established regional affiliated organizations. And it accepted an organizational focus on social issues, furthering the need for the kind of organization that could attain political effectiveness. In general, the MABA leaders acted on the concurrent and rational assumption that organizational and political power would best accrue through the recruitment of large numbers of people. That notion may have gained special strength from the financial worries that the organization had faced throughout its formative years, which made the economies of scale seem especially attractive. MABA was small and operated with volunteer help and meager funds. But it had originated in a larger organization (MPA) which had a big-budget tradition and large-scale fiscal operations. In spite of the poverty that circumstances now imposed, the early MABA leaders were accustomed to thinking about organizational money matters from a bigger organizational perspective.

The long-range organizational implications of these various moves toward guild issues, rapid growth, and

support of social causes were probably not obvious. Steps taken to build a scientific discipline, and moves taken to enhance political, social, or economic advantages, often have incompatible implications to which people are not sensitive at the time. Importantly, the MABA founders had been especially concerned about good science and had wanted to create an organization that would better support it. Nathan Azrin, an early MABA president, had expressed a hopeful visionary projection (from Peterson, 1978):

What we are witnessing with MABA may be not only a distinctive type of regional convention organization, but also the birth of a new discipline... separate from Psychology, Psychiatry, Education, and other related areas. Perhaps this perspective will give special impetus to the growth of the area, more so than has been possible when behavior analysis has been required to mold its development to conform to the format of the other disciplines. (p.15)

However, the organization drifted toward a policy of mass recruitment of members. Inevitably the need for income from dues and the need for a collective political voice conflicted with efforts to impose scientific and philosophical skill requirements on new applicants. The organization increasingly emphasized action on social and cultural fronts which could unintentionally divert attention from important scientific and philosophical issues. This was an emphasis that would appeal to potential recruits with more of a bent for social and political action than for the science that could best inform those actions. And with membership came control through unrestricted voting rights.

While affecting an *organizational* separation from psychology, the early failure to bear the cost of dissociating conceptually and scientifically from psychology permitted some followers to keep a foot in both disciplinary camps. This also kept the door open for ABA to continue recruitment of large numbers of behaviorally inclined *psychologists*. Most such persons would remain loyal to the organized discipline of psychology and would not work to support a separate discipline. They would support ABA only as a semi-autonomous piece of organized psychology and as a general behavioral cause—and political—action group.

Ever since MABA became ABA, tensions have continued in ABA between different factions: One group preferred that behavior analysis remain a disciplinary facet of psychology. Another group wanted a separate discipline called *behavior analysis* with its own philosophy, science, and behavioral technologies apart from those identified with psychology. But by the late 1980s, after long erosion of any such potential once there, the dream of a separate “behavior analysis” discipline centered in ABA seemed re-

mote. This was especially so with the large number of psychologists and others loyal to psychology residing in ABA as voting members who could not be expected to endorse their own discipline’s exclusion.

Some members on both sides of the argument had made heavy professional investments in the implications of their respective positions. Therefore they were not in a position to acquiesce, so a moot debate continues. Consider, for example, the equality of the substantial personal professional investments of the people in behavior analysis training programs, some within, and some outside of, organized psychology. ABA members at North Texas State University or at the University of Kansas, where behavior analysis training is *apart* from psychology, are in no more of a personal position to acquiesce on this issue than are ABA members from universities where behavior analysis training is embedded *within* psychology departments, such as at Auburn University, West Virginia University, or Western Michigan University.

Behavioral psychologists, dedicated to changing psychology to a more behavioral discipline, have had an option of supporting either ABA or Division 25 in APA. Political leverage might be gained by threatening the larger psychology establishment with defection by participating in ABA. This is similar to an abused spouse initiating divorce proceedings and temporarily moving out in what sometimes is actually a threatening gesture aimed at ultimately saving the relationship. When the split begins to look permanent or is taken too seriously, the party engaged in the ploy makes moves of reconciliation (see the discussion in a later section about the American Psychological Society for other examples of this strategy). However, little evidence suggested that the massive APA, with nearly 65,000 members (Hayes, 1987b, p.41), had grown concerned about the gestures of independence by the few percent (Division 25, 1990) of its members in the behavioral minority.

A reasonable conclusion seems to be that the ABA of today cannot serve as the organizational locus of an entirely independent and singular discriminable discipline—nor has that been possible since the early days of MABA. This is due to a series of early assumptions, decisions, and commitments by its leaders, as well as because of moves within organized psychology against an independent behavior analysis discipline. To a debatable degree, which some members seek always to reverse, ABA has attained an *organizational* independence from the psychology establishment. But persons committed to retaining behavior analysis as a conceptual facet of psychology continue to share heavily in the control of ABA.

The reluctance of some devoted ABA supporters to accept the view that ABA represents a disciplinary mix, instead of an integral discipline called behavior analysis, was underscored when one of the authors of this docu-

ment submitted a final draft of a paper (Fraley, 1988c) to *The Behavior Analyst*. In that paper he had described behavior analysis as “a branch of psychology represented by an APA division much influenced by behaviorological science.” The editor subsequently telephoned to say that the paper had been forwarded to the printer with that statement deleted at the editor’s discretion. One stated reason was to save space. Another was because the editor disagreed that that should be the case, and was uncomfortable about acquiescing to the psychology establishment on that point.

Another example surfaced early in February 1989 on the ABA ballot to elect a Council representative at-large. One candidate’s statement of goals for ABA included strengthening the identity of behavior analysts—credentialing them as such and accrediting their training programs. The candidate lamented the fact that the special skills of behavior analysts are “too readily claimed by the special educator, counselor, psychologist or other ‘imposters’...” At that time both the editor and the candidate mentioned in these examples were associated with training programs that had the phrase “behavior analysis” in their titles yet operated apart from psychology departments.

The founders of ABA, however, were mainly psychologists who had adopted the phrase *behavior analysis* and taken their organization outside of APA. But they had not also taken its scientific and philosophical property outside of the *concept* of psychology. Psychologists remained welcome in ABA. And so many of them joined ABA that for psychologists with territorial tendencies to claim “behavior analysis” as one of the areas either within psychology, or under the umbrella of its influence, seemed natural and appropriate (see J. Vargas, 1989, for some relevant data).

As of this writing ABA is playing out a perhaps necessary historical role by occupying a transition niche in the evolution of behavioral science. Organized in ABA, *behavioral* psychology, including behavior analysis, constitutes an evolutionary branch between the trunk of organized mainstream psychology and the emergent organized discipline of behaviorology (also, see Ledoux, 1997a). At least a few of the ABA founders apparently viewed ABA’s emergence as the organizational expression of an independent disciplinary movement. But that was neither the functional reality of its beginnings nor of its existence since that time. Even Azrin, whose hopeful appraisal of disciplinary independence was quoted earlier, was later counted among those who preferred that behavior analysts remain within psychology. He even argued against behaviorists leaving the APA for the equally psychological APS (American Psychological Society) saying, “behavioral interests are not served by actions that encourage individuals or Division 25 to weaken the involvement with APA” (Azrin, 1988, p. 140). Many ABA members hesitate, for apparently rational reasons, to let philosophical and

scientific commitment jeopardize a personal professional position mediated either by organized psychology or by an indefinite discipline called behavior analysis. ABA contains a smaller number of behaviorologists (or persons prepared to assume that identity).

ABA continues to serve what many behaviorologists see as an important purpose. If not as a disciplinary locus protective of a particular science and its special philosophy, then ABA serves as a scientific coalition. ABA provides a meeting ground for behavioral professionals of different types. At the same time, the various behavioral *disciplines*, organized elsewhere to foster their own conceptual integrity, more clearly define themselves and work to resolve any conflicts attendant to their mutual existences. In such a role ABA, as a general behaviorally oriented organization, operates more as a widely representative science forum and social/political advocacy body supporting behavioral causes, most of which are ardently supported by the behaviorologists in ABA. In the advocacy role ABA *complements* mainly scientific disciplinary organizations such as TIBA (Ledoux, 1989). All early TIBA leaders and most of its members retained their formal affiliations with ABA. In her 1989 ABA presidential address, Julie Vargas (1989), in a data-based examination of the nature of ABA, revealed a kind of organization left suited by its history for precisely the kind of mission described here.

E.A. Vargas (1988) had earlier elaborated on the respective roles of TIBA and ABA. In part, he wrote that we behaviorologists

...can spend our time talking and becoming active over matters such as credentialing, women’s rights, rights to effective education, and other social policy matters. Or...we can spend our time talking about habituation, foraging behavior, experimental work in the classroom, clinical applications of behaviorological principles, contingency analysis of cultural phenomena, five-term contingency analysis, verbal behavior, and other science matters.

Other organizations, for example, the Association for Behavior Analysis to which many of us belong, already deal with advocacy issues. Such a role well fits an organization such as ABA. ABA’s members divide themselves on basic scientific and epistemological issues, as denoted by such labels as interbehaviorists, radical behaviorists, cognitive behaviorists, and methodological behaviorists. These groups would never agree on a basic disciplinary foundation. Furthermore they represent different disciplinary back-

grounds in their interest about behavior analysis. There are psychologists and behaviorologists, social workers and sociologists, educators and clinicians—and a stray member here and there from other disciplines, for example, philosophy. Many, if not most, of these professionals did not join ABA to step away from their home discipline and set up a new one. They joined ABA to exchange views on mutual interests. They share, for example, common ethical concerns. Though a psychologist and a behaviorologist would disagree whether it is language or verbal behavior that should be studied, they would agree that all students and patients should have the right to effective treatment. These common social policy concerns can be pursued within an organization such as ABA. Our scientific disciplinary needs can be fully met only by our own organization. (p. 3)

By 1991, a scientific/technological trend, congruent with this analysis of organizational evolution, was detectable within ABA. For instance, in its 1991 publicity flyer, the Society for the Advancement of Behavior Analysis (SABA, ABA's tax-free satellite organization) was defining behavior analysis as "a science-based helping profession based on the application of basic principles of learning to the solution of behavior and performance problems" (emphasis added; see Fraley, 1992b). Calling behavior analysis a profession, which is defined by the kinds of problems its practitioners attempted to solve, skirted the issue of how to describe and organize a verbal community around the underlying science that implicitly informed such work. But more importantly, the behavior analysis leaders were defining that movement in guild terms rather than discipline terms. Their action was perhaps a tacit recognition of a kind of shift in the behavior analysis movement that carried it further away from any posture of competition, with movements like behaviorology, for the organizational locus of the underlying basic natural science.

Those events did not occur without resistance. The appearance of this guild-focused definition brought strenuous objections from the science-oriented faction within ABA, and the president of ABA subsequently disavowed it (Morris, 1991). The following year, the *Behavior Analysis Digest*, a publication edited and distributed independently of ABA by Joseph Wyatt, responded to this debate by publishing its own definition of behavior analysis as "a natural science approach to the study of behavior, and the application of science-based interventions to problems of individual, social, and cultural

importance" (Behavior Analysis..., 1992). This marked the first time that members of the organized behavior analysis movement had emphasized the adjective "natural" in a scientific definition of their activity. The behaviorologists had, from the beginning, accepted their own natural science approach as a basic assumption of their self-definition. But the behavior analysts, in nursing their relations with the heavily metaphysical mainstream psychology community, had never before so blatantly defined themselves in language that emphasized that facet of their contrast with the more traditional psychologists. In doing so, some of them were nudging themselves away from psychology and closer to behaviorology.

However, ABA's 1989 membership questionnaire (ABA, 1989) had also asked respondents how they would characterize ABA *then*, and how they would predict it *in 5 years* and *in 10 years* hence. With percentages of respondents listed in that order (i.e., 1989, +5, and +10), responding members characterized their organization as follows: as a scientific society (27.2%, 25.5%, 23.9%), as an academic organization (34.2%, 28.8%, 25.9%), as advocacy oriented (9.6%, 12.8%, 14.3%), as practitioner oriented (28.0%, 31.9%, 34.7%), and as other (1.0%, 1.0%, 1.2%). These responses predicted a slow drift toward a less scientific and less academic organization that would be more focused on clinic-related issues. Such a trend may have appeared likely in part due to immigration from the more clinically oriented Association for the Advancement of Behavior Therapy (AABT). Long-term changes in AABT (discussed in the next section) continued to make that organization increasingly less appealing to consistently behavioral practitioners.

For behaviorologists to try to change ABA into a behaviorological disciplinary organization did not seem feasible, or even appropriate. This was for the same kinds of reasons that made the attempt to recast psychology into a suitable home for their discipline seem not only inappropriate but futile. Many ABA members spoke ambiguously about their respective disciplinary orientations. Others seemed ambivalent. ABA members needed individually to analyze the contingencies governing their involvement with ABA. They would have to consider the incompatible basic analytical approaches represented respectively by psychology and behaviorology. And they would have to chart their own best courses for future disciplinary and professional progress.

Ironically, by the early 1990s, moves with strong independence implications (discussed in detail in Chapter 5) were coming from the clinically oriented ABA members rather than from the more scientifically oriented group. Behavior analytic clinicians, who objected (a) to being state licensed as "psychologists" and (b) to required supervision by psychologists as a condition of practice, were seeking to be licensed as "behavior analysts." This would require state

recognition of behavior analysis as an independent discipline (Shook, 1993). While political and economic issues had attenuated the independence moves by the more science focused members, these issues were now propelling the clinical group toward disciplinary independence.

In summary, the founders of ABA included a number of radical behaviorists who were prepared to experiment with disciplinary separation. But political and economic contingencies favoring quick numerical growth soon produced a large coalition of “behavioral” people among whom disciplinary unity was increasingly infeasible. Soon the issue of commonality in science and philosophy, beyond that generally connoted by the term *behavioral*, was overshadowed by other causes shared among the members. Appropriately, under the circumstances, ABA began to entertain advocacy issues rather than assuming the organizational role of protector of the scientific and philosophical integrity of a particular basic discipline—though passionate advocates of an independent behavior analysis discipline continued to resist these trends on scientific grounds. By the early 1990s they had gained support from clinicians seeking independent credentials for behavior analysts. Apart from the question of whether behavior analysis is a kind of psychology or an independent discipline, ABA is organized for, and heavily committed to, the pursuit of social causes and professional issues. As such it complements more than competes with the more purely science focused TIBA.

The Association for the Advancement of Behavior Therapy (AABT). Over the years most members of AABT have been practicing clinical psychologists who have found behavioral techniques useful. The integrity of the organization centers on a particular field of application, namely clinical practice. The relation of AABT to organized psychology is less revolutionary than that of ABA. Membership in AABT does not significantly jeopardize one’s status as a psychologist with respect to basic disciplinary foundations. In AABT, where many members regard themselves as either psychologists or practitioners of the medical specialty of psychiatry, the more mentalistic members impose compromises on their more behaviorist colleagues (see Giles, 1984). This weakens any countercontrols that behavioral practitioners might mount against the drift toward cognitive or mentalistic verbal behavior. Werner Matthijs, the Belgian behaviorologist, and others familiar with the European Association of Behavior Therapy (EABT), have described that organization in the same way (personal communications). Al Kearney (1988) reported in his presentation at the first TIBA convention:

The cognitive movement has gained such great ascendancy and strength in AABT that, at the World Congress of Behavior Therapy, Joseph Wolpe was prompted to suggest publicly that the name of AABT be

changed to the “Association for the Promotion of Eclectic Recipes.”

Given these realities plus its applied focus on clinical applications as opposed to the development of the basic science, the early behaviorologists construed AABT to be even less suitable than ABA to serve as the organizational home of a new discipline featuring a consistent philosophical, scientific, and technological integrity. The founders of the behaviorology movement therefore did not seriously consider attempting to organize under the auspices of AABT.

In general, besides the uncertainty about which discipline should inform a clinician’s practices, within psychology departments three kinds of contingencies detract from the quality of clinical training that behaviorologists would deem adequate (see Michael, 1980, for an extended discussion): (a) The prevailing perception of virtue in eclecticism (Ledoux, 1997a) results in surveys of the conceptual foundations of various schools of analysis, and training in the behavioral perspective is necessarily diminished when treated merely as one of those approaches. (b) The enthusiasm for applied research in such programs can lead to curricula which de-emphasize conceptual fundamentals to allow time for accelerated and extended involvement with applied practice. And (c) faculty members in clinical programs, themselves products of such training, can lack interest or skills in the conceptual foundations of a behavioral science. A professional organization peopled by such faculty members and their students, or former students, appeared unprepared to play a significant role in the emergence of an independent basic science of behaviorology. Even behavioral psychologists, echoing Wolpe’s lament, have been voicing complaints in conversation about what they see as AABT’s creeping disrespect even for the principles of *behaviorally influenced psychology*.

The American Psychological Society (APS). During the mid-1980s a substantial political rift developed within APA when the applied, clinically oriented practitioners, with their numerical superiority, assumed increasing political control of the giant APA to further what were primarily their guild interests. The rift seemed to be exacerbated by traditional biases against clinicians among more theoretical and conceptual APA members: They often deemed clinicians to be less well trained in the scientific and philosophical fundamentals, and more prone to scientific drift under both the practical exigencies of clinical practice and the strong personal financial contingencies that often pervade clinical operations. These more scholarly types, whose salaries were often provided through universities, were typically free of the kind of economic contingencies that prevail in the working environments of those who must survive on client fees. The more scholarly, academic types within APA, representing

the scientific and philosophical mix typical of psychology, became increasingly concerned about their relative loss of control over the disciplinary literature, training programs, credentialing, and even definition of the psychology "discipline" itself. In the subsequent political battles, waged over efforts to reorganize APA, these numerically inferior forces, representing more attention to scientific fundamentals, continually lost.

Steven Hayes (1987a, 1987b, 1987c), from his platform as president of the behavioral Division 25 of APA, described the mounting crisis and marshaled support to resist the guild-focused clinicians. He helped organize the APS as a separatist group initially called the Assembly for Scientific and Applied Psychology (ASAAP), and threatened to lead that group out of the APA if political parity could not be achieved within the APA. The resistance movement operated under the joint auspices of ASAAP and the Council of Graduate Departments of Psychology. That resistance movement, defending what its leaders described as "good science," gained widespread support from various units within APA, but not an overall political majority.

More importantly, because Hayes and his close followers had long been identified with the tiny behavioral minority within psychology, perhaps such a new organization, with Hayes and his close followers taking a leading role in its development, might be more behavioral than APA. Persons identifying themselves as "behavioral" constituted only a small percentage of the APA membership (Epstein, 1987a, reported that Division 25 membership had dropped below 2% of the APA total). But the ratio of behavioral members to non-behavioral members *within* the new APS movement appeared to improve only slightly.

While still formally within APA, the movement scheduled its own separate conference for June 1988. Movement leaders invited over 100 psychologists thought to share their concerns. The regularly appearing news and information article in the APA Division 25 journal, *Behavior Analysis* (1988, 1, 23), noted that "approximately 10% of the invitees are in one way or another affiliated with behavioral psychology." This implies, of course, that about 90% were *not* behavioral. Although the behavioral faction might have accomplished a slight ratio gain as a result of the APA rift, that gain did not appear to be significant.

During these developments Hayes remained active in the leadership of APA Division 25, which he apparently hoped would defect to APS (see Hayes, 1987a-c). Division 25, focusing on the "experimental analysis of behavior," had always attracted more scientific and scholarly people than guild-focused practitioners. But by 1988 the appeal of Division 25 was waning significantly within the increasingly guild-oriented APA. Although APA had tens of thousands of members, Division 25 received only seventy new applications for membership during all of 1987. A

1988 report to the Division 25 Executive Committee stated "only 15 out of 3460 applications this quarter to APA were for Div. 25" ("Minutes of," 1988, p. 125). Within APA this was a market share of less than one-half of one percent. Hayes (1988b) described the deteriorating situation this way:

Year by year Division 25 shrinks in size. We are now down several hundred members from our peak; our total membership will probably sink below 1,000 in the next few years [a prediction that proved correct; see Division 25, 1990]. We just lost one of our Council seats and at the current rate will lose our last one within several years. Our leadership is leaving. Even former Presidents of the Division are no longer Division 25 members. The Division 25 convention hospitality suite, which used to draw hundreds, now draws only handfuls. The convention program, which used to receive several dozen submissions, now receives almost none. Basic behavior analysts often literally have to be begged to present a paper in our convention program. (p. 138)

In January 1989 a circulated brochure described the new academic and scientific psychology organization. (By that time it had become independent of APA and been renamed the American Psychological Society.) The brochure listed 64 important psychologists who had joined as "founders." Very few could be identified as behaviorists of *any* variety (e.g., the 1986 ABA membership list included only two of them, Stephen Hayes and Duane Rumbaugh). To attract the large numbers of followers necessary for the kind of political struggle being waged, the behavioral psychologists once again needed to maintain a togetherness with cognitivists, "humanists," and others of most any ideological persuasion. A coalition partner had only to pass as seriously "scientific" (as psychologists define that term). APS leaders increasingly employed the descriptive phrase "scientific psychology" apparently to emphasize their differences with the implicitly less scientific guild-focused factions that they sought to leave behind in the APA.

Signs of a political struggle to capture a large membership continued to appear, some aimed at recruiting ABA members. Hayes, who had become treasurer of the new APS, had long been an ABA member and participant in ABA conventions. But he sent a letter to ABA informing its leaders that he and his usual traveling party, having grown dissatisfied with ABA, would not attend the 1989 ABA convention. That action, however intended, was widely interpreted as an appeal for others to join him in

abandoning ABA in favor of the new APS movement in psychology. But ABA membership records across the following two years revealed no sign of such a shift.

At about the same time, B.F. Skinner, along with many important psychologists, received a request from the APS psychologists. They wanted him to join in formally endorsing their new movement and the APS organization. After some delay, in March 1989, the *APS Observer* featured a section entitled "Letters of Greeting" that included the following entry signed by B.F. Skinner (1989c):

Many years ago, E.G. Boring predicted that the future of the APA would depend upon whether those members who would eventually compose a very large majority, because there would be more places for them in American life, could control their own interests and aspirations and make sure that the scientific side of the Association would continue to occupy an important place. I do not think they have done so, and a fresh start is therefore needed. The American Psychological Society seems to me to be the natural next step in the furtherance of psychology as a science. (p. 8)

Though requested as one of a large set of such endorsements, the APS literature focused special attention on Skinner's letter.

The emergence of APS was particularly interesting to the leaders of TIBA insofar as APS, like TIBA, was purportedly established to foster good science. But APS members were also being subjected to strong contingencies in classes other than scientific. For instance, in its first 24 weeks of existence, APS membership rose to 4000 regular members plus 1000 students. Its leaders were contemplating establishing a "Washington presence," and they recommended that the APS budgets for 1989 and 1990 include \$100,000 and \$250,000 respectively for that purpose. (For more details, see the *APS Observer*, 2 [2], 1989.) By the start of 1993, advertisements for new members, mass mailed to university faculties including those in behavior-related departments, saliently touted insurance programs, travel and lodging discounts, financial services, discounts on profession-related purchases, and potential access to prizes and awards. These materials also heavily emphasized a strong APS mission of government lobbying.

Clearly, aside from whatever the "scientific psychology" label was intended to imply about the development and evolution of science, APS was formally bringing its members under a wide variety of non-scientific contingencies and appealing to potential new members on those bases. Furthermore, while APS might shun practitioners with self-serving guild focused interests who did not seem appropriately respectful of scientific founda-

tions, APS still reflected an intense preoccupation with maintaining the status enjoyed as a result of the cultural entrenchment of organized psychology.

Many behaviorologists had little difficulty agreeing that the APS movement may indeed be "the natural next step," as Skinner said, *for psychology*. But that agreement was quite aside from considerations about their own science. The struggle over guild issues, which precipitated the APS revolution, remains a problem in psychology for psychologists. It is not a particular problem in behaviorology (though the APA/APS struggle helped clarify some important matters for behaviorologists). Furthermore, the lines of fracture that have been produced in organized psychology by that issue are not lines of fracture that threaten the behaviorology movement. That is by design; the behaviorology movement was conceived and constructed to minimize guild-related contingencies. The scientific psychologists' whole revolutionary struggle was directed against those kinds of disruptive economic contingencies and their scientific and political implications within psychology.

Whether or not the APS movement ultimately succeeds, psychology, emerging either split or whole, will continue as the disciplinary home, or homes, of thousands of persons who defend and practice a science quite unlike behaviorology in nature and capacity. And the psychologists can be expected to multiply themselves prodigiously. Behavioral psychologists who support APS might, with the help of their majority non-behavioral allies, eventually impose some effective countercontrols on colleagues deemed conceptually shallow or economically distracted. But the APS alliance portends no relief for its behavioral faction with respect to the types of philosophy and science predominating in the discipline of psychology. ABA convention programs show that even Steven Hayes soon resumed his annual participation in the ABA conventions (e.g., in 1992 the program listed him as a participant in five convention events).

Except as a somewhat parallel organizational experiment to be observed and evaluated, the APS movement appears to be irrelevant to the behaviorology movement. One residual question of interest pertains to the behavioral psychologists who sought to further the interests of good science by bolting to APS *instead* of taking the opportunity to move away from psychology and join the behaviorology movement. Did they make the right move? Hayes's own growing dissatisfaction with APS as an organizational sanctuary for his science was revealed in a question that he raised about the future of APA Division 25. In his scathing denunciation of APA (Hayes, 1991), in which he explained his recent departure from that organization, he asked:

...where will a Division 25 live? It is not a scientist group, nor a practitioner

group, nor a scientist–practitioner group. It is a *disciplinary* group. *Yet we have no disciplinary organization left within psychology.* (p. 21, emphases added)

Earlier in that article he had allowed that “APS could still come through if its upcoming reorganization is brilliantly done. ...I still have hopes” (p. 21). But apparently he did not have many, in view of his unequivocal declaration that his discipline had *no niche within organized psychology* (which at that time clearly included APS). All things considered, Hayes seemed to be living through the kind of implications that several years before had led the behaviorologists to make their own kind of decisive organizational move.

A New Professional Organization

An organization originally called The Behaviorology Society came into existence on 24 May 1987 when nine radical behaviorists met on the eve of the thirteenth ABA convention in Nashville, Tennessee. Gathered for the meeting (which was tape-recorded and major portions subsequently transcribed—see Ledoux, 1987b) were Lawrence Fraley (West Virginia University), Sigrid Glenn (NTSU), Douglas Greer (Columbia University), Joe Layng (Enabling Technologies, Inc., Chicago, IL), Stephen Ledoux (State University of New York at Canton), Jack Michael (Western Michigan University), Mark Sundberg (Sundberg & Associates, Concord, CA), Ernest Vargas (West Virginia University), and Julie Vargas (West Virginia University). The meeting was organized and chaired by E.A. Vargas.

Members of the group were already in general agreement on the need to recognize formally the distinctive nature and independence of this scientific discipline. The agenda included (a) potential names for the organization, (b) the current situation of the basic science of behavior, and (c) the implications of the culture-wide monopoly, with respect to behavior-related matters, enjoyed by the cognitive/mentalistic coalition of researchers, practitioners, sympathetic administrators, and politicians.

The participants also considered the relation of any new organization to ABA. This was a sensitive issue because some persons in attendance had extensive professional investments in ABA, and a few continued to assume that ABA might yet become the organizational locus of an integral and independent scientific discipline. Those conservative in that regard prevailed in rejecting possible names for the new organization that contained the term “international” or “association.” The founders did intend that the organization evolve with an international character. But some objected to the grand connotation of that term, deeming it pretentious since the incipient organization had fewer than ten members, all Americans. Also, ABA was stressing *its* international operations (though until

the behaviorologists had subsequently done so with respect to their organization, ABA did not begin appending phrases to that effect to its name). Using “international” or “association” might have hinted at organizational competition, which some present would not tolerate, and all were anxious to avoid.

After adopting a name, *The Behaviorology Society*, those present constituted themselves as the Society’s Executive Board. Julie Vargas was unanimously elected to serve as the first chairperson. The Executive Board then asked her to organize the drafting of several statements: purpose and goals, cultural mission, relationship to other organizations, membership arrangements, and other statements deemed important. She was to circulate a draft of those constitutional documents among the Society members for review and critique. The members adopted plans for future meetings. By the time the meeting adjourned, the discipline of behaviorology had been given formal recognition under the auspices of an organization of its advocates and practitioners. So passed the 24th of May, 1987.

Seven months later, on 28 December 1987, an all-day meeting was held in Waltham, Massachusetts. Invitations to participate were extended to the five Executive Board members within a day’s driving distance of the Boston area. Julie Vargas chaired the meeting, which Lawrence Fraley, Stephen Ledoux, and Ernest Vargas also attended. This subset of the original group functioned as an Executive Board subcommittee. It had been empowered by the larger group at the founding meeting to take action on early organizational matters. Circumstances had gradually convinced the members of the subcommittee that the name should accurately describe the nature of the planned organization. Also the admission to membership of some persons from other countries was imminent. So the subcommittee decided to change the name of the organization to *The International Behaviorology Association* contingent upon subsequent approval by the members. The subcommittee would also ask the members to approve other actions taken, including scheduling the first convention of TIBA for August 1988.

Until more members became available to help with the organizational work and the tasks could be further subdivided, some of the early members accepted assignments to rather broad responsibilities: Ernest Vargas was to concentrate on membership matters; Stephen Ledoux would organize the first convention, and also focus on international activities; Lawrence Fraley would plan for publication activities; Julie Vargas would investigate the problem of how to develop teaching and training opportunities for the new discipline. She would also consider how behaviorological instructional materials might be created, and would explore the possibility of a handbook for the discipline.

More importantly, TIBA leaders were sensitive to the implications of the various organizational structures, operations, and mission-related assumptions discussed earlier in this chapter. Clearly, no existing organization was suited to providing an organizational home of the kind needed for this movement. Therefore, those attending devoted much of the meeting to designing a structure for the new organization that would keep members' behavior focused on the basic mission objectives. For example, any organization of persons with specialized repertoires can lose its way if an overzealous devotion to ballot box democracy is combined with a relaxation of standards for the admission of new members. Scientific organizations fall prey to that trend when the recruitment of large memberships, thought necessary for gaining status and influence through political and economic power, is accomplished at the expense of qualitative standards for the discipline-related repertoires of the recruits. The result can be control invested in a scientifically and philosophically divergent electorate. TIBA would chart the course of its growth to avoid that unacceptable trade. As E.A. Vargas (1988) wrote:

The strength of TIBA will lie not in size, but in the commitment of its members: how extensively they dedicate themselves to the science of behaviorology, the effort to improve that science, and to the means by which that science can be improved.... Such work concerns not only the products of science, but the infrastructure to facilitate the production of those products. TIBA constitutes part of that infrastructure, as would a journal, a department of behaviorology, and other resources that become tools to move the science forward. Our scientific work depends on its supportive base. (p. 4)

The TIBA Board members attending the Waltham meeting saw their task as designing an organization that could optimize and maintain the quality of the science—especially its capacity for cultural applications. So they adopted an organizational structure (which the full TIBA Executive Board accepted prior to the first convention) featuring four membership categories. Three were non-voting categories: (a) Student, (b) Affiliate (conceived at that time as a category for interested persons, typically from other disciplines or fields, and often with minimal if any behaviorological skills themselves, who, for a variety of reasons, want to maintain close contact with the organization, receive its publications, etc.), and (c) Associate (for persons with an extensive behaviorological repertoire and a good record of professional accomplishments). The fourth category, Fellow (later changed to “Full”), was to

be acquired by Associates through invitation extended by the Fellows.

Matters of importance would be resolved through voting by the Fellows. In this way the direction of the organization would remain in the hands of those members of the discipline who, over a long period of time, would have demonstrated substantial effective behavior under relevant philosophical, scientific, and political contingencies. Nevertheless, *this restriction of voting rights was a calculated risk*. The reservation of all voting privileges for a subset of people was understood to invite power seizing coups. The organization would have relatively few Fellows in its early stages, so the capacity to exert healthy counter-controls over an individual moving aggressively to establish personal control over the organization would therefore be limited. But at this time, all parties to these considerations endorsed the importance of maintaining appropriate balances of views and functional counter-controls among the Fellows.

The Associate membership category was designed to be the main category for accomplished behaviorologists, while Fellows were to exhibit a substantial extra increment of work on behalf of both the discipline and the TIBA organization. Fellows also supported TIBA by paying significantly higher dues from the outset (which they voted to double for just themselves in 1992). In December 1988, after the first convention, a new Executive Board further decided that all new, non-student TIBA members would enter at the Affiliate level. After a year of disciplinary contribution and organizational service, qualified behaviorologists could advance from affiliate status to the Associate level and beyond.

The subcommittee at the Waltham meeting assigned priority to scientific and philosophical integrity centered in the discipline of behaviorology. It rejected development strategies dependent on numerical strength *acquired by lowering the scientific standards for membership*. In a world pervaded by cognitive and mentalistic assumptions, the behaviorological vanguard could better tolerate thin ranks than tenuous resolve. The adopted organizational design separated the concept of membership size from the concept of organizational control. *The organization could accommodate any number of members*, but a personal share in controlling the organization was supposed to accrue only in proportion to one's recognized professional contributions. (See “A Small Selection of Photographs,” in the book containing Ledoux, 1997e, for a photo of the Waltham meeting participants.)

First TIBA convention—1988. TIBA held its first convention, TIBA-I, on 9–11 August 1988 at Clarkson University in Potsdam, New York. John Nixon, Director of Clarkson's Institute for the Study of Applied Behavior Analysis (ISABA), had extended an offer for ISABA to serve as the host. Local arrangements were coordinated by

Stephen Ledoux, who was employed at the State University of New York campus in nearby Canton. Ernest Vargas developed the program of presentations and sessions.

The convention served as a forum for detailed discussions about the scientific and organizational foundations of behaviorology and directions for its development. The purposes were (a) to consolidate the integrity of the discipline by bringing the members more strongly under control of its properties, (b) to refine the definitions of the discipline including its scope and mission, (c) to plan for the continued evolution of the discipline, and (d) to prepare TIBA, the scientific and professional organization of the discipline, to support these goals through its own development and activities.

The Executive Board subcommittee responsible for planning the convention preferred a small single-track convention of persons prepared to work for the discipline and for TIBA. They thought the intimacy possible among a smaller number of committed people would support a higher quality of scientific exchange and encourage fuller and more candid contributions. The capacity of the preferred meeting room imposed a limit of thirty participants (booked by advanced registration only).

News of the upcoming convention was reaching people too late in many cases to secure travel support or to avoid conflicts with previously scheduled professional events. Many could only express interest and asked to be kept informed. A couple mentioned having been advised (by colleagues who thought the new movement pointlessly divisive) *not* to attend. McIlvaine Parsons wrote a thoughtful letter to say that he sympathized in general with moves to strengthen "behaviorist approaches" but he could not lend his support to movements which fractionalize "operant psychology."

The convention program (TIBA, 1988), which was tape-recorded, featured papers on components of the discipline, its history, and its future development. The first morning session, entitled *Philosophical and Experimental Foundations of Behaviorology*, opened with Ernest Vargas's talk titled "Difficulties and Opportunities." Carl Cheney followed with "Impacting Behavior with Science." And Jack Michael concluded with "What Is and What Is Not Our Subject Matter." The afternoon session, entitled *Philosophical and Technological Foundations of Behaviorology*, featured R. Douglas Greer, then Laura Dorow (presenting for herself and Nan McCorkle who was unable to attend), a contingent from the Los Horcones community, and Al Kearney. They respectively related behaviorology to applied settings in general, to schools, to experimental communities, and to clinical practice. (Joe Layng, also scheduled to speak, did not attend the convention nor participate further in the behaviorology movement.) The final session, entitled *Past, Present, and Future of Behaviorology*, occurred the morning of the sec-

ond day. It began with a presentation called "Behaviorology: Where We Have Been and Where We Are Now" (an early version of the present paper) by Stephen Ledoux and Lawrence Fraley. A combined open forum, business meeting, and planning session followed that last paper. The discipline itself absorbed most of the discussion. Topics included TIBA journals and other publications, academic programs and departments, administrative and organizational features of the discipline, and future meetings. (At future conventions, scientific papers on topics of general interest would occupy a much larger part of the program while separately scheduled formal business meetings would deal with most organizational topics.)

Twenty people attended the convention. That number enabled the group to sit around a large circle of tables and engage in spirited discussion and debate. Most took economical lodging in an adjacent Clarkson University dormitory made available to participants. The participants represented a mix of disciplinary backgrounds and current fields. But they shared an affinity for the discipline of behaviorology, or at least for their respective concepts of it which, not surprisingly, proved to differ somewhat. In addition to the previously mentioned presenters of papers, also attending were Scott Beach, Guy Bruce, John Eshleman, Sigrid Glenn, Jeffrey Kupfer, Robert Spangler, John Stone, Jerome Ulman, and Julie Vargas. Also present was Juan Robinson who spoke for the Los Horcones Community, in Mexico, which holds a group membership in TIBA and sent a three-person delegation. (See "A Small Selection of Photographs," in the book containing Ledoux, 1997e, for a photo of most of the TIBA-I participants.)

During the extended and serious discussions about the behaviorology movement a number of issues surfaced. While not all were fully resolved, at least a tentative organizational position emerged with respect to most of them. None of the issues proved so divisive as to derail the movement. Debated questions included these: What is the relation between behaviorology and behavior analysis, both as sciences and as organized movements? Is holding concurrent memberships in TIBA and ABA inconsistent? Can TIBA and ABA be complementary, and if so, would either need to change? If TIBA, especially in its early years, de-emphasizes numerical strength (and thus political clout) in order to consolidate its scientific and philosophical integrity and establish an enduring foundation, then how, other than politically, might it exert its effect and influence? Is the endorsement and participation of important and well known people critical to the recognition of the behaviorology movement? Through what mechanisms can behaviorologists develop a distinct identity and a unique set of definitive characteristics for behaviorology? And finally, what ethics should govern in cases of behaviorologists who deemed it necessary to de-emphasize

or hide their scientific orientation when operating within their respective positions of professional employment?

Some convention participants most committed to a separate and distinct discipline of behaviorology shared a common intuitive notion that potentially divisive issues, especially those pertaining to relations with other disciplines and organizations, would sooner or later be resolved in their favor by events taking place *within those other settings*. If they were correct, organized behaviorology could afford to let nature take its course, with respect to some of those matters, rather than having to purchase gains through *immediate* and perhaps pressured conversions to a common wisdom.

Among the participants at that first convention, perhaps the most fundamental difference, with the most far reaching implications, was centered on the essential nature of the fledgling movement. Was it limited to the development, protection, and dissemination of effective natural science as a personal behavior repertoire in individuals? Or was it also primarily an *organizational* effort to produce a new and independent scientific verbal *community*, that is, to forge a new organizational product to serve as the organizational home of this science within the culture? This issue was about whether the behaviorology movement would evolve into a new scientific cultural agency or whether it would function more as a kind of retreat for the scientific refurbishing of persons who would do their work within other scientific verbal communities to which the benefits would then accrue. One variant was this: Were the energies and resources of this movement to be spent as direct and indirect contributions to the improvement of other organized disciplines, or would this movement stand alone ultimately to assume the cultural mission of organizing an independent discipline around this science, an endeavor in which others had lagged or faltered? Though in retrospect, the participants obviously brought these differences to that first convention in 1988, this issue was too subtle and complex to emerge explicitly, and it was not articulated on that occasion where it emerged, if at all, only as biases in the address of other issues. Debate on this potentially divisive matter would open later, mainly through the literature (e.g., see Fraley, 1988d & 1991).

In any case, the first convention was lively. Debates were vigorously pursued. Many offers to work in specific ways for the discipline or for the TIBA organization were tendered and accepted. As the convention concluded, all participants, to differing degrees, declared or implied that their further personal involvement would be worthwhile.

Prior to the first convention, the group chairperson, Julie Vargas, had contacted members of the founding group through a mailing of information about the subcommittee's preferences for organizational structure, dues, and transitional procedures. Also included was a

ballot listing all nine members of the original group, from which they were to elect a president, a secretary, and a treasurer as the first formal officers of TIBA. Use of the ballots indicated acceptance of the organizational procedures. Members returned the secret ballots to a secretary at West Virginia University who had agreed to receive them and provide a report on the election outcome. Julie Vargas announced the results toward the end of the convention: Ernest Vargas had been elected Secretary, Lawrence Fraley, Treasurer, and Jack Michael, President (each for a term that would end in January 1992).

Michael had recently assumed the office of acting psychology department chairperson at Western Michigan University. During the convention discussions about the behaviorology movement he had taken the position that he was a psychologist who wanted to continue the effort to change psychology. He wanted to regard himself as a "behaviorological psychologist." Some found that acceptable because they regarded psychology as a *field* of study (although Michael's own view of what psychology should become did not comport to that model). Others deemed that position inconsistent, and the label "behaviorological psychologist" self-contradictory, because they construed behaviorology and psychology to be separate basic disciplines featuring incompatible sciences and philosophies that preclude such dual allegiance. Yet another view held that, while psychology and behaviorology were indeed fundamentally different and largely antithetical, psychology represented a scientifically and philosophically ill-informed *approximation* of behaviorology. In some of these various comparisons both psychology and behaviorology provided analytical repertoires applicable to behavior-related phenomena, but they remained incompatible paradigms. Some people were still weighing the issues. Others reasoned that organizing a separate behaviorology was the *alternative* to further serious efforts to convert the tens of thousands of mentalistic psychologists to a new and improved science (see Fraley, 1988d, for details). In any event Michael expressed his appreciation for the honor of the presidency, cited lack of time to pursue the duties of TIBA president, and declined the office.

Those in attendance from among the original nine founders immediately met and on a new ballot chose Stephen Ledoux as the first formally elected president to serve TIBA under its accepted organizational practices. His first duty was to assume the convention moderator's position being relinquished by Julie Vargas. The three new officers (Ledoux, Vargas, and Fraley) became the first Fellows of the organization. They also functioned as the first formally elected Executive Board thereby initiating the newly approved organizational structure and operating procedures.

Before adjourning, the members endorsed the international character and thrust of the behaviorology move-

ment. They also indicated substantial support for holding some future meetings and conventions outside the United States. The delegation from the Los Horcones Community in Mexico volunteered to study the feasibility of their hosting the second TIBA convention. Their offer was enthusiastically accepted. (The second TIBA convention, TIBA-2, was subsequently held there in January 1990.)

When Clarkson University settled its account, TIBA discovered the first convention to have unexpectedly produced a profit of about \$1000. TIBA began to invest this in its fledgling publications and other professional activities. For about \$150 per person, the participants had paid the registration fee, received lodging, and even a couple of meals. Clarkson University had invested and recovered about \$2000 on the convention. Something about TIBA's special approach to launching the behaviorology discipline was implicit in these figures.

The TIBA Statement of Purpose. Before the convention closed, those present also extended general approval to a July 1988 draft of the TIBA statement of purpose, which Stephen Ledoux had authored and taken through many revisions. That version received further polish during the next year. At an Executive Board meeting on 26 May 1989, the following refined version was approved and incorporated as part of the TIBA By-laws ("TIBA By-laws," 1989):

TIBA is a professional organization dedicated to representing and developing the philosophical, analytical, experimental, and technological components of the discipline of behaviorology, the comprehensive natural science of the functional relations of behavior including determinants from the environment, both socio-cultural and non-cultural, as well as determinants from the biological history of the species. Therefore, recognizing that behaviorology's principles are generally relevant to all cultures and species, the purposes of TIBA are:

- A. to foster the philosophy of science known as radical behaviorism;
- B. to nurture experimental and applied research analyzing the effects of physical, biological, behavioral, and cultural variables on the behavior of organisms, with selection by consequences being an important causal mode relating these variables at the different levels of organization in the life sciences;
- C. to extend technological application of behaviorological research results to areas of human concern;

- D. to interpret, consistent with scientific foundations, complex behavioral relations;
- E. to support methodologies relevant to the scientific analysis, interpretation, and change of both behavior and its relations with other events;
- F. to sustain scientific study in diverse specialized areas of behaviorological phenomena;
- G. to integrate the concepts, data, and technologies of the discipline's various sub-fields;
- H. to develop a verbal community of behaviorologists;
- I. to assist programs and departments of behaviorology to teach the philosophical foundations, scientific analyses and methodologies, and technological extensions of the discipline;
- J. to promote a scientific "Behavior Literacy" graduation requirement of appropriate content and depth at all levels of educational institutions from kindergarten through university;
- K. to encourage the full use of behaviorology as the essential scientific foundation for behavior related work within all fields of human affairs;
- L. to cooperate on mutually important concerns with other humanistic and scientific disciplines and technological fields where their members pursue interests overlapping those of behaviorologists; and
- M. to communicate to the general public the importance of the behaviorological perspective for the development, well-being, and survival of humankind.

The first meetings of the new Executive Board. On 20 November 1988 the TIBA Executive Board, consisting of the first three TIBA Fellows (Lawrence Fraley, Stephen Ledoux, and Ernest Vargas), convened in Albany, New York at their own expense for a three-day meeting. Through approximately twenty hours of business meetings, they addressed a long agenda, including development strategies and tactics for the organization, organizational structure, and operating style. They worked to develop the discipline of behaviorology, and its professional organization, TIBA, in accordance with behaviorological principles. Like so many other aspects of the culture though, when subjected to behaviorological

redesign, familiar organizational aspects had to be cast in new and unfamiliar ways by the designers.

The Executive Board devoted much time to the careful analysis of the behavior controlling implications of each proposed step, both immediate and long term. Three crucial issues emerged. First, how might TIBA further adjust its organizational structure and operating procedures to prevent the behavior of its people from drifting away from the organizational objectives? For example, the board devoted much time to finding ways of doing things that would avoid the leaders coming under economic contingencies that could jeopardize scientific standards. Second, how could TIBA balance the need for organizational stability against the need to prevent individuals or groups from retaining control and exploiting the organization to mediate access to their personal reinforcers? Third, how should TIBA manage growth? Since TIBA had to invent the organizational structure, and the arrangements for operating within it, as the organization developed, the proper pace of growth was critical. TIBA would have to develop its human resources individually and employ these resources productively. If the new movement merely became the object of the latest wave of popularity, then the transient “sincerity peddlers,” who attach themselves to the latest scientific fad, could quickly inundate it. In the long run such people can have adverse effects on any movement that attracts their “loyalty.”

The Executive Board recognized as its tasks (a) to develop the new disciplinary organization around the only current science with which to address effectively, at its level of analysis, the behavior-related problems of the global culture, (b) to protect the integrity of that verbal community, and (c) to provide for the constructive evolution of that science while countering the powerful and omnipresent contingencies to compromise it. The first TIBA Fellows thus attempted to create a *behaviorological* organizational product by design. They acted upon the premise that the newly organized discipline of behaviorology would ultimately eclipse psychology in providing effective behavioral technologies to address problems throughout the culture. This was already true in cases where stringent accountability was being enforced. Persons who *had to produce* effective behavior-related outcomes quickly operated in a more behaviorological fashion. But, for the time being, the general paucity of accountability in behavior-related operations throughout the culture allowed a variety of alternatives, usually pseudo-scientific and maintained under non-scientific contingencies, to prevail. The behaviorology movement would have to be initiated, constituted, maintained, and operated ultimately to assume the cultural niche reserved for an effective science of behavior. Many of the independent variables critical to that ultimate disciplinary dominance were controllable by the early TIBA Fellows as

design variables—a circumstance felt as a substantial responsibility. Nevertheless, how best to arrange those variables with respect to any particular issue was seldom immediately clear.

The three participants practiced and refined the skills of reasoned argument necessary for consensus building, a process which hopefully would define the operating mode of TIBA. They reached many specific decisions, and a multitude of planning details fell into place: TIBA would develop its own general journal, named *Behaviorology*. TIBA would also pursue the proposal that it publish *The Analysis of Verbal Behavior*, an established journal, with Mark Sundberg continuing as editor (however, Sundberg subsequently dropped membership in TIBA and sought to transfer control of his journal elsewhere while continuing his career-long efforts to change psychology). The organization would ratify By-laws for TIBA. And the organization would become incorporated, seeking the non-profit tax status proper for a scientific organization.

Subsequent meetings produced further actions. The Executive Board appointed Jerome Ulman as International Activities Coordinator (replacing Stephen Ledoux who had served in that capacity prior to his election as President of TIBA). The Board also appointed Carl Cheney to the post of Publications Coordinator. As the three elected officers (Fraley, Ledoux, and E.A. Vargas) were initially the only TIBA Fellows, they also met not as an Executive board but *as* TIBA Fellows and extended invitations to Cheney and Ulman to become Fellows as well.

All five Fellows—and-officers were in Milwaukee for the 1989 ABA convention. They met again there, both as TIBA Fellows and as executive officers, for a total of about six hours. They approved the TIBA By-laws that Ledoux had drafted and other Executive Board members had edited. They made further plans for the second TIBA convention. And they reviewed progress on other fronts. Psychology-behaviorology relations, and the continuing question of what publicly to say about those relations, absorbed much of the discussion.

TIBA Publication Operations

To appeal to natural scientists of behavior, the behaviorology movement would have to offer a wide range of publication opportunities. The early behaviorologists were generally committed to developing a *complete* disciplinary model, including a full spectrum of professional publications.

TIBA Publications Board. In the fall of 1991, Cheney announced a TIBA Publications Board which he would chair (as Publications Coordinator). Also serving were Robert Crow (newsletter editor), John Eshleman, Lawrence Fraley (journal editor), R. Douglas Greer, Glenn Latham (journal managing editor), Nancy Marchand-Martella and Ronald Martella (*Behaviorological*

Commentaries co-editors), Jerome Ulman, Ernest Vargas, and Julie Vargas. Cheney also began to function as TIBA's coordinator of textbook production.

The TIBA Newsletter. The immediate need for communications to link the members of the new behaviorology movement had led to the early establishment of a newsletter. Volume 1, No. 1 appeared in the fall of 1988, edited by Carl Cheney. Called simply "*TIBA Newsletter*," the first-page heading, composed by Lawrence Fraley, included the TIBA stationery logo previously designed by Stephen Ledoux and Scott Beach (featuring the name "Behaviorology" in five languages). After three issues in Volume 1, Volume 2 appeared in a more attractive layout under the editorship of Scott Beach who produced issues No. 1 and No. 2 during the spring and summer of 1990. As a major financial contribution to TIBA, Beach produced and distributed those two issues at his own expense. Unfortunately, he then contracted a serious illness during a foreign trip and could not continue. With Beach unable to produce issue No. 3 in Volume 2, Lawrence Fraley produced that issue while a new editor was sought. Robert Crow agreed to assume the editorship, and the newsletter operation was transferred to Crow, at the Louisiana State Medical Center in New Orleans, beginning with Volume 3. Following the Winter 1991 issue (Vol. 4, No. 1), TIBA changed the name from *TIBA Newsletter* to *Selections*. Late in 1992 Kathleen Orlando, then a TIBA student member at West Virginia University, took over the editorship.

A journal for TIBA. Shortly after forming in 1987, TIBA organized an investigation into all aspects of the feasibility of a TIBA journal. This investigation continued throughout 1988 (the year the TIBA newsletter began publication).

Many considerations and recommendations arose from that investigation. While journal contents would have to make worthwhile scientific contributions, a journal would also play an important role in defining, anchoring, and establishing the discipline. The contemplated reviewing arrangements were rigorous and included substantial assistance to authors in shaping high quality articles. By the selective nature of the subscription list (including all TIBA members) TIBA could assure authors that a critical audience of behaviorological colleagues would receive their published works.

An additional proposal was that each author receive a copy-ready master of his or her article in the form published in the journal (along with a suitable cover sheet saliently identifying the journal of origin). Using these materials, authors could acquire high quality reprints prepared through their own local and hopefully inexpensive copying services. Authors would be encouraged to distribute these among their respective colleagues and to other parties potentially interested in their work. In this

way, authors could construct further professional networks linked to behaviorology.

In May 1989 the Executive Board reviewed and reiterated elements of journal policy: The journal would feature a balance of article types and would maintain quality through careful reviewing procedures. By the fall of 1989 about a dozen authors had made commitments to submit specific works.

A variety of issues arose and had to be addressed: Would behavioral psychologists, behavior analysts, or various other behaviorists, all committed to other than the behaviorological way of advancing the science, want to publish in this journal? Would they find themselves comfortable contributing to this journal and thereby lending an implicit measure of personal support to the particular long-term strategy represented by the behaviorology movement?

Another potential problem concerned the founders of TIBA who, as active professionals, had long established production patterns. As a matter of practicality, how difficult would those patterns be to change? Would other established behavior-related journals compete for their work? And, would rational organizational engineering outpace emotional predisposition? For instance, might a behaviorologist discover that his or her professional lifetime of efforts to promote better science and philosophy within the broadly defined domain of organized psychology not be as readily transferred to the support of an independent discipline as that person might have assumed?

An additional issue had implications for project management: In spite of other potential difficulties, the importance of the journal *was* understood. So, not unexpectedly, people therefore were cautious in taking concrete steps in support of it, taking time to satisfy themselves on the many points of concern about qualitative matters.

A more strategic issue revolved around the tension between member recruitment and journal establishment. To advertise the new movement and the science that it represented would necessitate publishing behaviorological articles in other more established, mass-circulation journals and in mass-marketed books. But that would make those works unavailable to a TIBA journal. Yet, the longer TIBA went without a journal, the more tenuous and uncertain the development of the movement appeared, especially in the view of outsiders who might be contemplating joining it. Publication outlets can be very important, especially to the kind of scientifically productive person often regarded as valuable to recruit. The TIBA leaders had to consider whether to disseminate the available behaviorological articles by publishing them in the books and journals of other disciplines, which would inform a wider audience about the behaviorological science and its organized movement, or whether, at least tem-

porarily, to commit those products to enhance the movement's publication-related infrastructure.

Finally, as TIBA proceeded to define and refine its operations, it dedicated 25 percent of its dues income to fund what was regarded as another important and innovative aspect—a formalized program of research support. Could TIBA fund concurrently a journal and a substantial research support program? At its May 1992 meeting the TIBA Executive Board determined that both programs could and should be pursued concurrently. The journal effort would go forward.

All of these were worthy issues. In the end they did not prevent the establishment of the journal or significantly delay it. They are reported here to reveal how important journals do not spontaneously and abruptly arise out of the complex scientific, social, political, and economic communities that such journals represent; the actual origins of such journals are more complex and far more interesting.

In January 1992 Lawrence Fraley, who had headed the journal feasibility study and had been serving as Editor *Pro Tem*, was named Editor for a two year term. During 1991-1992 he expanded the editorial team to include associate editors: R. Douglas Greer for experimental studies, Nancy Marchand-Martella for applied studies, and John Eshleman for conceptual studies. Shortly thereafter Gloria Paige was also added as the Book Editor. The Managing Editor's function was consolidated under Glenn Latham and with his supervision the production team prepared to do the final styling of manuscripts and produce camera-ready copy of journal issues for the printer. By the summer of 1992 a set of manuscripts that had passed successfully through the reviewing process had been forwarded to Latham for that kind of final preparation.

The debates about journal issues and attendant activities brought into focus the need to further bureaucratize the organization. The founders of TIBA had created it as an engineered product, and had done so in a very short time relative to the long, slow, and more natural evolution of similar organizations. For them, designing and operationalizing TIBA embodied an exercise in behavioral organizational engineering. Furthermore, those who had set TIBA into motion were monitoring it. And as engineers are wont to do, they continued to tinker with it, sometimes through direct interventions—especially E.A. Vargas whose training included a sociologically based specialization in complex organizations. Yet room for such continuing interventions was not an aspect of the normal, designed operating mode of TIBA. Perhaps the time had come for the designers to relax some of the direct control that they had retained informally more or less in the form of what might be called “inventor's privilege.” But much was at stake. Could the organization operate more independently in accordance with its intrinsic

nature? Would the clock run smoothly and keep good time if the clock makers ceased their tinkering and closed the case? Adjustments would continue to be required, but whether or not they could more often be made to occur through the normal self-correcting mechanisms built into the operating arrangements of the organization remained an unresolved question.

The International Behaviorologist. During the summer of 1991, with the *TIBA Newsletter* in its third volume and the journal still under development, Carl Cheney, as Publications Coordinator, took the lead in establishing what was initially called *Behaviorological Commentaries*. This publication featured letters, essays, and point-of-view pieces, and was edited by Ronald Martella and Nancy Marchand-Martella, although Nancy would soon transfer to the journal leaving Ronald as the sole editor. By its magazine-like nature and its featured subject matter, this publication fit between the newsletter and the journal, giving TIBA a three-faceted publication front. This journal maintained short-process peer reviewing. It also left copyrights with the authors who could then republish their material elsewhere without special permission. In 1993, coinciding with Ronald Martella's announced intention to relinquish the editorship, the Executive Board changed the name of this publication to *The International Behaviorologist* (TIB). However, the future of the publication remained ambiguous; manuscripts accumulated but as of this writing no issue of TIB has appeared. This extended delay has resulted from uncertain financing, a failure to achieve consensus on editorial policy, and latent concerns about the kinds of articles that should be featured.

Occasional publications. During 1992, members of the TIBA Publications Board were discussing a further expansion of publication operations to include the occasional publication of special works unsuited by excess length or other characteristics for inclusion in newsletter, magazine, or journal. These might include special purpose booklets or full sized books of various kinds.

Little Tests for TIBA— The History of Some Policy

In the early stages of an organization, a multitude of new issues arise. Many of these are inconsequential, some are important, and a few are loaded with potentially calamitous implications for an organization. Unfortunately, the relative significance of these various matters is *not* indicated in the often ordinary, mundane, and unheralded circumstances in which they arise. Yet the early establishment of a simple precedent or policy in resolving any such issue can unknowingly set an organization on a possibly disastrous course from which it cannot later easily deviate. This possibility poses a managerial challenge. As previously noted, for example, early decisions within ABA

eventually loomed as a major obstacle to the emergence of ABA as the locus of an independent discipline in spite of its origins and organizational separation from psychology.

In the spring of 1989 two seemingly small issues surfaced that possibly represented such tests of disciplinary character for TIBA. These examples are described here, capturing in the record those passing moments of confusion and uncertainty plus the actions taken to resolve them. This is done to suggest potential adverse implications which might thereby have been avoided in the future of TIBA.

Use of the TIBA label. The first issue had implications for the relation of the organization to its members. A two-page journal article (Fraley, 1988c) featuring a behaviorological theme appeared with TIBA listed as the author's affiliation. The newly formed TIBA was publicized, but articles labeled this way could be interpreted incorrectly as official TIBA statements. After some discussion the TIBA Executive Board addressed the issue in language later included in the TIBA By-laws (1989/1990):

The name of the organization may not be used by any member in a way that implies endorsement by the organization unless such an official endorsement has been extended by TIBA and the member is acting in accordance with the provisions of that endorsement. (1990, p. 16)

In this way TIBA reserved, as a group resource, the property rights to an increasingly strong conditioned reinforcer. TIBA withdrew those rights as resources for personal control by individual members. (For the underlying principles, see Skinner, 1953; compare Chapters 20 & 21.)

Use of the Behaviorology label. The second issue, concerning use of the behaviorology label, had implications for the autonomy of the discipline. TIBA members wanted behaviorological training opportunities to develop. The question of how to start new courses and programs had been a prevailing concern of TIBA leaders. Historical circumstances had left many people who were sympathetic to the behaviorological movement trapped in academic departments of psychology. There, any course that they taught would be labelled formally as a *psychology* course, and any behaviorally oriented programmatic track would be interpreted as a facet of psychology.

The behaviorology movement had taken the significant step of organizing itself outside of psychology. At the moment of constitution, the TIBA founders had created a critical discontinuity between the behaviorology movement and a debilitating legacy from which they thought it could not otherwise subsequently extract itself. But although behaviorology had been recognized, defined, and organized as a discipline independent of psychology, the differences were not yet as well defined in these early times as they would later become. Also, existing

differences were not yet clearly perceived among members of the cultural community at large. The new movement seemed to have materialized just beyond the outer rim of psychology. Created free, it was now challenged to propel itself safely away in spite of some strong contingencies that could draw it back into the psychology vortex.

At the spring 1989 meeting of the Northern California Association for Behavior Analysis, some TIBA leaders had led a discussion session about the behaviorology movement. While there, a psychology faculty member described a new psychology course to be taught during the following fall term. These TIBA leaders returned from California with a request that TIBA condone that person's using the term "behaviorology" in the title of that course. On the one hand TIBA leaders wanted to see behaviorology taught under its own disciplinary title as part of the effort to maintain and further develop disciplinary independence. On the other hand, they wanted courses with behaviorological content to be developed. And they were also anxious to reinforce the enthusiastic behaviors of newly interested people who were offering to take concrete steps. A round of discussions arose among the TIBA leaders as they attempted to identify, measure, and pursue the conflicting implications. Significance in this episode resided less in the immediate aspects of the issue than in how members were reading and reacting to the somewhat remote and obscure implications attached to what at the time seemed a rather minor and passing event.

In the strict or conservative view, any course, regardless of title or content, taught within a psychology department and identified with a psychology prefix or designator, would formally define that topic and subject matter *to be* an element or facet of psychology. This had already happened repeatedly with "behavior analysis" despite continuing though unsuccessful efforts by many influential behavior analysts to deny organized psychology's claim to behavior analysis. Eventually, behaviorology courses would safely arise in departments and programs bearing explicit *behaviorology* titles, but that could only occur after such departments came into existence.

Explicitly identified behaviorology courses could also safely operate in any existing department if the integrity of that department were based upon any *applied* field. This is because no specific basic behavioral disciplinary affiliation is assumed for the faculty who work in such departments. Each applied behavior-related field, from advertising to zoo keeping, is open to whichever behavioral discipline can become established and accepted as the foundation behavioral science supportive of the field's operations. Courses with "behaviorology" in their title were appearing in such areas both internationally and domestically. The applied field of education is a typical example. For instance, in 1991 the Graduate Studies Department of the Xi'an Foreign Languages University

in Xi'an, China (PRC) offered a course titled "Behaviorology and Education." Concurrently, the English Department of that university offered an undergraduate version of the same course titled "Behaviorology and Teaching" (Ledoux, 1997d). Even if other competing disciplines such as psychology are already being taught in such an applied field, the discipline of behaviorology can be introduced, and it can contest for the role of supportive philosophical and scientific foundation.

A few academic departments, however, are of a different nature. Though they pertain to something about which to think (behavior, in these examples), they are not organized primarily around *specific classes* of its occurrence (i.e., they are not organized the way, for example, that a criminal justice department is organized around the study of the occurrence of *illegal* behavior). Instead they offer programs that might be classified as more purely foundational or basic. Each features a disciplinary integrity established more around its particular epistemology than around specific kinds of environmental occasions to which it might be relevant. Examples include departments of behaviorology, psychology, and theology—each of which focuses on many of the same behavior-related events as do the others. While sharing a focus on a similar subject matter in general (behavior), their substantial differences pertain to the respective ways in which their followers think about behavior. In their manifestation as foundation disciplines, apart from applied studies that often accompany them, such departments prepare students to think in a particular way about behavior in *any* applied setting. (Each such setting might in turn give rise to its own academic department of applied studies, for example, social work, or a department of labor relations in a business school). To the extent that recognized elements of the basic discipline of behaviorology are allowed to appear under the rubrics of *foundational* disciplinary competitors, behaviorology surrenders its epistemological identity as a unique discipline—that is, as a unique way to think about subject matters to which it might be relevant.

In the contrasting lenient or liberal view—because many of the best potential behaviorologists worked in psychology departments and were the people upon whom the movement might have to rely for many of its developmental products—tolerating psychology labels, and perhaps also even explicit claims to behaviorology by psychologists, may be necessary in order to get early behaviorological products. The movement needed those products and might not otherwise obtain them. Perhaps no realistic alternative was available. Maybe the pressure to develop behaviorology-labelled courses and tracks within explicitly labeled psychology units would simply overwhelm any resistance that TIBA could muster. So why

crystallize the issue by fighting to an inevitable and publicly visible loss?

Another argument held that displaying the behaviorology name and developing behaviorological products under the formal designation and auspices of organized psychology, at least to a limited extent, would not matter enough to make a fuss about it, especially if any such courses or materials incorporated the behaviorology/psychology disciplinary difference as an explicit theme. Perhaps moving out of psychology later might not be as difficult as some seemed to think. Also, behaviorologists could successfully assert that behaviorology, though some of its parts were temporarily trapped in psychology units, was represented there only by prisoners of circumstance who were not really psychologists but something different.

The discussions meandered back and forth between counter-posed questions (presented here rhetorically): Are behaviorologists abandoning the concept of disciplinary independence? Why not, as a necessary short-term tactic, take an occasional step away from what remains the ultimate goal? Could not the new movement lose far more than it stands to gain from the emergence of behaviorology-titled courses within departments recognized as the disciplinary keepers of competing epistemologies? Will the new movement run aground on the shoals of stubborn allegiance to impractical ideological notions? Will it be destroyed by the careless neglect of its own principles?

Within a short time TIBA reached a resolution. The minor moment faded into the history of the movement. TIBA would *not* condone appearances of the name "behaviorology" in contexts that could imply that behaviorology is a part of any other discipline. TIBA would condone behaviorology-titled courses in the training departments of applied *fields*, a context in which epistemological competition is appropriate. Behaviorological materials would be developed and tested in such settings, but caution would prevail. Where ambiguities might arise behaviorologists expected explicit, unambiguous clarifications on the issue of disciplinary separateness. In psychology-titled departments behaviorologists could develop and test explicitly behaviorological teaching materials provided that these clearly and adequately informed students that behaviorology was an independent discipline and not part of psychology. But no course or curricular track in a psychology department would bear a formal behaviorology title. (See Ulman, 1990b, and Fraley, 1990b, for related examples.)

If this was a little test, had TIBA passed? Many such issues were arising, each with its hidden implications of unknown potential. This section presented just a couple of incidents to suggest that critical organizational circumstances often stem from origins that might have gone unrecognized and unheralded in the stream of mundane affairs.

Within a few months another psychologist, this time on the East Coast, was calling to say that he was designing his introductory psychology course for the fall 1989 term. He wanted reactions from TIBA leaders to some materials about behaviorology that he planned to include in that course. The materials did *not* make explicit that behaviorology and psychology were entirely different disciplines, nor was it clear that the caller fully understood that. The textual materials also implied incorrectly that the essence of the name behaviorology was in its greater connotation of clinical respect for certain ethical principles popular with the public than was true of the name psychology. TIBA leaders were pleased to have had some relevant policy already in place. After discussion of the matter, the caller agreed, in response to feedback from the behaviorologists, to mention the emergence of behaviorology as a separate discipline but to teach as “behavioral psychology” or “behavior analysis” much of what otherwise would have been erroneously ascribed to behaviorology.

During the discussions about what TIBA policy should be, Fraley, whose applied-field teaching job was in an educational *psychology* department, recalled for TIBA leaders a relevant incident involving one of his courses. He had developed it as an explicit introduction to behaviorology, though the official course title was “Introductory Behavior Analysis.” A couple of years earlier, while away on summer vacation, his cognitively specialized department chairperson had reassigned that course. It was to be taught that summer by another faculty member who for years had collaborated with the department’s cognitive majority faction. That person had long acted as a strong opponent of the behaviorology movement within the department. He had publicly denounced his own somewhat behavior-analytic, clinically oriented training as having been much too narrow. During that summer term, he taught Fraley’s course after having advertised it on campus using flyers similar to those Fraley had been using. But in class he substituted methodological, clinically oriented textual materials and taught a strict psychology course with a behavior-analytic slant.

TIBA leaders were quick to recognize that the same thing could easily happen in the case of *any* formal behaviorology-titled courses established in departments committed to alternative epistemologies and disciplines. And the usurpation could be permanent.

Refinements to TIBA Status and Infrastructure

A disciplinary and professional data base. In the fall of 1989, Ernest Vargas, as Secretary of TIBA, began to organize a data base for the discipline of behaviorology. Vargas’s design included far more than the customary membership directory for a professional organization. For example, records of the publications of all TIBA-member behaviorologists would be electronically config-

ured for retrieval according to different search variables. TIBA would thus maintain in a highly usable way the complete body of discipline-related literature that its members produced. Vargas enlisted John Eshleman and Scott Beach to work on the technical development of the necessary computer software. They made plans to get the system operating while the TIBA membership was still small. Within two years Eshleman demonstrated to TIBA an elaborate custom-designed electronic format to accommodate such a data base. In the spring of 1992 TIBA Secretary Julie Vargas collected relevant data from each TIBA member and began the process of inserting this data into the new system.

Formal establishment. As Treasurer of the organization, Lawrence Fraley drafted a one-page “Document of Constitution” for TIBA, dated 26 May 1989, and secured its ratification by signatures of the five TIBA Fellows. On 7 August 1989 TIBA received a certificate of incorporation from the State of West Virginia, the treasurer’s home state. And on 3 November 1989 Fraley submitted a formal application to the Internal Revenue Service (IRS) for TIBA’s exemption from Federal income tax as a non-profit scientific organization. The IRS subsequently approved this application.

Cash-flow management. TIBA’s cash flow was based mainly on member dues. TIBA Treasurer Fraley designed and put into effect a membership renewal scheme wherein the membership year of each TIBA member was determined by the month on which that member first joined the organization. This renewal arrangement had the advantages of (a) distributing the annual work of the treasurer more evenly, (b) keeping the organization’s cash flow steady across the calendar year, and (c) allowing the earlier detection of membership trends and changes. In 1992 Jerome Ulman, who followed Fraley as treasurer, transferred the financial operations of TIBA to an electronic database. This brought the behavior of the TIBA leaders under much stronger stimulus control of various money-related variables.

Scientific Contingencies Committee. At the 1991 ABA Convention in Atlanta, Ernest Vargas, the Acting President of TIBA while Stephen Ledoux was teaching in China, organized the first TIBA Scientific Contingencies Committee. It brought together a group of young professionals charged with developing recommendations to the Executive Board about how TIBA might arrange contingencies to increase and maintain the scientific productivity of its members. Chaired by Marta Metcalfe, the committee included Guy Bruce, Mark Clingan, John Eshleman, and Doreen Vieitez (Clingan did not renew his TIBA membership and briefly faded from TIBA involvement until he rejoined in 1993 after securing a position at Vanderbilt University in Nashville). This important committee was formed with young professionals on the as-

sumption that TIBA would evolve and prosper in the long run though the lifetime commitments of its younger members who should be heavily involved in fashioning the contingencies that would bind the organization.

Conventions. About a year after TIBA was founded, it began holding two-to-three day long, single track annual conventions. In August 1988, Clarkson University was the site of the first convention, described previously.

Comunidad Los Horcones in Hermosillo, Sonora, Mexico, was the site of TIBA-2 on 4-8 January 1990. Community members coordinated the site. And Ernest Vargas organized the program and chaired the sessions. The convention, with a theme of "cultural design and development," drew about 20 attendants. The first souvenir TIBA-logo badges appeared at this convention thanks to Scott Beach who had designed and produced them. They reappeared at later conventions.

Ball State University in Muncie, Indiana, was the site of TIBA-3 on 4-6 January 1991. Jerome Ulman coordinated the site, organized the program, and chaired the sessions. The convention drew about 17 attendants. The B.F. Skinner Memorial Lecture series began at this convention with historian Daniel Bjork presenting "Toward a biography of B.F. Skinner: Rationale and interpretation" (see Bjork, 1993). The first souvenir TIBA-logo mugs appeared at this convention and reappeared in new designs at later conventions.

The Clarion Hotel in New Orleans, Louisiana, was the site of TIBA-4 on 3-5 January 1992. Robert Crow coordinated the site, while Carl Cheney organized the program, and Grace Baron chaired the sessions. The convention drew about 32 attendants. Fred Keller prepared "Education by torchlight" as the B.F. Skinner Memorial Lecture (see Keller, 1993). But he developed a case of the flu, and his paper was delivered by Julie Vargas. The Executive Board settled into the practice of meeting on the days before and after the convention. (See "A Small Selection of Photographs," in the book containing Ledoux, 1997e, for a photo of the authors of this paper at TIBA-4.)

The Stone House Club Inn in Little Compton, Rhode Island, was the site of TIBA-5 on 18-20 March 1993. Grace Baron coordinated the site, while Anne Kupfer organized the program and chaired the sessions. The convention drew about 25 attendants. Joseph Cautela presented "General Level of Reinforcement" as the B.F. Skinner Memorial Lecture (see Cautela, 1994). The rest of the program, which included poster presentations (now a regular program feature), was also heavily research-focused, more so than any previous convention program. By this convention, TIBA was matching contributions from individuals to support undergraduate student attendance. Also by this convention TIBA had decided to continue to schedule its conventions in the month of March, around B.F. Skinner's birthday if possible.

Conventions continue as annual events. (One of the appendices in Ledoux, 1997e, provides basic details on the 1994, '95, '96, and '97 conventions.)

Research support. In a somewhat bold and innovative move during TIBA-4 in New Orleans, the TIBA Executive Board dedicated 25% of the organization's income from dues to the support of scientific research projects. TIBA members would be able to apply for funds to support their behaviorological research. Ernest Vargas developed this concept and presented it to the Board for endorsement. The Board delayed refinement of the proposal and the procedural details for a future meeting. A year later at the 1993 TIBA convention, the Research Funding Committee chaired by Anne Kupfer began to make research grants in response to proposals submitted by TIBA members.

Fine-tuning of membership criteria. The mid-year Executive Board meeting of 23 May 1992, held in San Francisco to coincide with travel to the 1992 ABA convention, saw an important formal adjustment in the membership area: Acting on a proposal from the Membership Committee, the Executive Board emptied the category of TIBA Fellow. All existing TIBA Fellows moved into a new category called "Full Member." Affected by this change were the three initial Fellows (Fraley, Ledoux, and E. Vargas) plus Cheney, Ulman, and J. Vargas. A new requirement was also added: Full Member status would require not only work for the TIBA organization and higher dues (requirements for the former Fellows) but also producing and promulgating a minimum of one scientific data-based product every three years. This stressed the importance of Full Members being directly involved in scientific activity, and attempted to make it so.

The vacated Fellow category was retained, but redefined as a more honorary status. During the discussion of how the now empty Fellow membership category would be defined, a long-standing difference of opinion about the nature of the behaviorology movement surfaced indirectly: Some suggested that TIBA Fellowship might be bestowed as an honor on persons who had conducted good science but who had not joined TIBA nor supported the behaviorology movement. This was consistent with the basic notion that TIBA existed merely to support the science, and to do so without regard to where, organizationally, the scientific activity occurred—and without regard to the views of the recipient about how best to organize scientific disciplines.

Opposed to that position were those who viewed the behaviorology movement not only as a developer of the science but also as an organizational solution to the problem of how best to insure the constructive evolution and vitality of the science within the context of the culture. Those who construed TIBA to have a cultural as well as a scientific mission (see Fraley, 1991) wanted the category to

contain only Fellows who had ascended up the TIBA membership ladder and who endorsed the establishment of an independent behaviorology discipline. That, they argued, would be in keeping with the TIBA commitment to shape repertoires rather than give prizes.

The Executive Board did not settle this issue at that meeting, although majority sentiment appeared to favor ascension to Fellow via the established TIBA membership track. The Board did agree that Ernest Vargas, as Membership Committee Chair, would receive suggestions about criteria for the Fellow category and develop relevant proposals for future consideration by the Executive Board.

Leadership. A small number of people, contributing a variety of necessary and useful skills, founded the behaviorology movement. That small band advanced in a coordinated way on several fronts important to the success of the movement. Assessing the respective contributions of those people would be difficult. Nevertheless, it soon became obvious to those who worked on the early stages of the movement that one person was filling a particularly important role. The accidents of history had brought to the behaviorology movement a specialist in the organization of human resources—a coordinator who accomplishes exceptional feats mediated through the behavior of others. That is, the movement had a functional leader. Good ones require little in the way of formal authority, although that is helpful. True, people in the behaviorology movement came predisposed to emit a lot of behavior on behalf of an independent discipline. But to appreciate how so much of it came to be emitted in such a timely and coordinated fashion one must take into account the special repertoire in managing complex organizations brought to this movement by Ernest A. Vargas. (In 1991 he was elected to become TIBA's president for a three-year term beginning January 1992.)

Summary of Chapter Four

After a sufficiently definitive concept of behaviorology had come to control *descriptive* verbal behavior, a small band of self-declared behaviorologists took concrete steps to organize a new and independent verbal community around that discipline. Manifestation as a scientific professional organization came in the form of TIBA, but its relation to other behavior-related organizations raised many questions that could only be answered on the basis of careful analyses of the relations of TIBA to each of them. In the years 1987-1992 TIBA established a governance structure, largely through agreement on by-laws and the subsequent non-profit incorporation of TIBA. The organization established policy through which it exercised control over its intellectual property. And TIBA designed and partially developed a three-level publication program, successfully conducted the first several TIBA conventions, and—importantly—largely resolved ambi-

guities about the integrity of the discipline and its relations to other disciplines and fields. Personal commitments were then possible on the basis of more explicitly and starkly contrasted principles—and people could, and did, sort themselves accordingly. TIBA established an operating style that cast the appeal of the movement in such a way that only those prepared to support it, and to accept the implications of doing so, tended to add themselves to its ranks. The TIBA leaders concentrated on behaviorological designs for the organized discipline that would strengthen the focus on science, and prevent drift under political, economic, or social contingencies. Slowly, *and by design*, TIBA tried to bureaucratized itself as its designers relinquished direct control over its operations. Systematically, the behaviorologists were installing behaviorology in the community of natural sciences.

The next chapter, Chapter Five (“The Continuing Debate: Reactions from the Behavioral Community at Large”) will review the prevailing cultural milieu and analyze the support for, and opposition to, the growing behaviorology movement. While the behaviorologists were moving steadily ahead to refine the concept of their discipline and organize the scientific verbal community that would exercise intellectual proprietorship over it, the general “behavioral” community continued an increasingly moot debate on the concepts of a behaviorology movement and discipline. ✦

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Quoted

While the seven-chapter paper by Lawrence Fraley and Stephen Ledoux (of which chapter four appears in this issue) began with quotes from some early behavior scientists (see Fraley & Ledoux, 1997), other relevant quotations appeared at various points following the paper in the book containing the original publication (Ledoux, 2002) including these:

...Skinner and his followers never had a chance of making over psychology by demonstrating that practices informed by their natural science were more effective... Should accumulating evidence force a traditional psychologist to the brink of either abandoning mysticism or discounting valid and reliable evidence, the typical traditional psychologist treats the dilemma as a Hobson's choice—there is no real option. Any science that contradicts the fundamental mystical assumptions is abandoned. People who got into science in the first place in order to shed some scholarly light on the details of their deepest philosophical assumptions (including, especially, those of a religious nature) are not going to abandon those foundations if that science starts causing trouble. Instead, they abandon the science, which at that point is merely an intellectual tool that initially looked helpful, but has proven to cause more difficulties than it is worth. (Lawrence E. Fraley, from a talk at the 1996 ABA Convention [Fraley, 1996]; a part of a longer quote on pp. 128–129 [Ch. 5] of the original publication [Fraley & Ledoux, 1997].)

...I have tried too long to follow Watson in saying that psychology is the science of behavior. I am now convinced that is wrong. Psychology has always been concerned with internal explanations. To show how futile that is, let us imagine that it has been successful. Let us suppose that all those who examine mental processes introspectively now agree on what they see. Let us suppose that what they see confirms a set of theories upon which all cognitive psychologists now agree. And let us suppose that brain science, looking inside the behaving organism in a different way, has found what convincingly can be called the same thing. Shall they then have discovered the causes of human behavior or simply more about what is behaving? (B.F. Skinner, elaborating on p. 3 of the first issue of the TIBA journal *Behaviorology* on the disciplinary independence of the science he founded; see Skinner, 1993.)

...We have been accused of building our own ghetto.... Rather than break out of the ghetto, I think we should strengthen its walls. No field of science has ever been more clearly defined than this world of ours. In no other world are there more fascinating things to be explored. No world has a greater potential for solving the problems that face the world today, above all saving the planet Earth. (B.F. Skinner, elaborating on p. 5 of the first issue of the TIBA journal *Behaviorology* on the disciplinary independence of the science he founded; see Skinner, 1993.)

...Cultural survival appeared to be at stake during the emergence of modern biological science and on other occasions in human history. And so again today. However, the technologies capable of destruction that characterize the present era (whether actively, as with nuclear weapons, or passively, as with unchecked population or pollution) are qualitatively greater than those of previous times. This puts not just cultural survival but the survival of life in general on this planet at risk (e.g., from a nuclear winter). The early behaviorologists believed... that that was what was at stake, and so they incurred the costs of organizing the behaviorology movement and discipline. (Stephen Ledoux & Lawrence Fraley, from Appendix 2 [Ch. 7 section] of Ledoux, 2002, p. 313)

...Future readers, should their lives have unfolded within the context of a culture pervaded by behaviorology, might have difficulty appreciating a past era of antithesis to behaviorological science. That people would not have readily invested in a repertoire that effective—one that obvious and well demonstrated in its validity and implications, one that elegant in its parsimonious reduction of false complexities—could tax the comprehension of those who live in such a future.... (Lawrence Fraley & Stephen Ledoux, from Chapter 7 of Fraley & Ledoux, 1997, p. 158.)

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The interrelations among these quotations compels careful consideration.—Ed.

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Syllabus Directory

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

Up-To-Date Syllabi in Current or Past Issues

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

Syllabi Planned for Future Issues

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

- Volume ?, Number ? (Spring/Fall 20??): BEHG 455:
Advanced Instructional Practices in Educational Behaviorology.
- Volume ?, Number ? (Spring/Fall 20??): BEHG 445:
Advanced Experimental Behaviorology.

Syllabi Locations Listed by Course Number

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

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TIBIA Memberships & Benefits

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

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

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

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

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

\$20 Behaviorology Student membership (requires paper membership application co-signed by advisor or department

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

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

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

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

Other Benefits

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

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

ever join TIBIA, the US\$20 already paid will be applied to the initial membership dues owed. (Faculty teaching behaviorology courses can encourage their students to request this evaluation.)

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

TIBIA Membership Criteria & Costs

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

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

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

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

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

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

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

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

<u>Category</u>	<u>Dues (in US dollars)*</u>
Board of Directors member	The lesser of 0.6% of annual income, or \$120.00
Faculty member	The lesser of 0.5% of annual income, or \$100.00
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*Minimums: \$20 director or faculty; \$10 others

TIBIA MEMBERSHIP APPLICATION FORM

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

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TIBIA Treasurer
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I verify that the above person is enrolled as a student at: _____

Name & Signature of Advisor or Dept. Chair: _____

TIBI / TIBIA Purposes*

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

- A. to foster the philosophy of science known as radical behaviorism;
- B. to nurture experimental and applied research analyzing the effects of physical, biological, behavioral, and cultural variables on the behavior of organisms, with selection by consequences being an important causal mode relating these variables at the different levels of organization in the life sciences;
- C. to extend technological application of behaviorological research results to areas of human concern;
- D. to interpret, consistent with scientific foundations, complex behavioral relations;

- E. to support methodologies relevant to the scientific analysis, interpretation, and change of both behavior and its relations with other events;
- F. to sustain scientific study in diverse specialized areas of behaviorological phenomena;
- G. to integrate the concepts, data, and technologies of the discipline's various sub-fields;
- H. to develop a verbal community of behaviorologists;
- I. to assist programs and departments of behaviorology to teach the philosophical foundations, scientific analyses and methodologies, and technological extensions of the discipline;
- J. to promote a scientific "Behavior Literacy" graduation requirement of appropriate content and depth at all levels of educational institutions from kindergarten through university;
- K. to encourage the full use of behaviorology as the essential scientific foundation for behavior related work within all fields of human affairs;
- L. to cooperate on mutually important concerns with other humanistic and scientific disciplines and technological fields where their members pursue interests overlapping those of behaviorologists; and
- M. to communicate to the general public the importance of the behaviorological perspective for the development, well-being, and survival of humankind.☺

Periodical Information

Behaviorology Today [known as *TIBI News Time* for the first 4 volumes / 8 issues], is the magazine of *The International Behaviorology Institute* (a non-profit educational corporation) and is published in the spring and fall each year.

Behaviorology Today and TIBI can be contacted through the Editor at these addresses and web site:

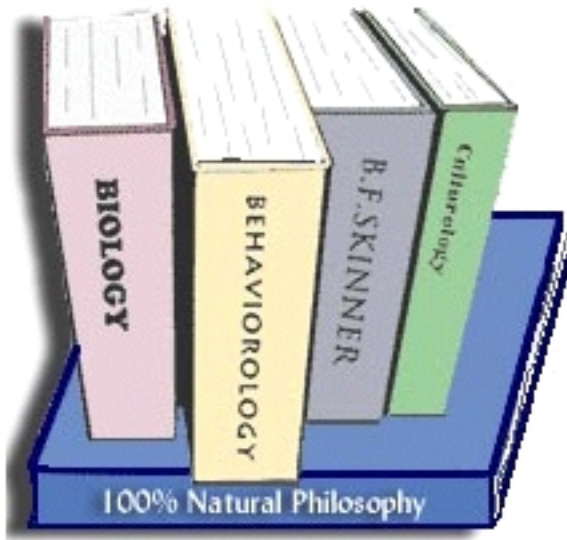
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To submit items for publication, contact the editor. Send items initially to the editor both by email (or disk) and by hard copy.

Authors' views need not coincide with official positions of TIBI. (Authors retain copyrights.)

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

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



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