

Appendices

Stephen F. Ledoux

Appendix 1: Excerpt on “*Adventitious Control*”

In helping clients solve problems under actual practical contingencies, non-behaviorological practitioners, including psychologists, sometimes *do* attain control. But that success is adventitious if it occurs merely as a result of the practical contingencies. Those practitioners then make invalid claims of credit for that success on behalf of their preferred theories, which are usually of a cognitive or mentalistic nature. But little if any of that control becomes possible *because* of the supplementary verbal antecedents of their theories. Instead it seems to occur in spite of those theories. Mentalistic analysis hinders control of behavior by misdirecting attention to specious constructs. Physiological analysis hinders control by fixating on a locus where control of behavior is usually not technically feasible. As a result mentalistic and cognitive practitioners can attain behavioral control only by coming under natural contingencies to emit successful technological behaviors that are beyond the reach of their conceptual analyses.

Natural contingencies are those in which verbal supplementary stimuli play no significant role. Under such contingencies those non-behaviorological practitioners, like other professionals, might respond in ways that prove effective. They then lay disciplinary claim to that naturally evoked technique for attaining some control, all in spite of its non-relation to their approach. For example, a cognitive practitioner might be faced with a student's earning poor math grades. He or she may then decide, from his or her particular variety of theory, that this is due, say, to a “minimal brain dysfunction” (MBD), or to what laymen might call a “mental block” within the student. (Why “minimal” brain dysfunction? Because the practitioner cannot find any real, physical dysfunction of the student's brain. But the behavior *is* a problem so the brain surely must be dysfunctional; if the dysfunction cannot be found, then it simply must be “minimal.” The problem is that the presumption of a brain dysfunction is both invalid and

The first appendix originated in some of the author's interactions with students over the years. These interactions were the regular attempts to disabuse students new to behavior science of their culturally ingrained, automatic acceptance of inner causes (i.e., explanatory fictions) as being somehow adequate as scientific explanations of behavior. The first and second appendices contain modified excerpts from the historically unique 1992 version of the first paper on the origins of behaviorology (Fraley & Ledoux, 1997). Such excerpts, historically valuable as indicators of the verbal milieu at the time of behaviorology's emergence as an *organized* independent discipline, were not included—partly because they added an excess level of detail—in the separately published 1993 version of that paper which formed the basis of the 1997 version. The other appendices provide information supplemental either to one of the papers in *Origins and Components of Behaviorology* (Ledoux, 1997a) or to the general behaviorology movement.

unwarranted in the first place. An initial assumption warranted by the situation, and worth exploring and fixing—as a behaviorologist would do—is that something about the person's environment is “dysfunctional.”

The cognitive practitioner, however, has no intervention technologies, congruent with the natural laws governing behavior, that stem from cognitive/mentalistic analysis while also being appropriate for directly dealing with either MBDS or mental blocks (or perhaps even with ineffective math-focused behaviors). This holds regardless of whether these involve real physiological events or are entirely fictitious.

Real physiological events are sometimes directly investigated (appropriately, if by physiologists). But this still has little bearing on the emergence of a behavior-change technology as opposed to a more medical technology such as some form of drug therapy. More often these putative physiological events are only supposed, hypothesized, invented, or theorized—a well-criticized pattern of non-explanation known as gratuitous physiologizing (e.g., see Skinner, 1953, 1974).

As for fictitious events (explanatory fictions), the chief objection to them is *not* that they do not exist (although that is often a problem as well) but that they are irrelevant to the prediction and control of the behavior they are said to explain. They do not have manipulable, independent variable status. They are not a member of any class of terminal variables. They cannot be changed to bring about a change in the behavior they supposedly cause. One can claim that changing something else, such as an environmental variable, changes the fictitious variable which then changes behavior in the sense of A causing B, and B causing C. But then the relation is similar to relations in mathematics: A causes C, and the middle term is at least unnecessary.

The middle term also has other problems. The major (and often only) source of knowledge or information about these “inner causes” (Skinner, 1953) turns out to be the very behavior they are supposed to explain. These “causes” are *inferred* from that behavior, and then they are used to account for that same behavior. Such a sequence is circular. A more detailed example clarifies the inferential circularity:

“Why does the child do poorly in math?”

“Because he has a mental block [or MBD, or...].”

“How can you tell [or, How do you know] that he has a mental block?”

“Because he does poorly in math.”

“But, why does he do poorly in math?”

“Because he has a mental block .”... [and so on...]

The cause is being circularly inferred from the very behavior to be explained (Chiesa, 1994, Ch. 7; Michael, 1989, Ch. 6; Skinner, 1953, Chs. 1–3). From a natural science perspective, laboratory methodologies are wasted to investigate what is not really there, while legions of students are trained to replace the professionals expended on the crusade. (An enlightening exposition appears in Chapter 1 of *Beyond Freedom and Dignity* [Skinner, 1971]. Also, some authors prefer more precise descriptors, as appropriate, such as nominal fallacy, teleology, or reification; see Gould, 1981, and Poling, Schlinger, Starin, & Blakely, 1990.)

Another problem for fictitious variables, perhaps the worst problem, is that they leave the analyzers comfortable; the analyzers seem to have found the sought-after cause. But in reality they have added nothing new to the analysis. Yet the search for the cause stops because one seems to have been found. The result is that accessible causal variables that might be changed to improve (control) the behavior in question remain

unanalyzed. These are seldom even looked for when a pat answer is available unless separate, practical contingencies for doing so are in force. The search essentially stops because the mentalistic or cognitive analysis provides no compelling reasons to continue. The *analytical* approach ends without attaining the capacity for control.

The dangers of inner causes may be less important when the behavior being explained presents no problems, such as excelling in math being explained by “intelligence” (inherited or not). But those dangers can be crippling in the opposite case.

Yet a type of control may still be achieved. For example, the job specification of the above-mentioned practitioner (with the student client who is doing poorly in math) may supply the practical contingencies that require effective behavior-controlling technologies. The specification may require him or her to help that student. If he or she finds that student parked in front of a television set for five hours each day to the exclusion of study on school assignments, he or she may change the student’s environment by pulling the plug on the set. If the ultimate result of that action is that grades improve, then that *functional* control results from the environmental change. That functional control does not result from the practitioner’s cognitively focused, verbal analytical repertoire, but in spite of it. And that fact is not altered by that practitioner trying to tie the beneficial outcome to his or her paradigm by insisting that eliminating access to television must have diminished mental blocks or MBDS.

Mentalists sometimes conveniently disavow control as their goal since their approach will not support analyses that back-track through functionally related variables to the environment. (Radical behaviorists define environment as that milieu, part of which can be inside the skin, in which independent variables can be manipulated in behavior-controlling relations.) Mentalists’ analysis precludes control so they argue against it. Instead of stressing control, mentalists emphasize understanding and dwell on predicting behavior. Their practical work is often confined to predicting behavior from other behavior—as when behavior on a specific occasion is predicted from a putative trait inferred from behavior on previous occasions. For example, educational psychology courses in the measurement of behavior typically ignore direct measurements of the properties of behavior (as delineated, for example, by Johnston and Pennypacker [1980, Ch. 7]) and instead feature textbooks devoted to identifying and measuring the intensity of what are assumed to be behavior causing traits.

On the other hand, a complete behaviorological analysis of behavior cannot satisfactorily stop without pursuing the functional sequence further back. Behaviorologists pursue the sequence of antecedent events to a point at which either (a) intervention becomes possible or (b) a behavior is interpretable in the sense of being described in terms of relations of the same kind which on other occasions have permitted control under more technologically feasible circumstances.

The quest to gain control of behavior is sometimes regarded as an allegiance to superficiality. However, failure to include control as at least a planned final step condemns a discipline to scientific immaturity (see Skinner, 1953, Ch. 2). Those who strive for control in their subject matter develop effective behavioral technologies based on the manipulation of real independent variables. They become behavioral engineers discovering largely untapped markets for those skills in all facets of the culture. Those who would not strive for control in their subject matter can only *interpret* life in various ways. But the point *is to change it*, a point made by Marx and echoed by Gould (1987, p. 154) and most important scientists.

An additional thread running throughout these components of adventitious control is the generally unparsimonious nature of the mentioned, non-behaviorological accounts. Here, parsimony refers to reliance on the simplest *yet adequate* explanation for an event until experimental analysis with that explanation demonstrates some inadequacy. Most natural science disciplines developed their subject matter in incremental steps over the last several centuries as they faced more complex questions *arising from their work*. In this process they came to take parsimony for granted; not doing so would be difficult for them.

Disciplines (mostly social sciences) concerned with behavior, however, have had to face the whole range of their subject matter from the beginning. An ancient question of our species, perhaps arising as soon as we could talk, was “Why do people do what they do?” Inventing explanations for behavior has been common ever since (e.g., primitive animism). But the continued invoking of such explanations (e.g., the mind) violates parsimony. The non-critical, cultural-linguistic familiarity with parsimony violations seems to make violating parsimony easy and, conversely, adherence to parsimonious explanations difficult, for some disciplines such as psychology. Psychological explanations continue to involve unnecessary and untestable complexity. This itself reduces sensitivity to the value of parsimony, for one can be easily enamored by the apparent profundity that commonly characterizes psychological discourse.

Together with natural scientists everywhere, however, behaviorologists prefer the most parsimonious principles and practices. With parsimony respected, newer or important points stand out more clearly. Inadequacies are recognized and acted upon more quickly. Confusion, misunderstandings, and untestable interpretations exert less reign. These are substantial benefits for *any* discipline.

As a general example of these questions of control and parsimony, recall what speakers from various disciplines say about the ubiquitous crises in education, and reflect: Are their explanations parsimonious? Do they provide much opportunity for taking effective action? The work of disciplines for which affirmative answers to these questions are justifiable portends greater benefits for the world-wide human culture, as developments in independent natural sciences like behaviorology have shown. ❧

Appendix 2: Other Excerpts

*T*hese excerpts, being separated from much—though not all—of their original context, take the form of “notes.” Pointers to the original context in the Fraley and Ledoux paper in *Origins and Components of Behaviorology* (Ledoux, 1997a) are provided by the side-heading chapter indicators, the paragraph sub-heads, and some of the text itself.

From Chapter 2

...Behaviorology is the independent natural science of behavior relations (see Ledoux, 1997b, for a comprehensive definition). Natural sciences respect the continuity and interrelatedness of events in space and time that accumulate in, and present as, a pattern often called a natural history. Natural sciences *disallow* the inclusion of metaphysical events in their explanatory accounts because allowing metaphysical events to enter explanatory accounts interrupts and disrespects the interrelatedness and continu-

ity of space–time events. (The use of “continuity” and “interrelatedness,” “accumulate in” and “present as,” and “pattern” and “history,” result from the attempt to undo the bias stemming from the anti–scientific *agency* cryptotype inherent in the English language; see Baum, 1995.) “Scientific” disciplines that *allow* (even if they may not encourage) metaphysical events to enter their explanatory accounts are not natural sciences and are usually referred to as social sciences...

...Behaviorologists study behavior and its contextual relations. They do not explain behavior with “appeals to events taking place somewhere else, at some other level of observation, described in different terms, and measured, if at all, in different dimensions” (Skinner, 1972, p. 69), nor do they study minds, souls, selves, or psyches—some implications of the name “psych”–ology. And they defer to the discipline of physiology to discern information on the physiological bases of behavior in the brain and in the nervous and other bodily systems. (On the other hand they provide physiologists with the information on which to judge which events have bases worth discerning.) While an important qualifying feature of behaviorology is that it addresses behavior contextually, in naming their discipline behaviorologists declined to append additional terms and phrases (e.g., contextualism) to emphasize this fact.

...The concept of “determinism” is used, in the context of behaviorological and other natural sciences, simply in the sense of indicating a starting point with several characteristics. This starting point involves the working presumption that natural events (i.e., events in nature) at least (a) are orderly and lawful, (b) are functionally related to other natural events, and (c) are not related to a–natural, non–natural, un–natural, super–natural, or other metaphysical events.

From Chapter 3

To varying degrees, different classes of contingencies controlled personal commitments to the behaviorology movement. Among the kinds most frequently identified as having been important were those pertaining to (a) the scientific rift (mainly with psychology), (b) behaviorological training, (c) approaches to job markets, (d) improvements in one’s capacity to make scientific contributions to the culture, and (e) control over the organizational infrastructure of scientific verbal communities...

Those types of concerns affected people on a continuing basis. In the early 1990s, the small, behavioral division of the American Psychological Association (APA) published the Winter/Spring 1992 issue of its newsletter, *Division 25 Recorder* (Vol. 27, No. 1). As was typical of prior and subsequent issues, this one contained a variety of examples of these concerns. Four will be mentioned.

Two examples come from the “For the Record” column. One example regards an APA document on guidelines for school redesign and reform. The columnist (the author was not listed) lamented “that our Division’s perspectives, interests, and expertise are underrepresented...[in this document]” (p. 3). (b) In the other example, the columnist took umbrage with the advertising for a new book on cognition. The advertising said:

Is thought confined within the boundaries of each individual mind? Today, psychologists are pioneering a new alternative to this classic view, claiming that our thinking is shaped by others... (p. 3)

The columnist commented:

This may be a “breakthrough volume” for APA, but it is hardly startling news to behavior analysts. This “breakthrough” is a central thesis of Skinner’s

(1974) analysis of private events and consciousness—and has been since 1945 (Skinner, 1945). (p. 3)

Two other examples come from another column (Book and Journal Notes) which reprints abstracts. One example concerned an abstract on a study of the extent of misunderstandings about Skinner's work. The abstract said: "Results indicate that subjects at all levels of education hold several myths concerning Skinner's work" (p. 5). In the other example, a report on codes of ethics discussed the resistance among psychologists even to suggestions that they consider tying the use of therapies to evidence of the effectiveness of those therapies. One practitioner "predicted a bloody fight... and complete defeat of the [suggestion]" (p. 10). All this came from a single issue of the *Recorder*.

The scientific rift. ...The scientific rift between the independent natural science of behaviorology and the social sciences focused on behavior, especially psychology, involves a fundamental paradigmatic incompatibility. This paradigmatic incompatibility arises from several related areas ranging from the irrelevance of physiological subject matter to ontological fallacies and quality of science issues.

Physiology is a valid natural science alternative to non-natural mentalism that is not directly relevant to the concerns of behaviorologists engaging in a science of behavior relations. Physiologists shun non-natural assumptions. But some tend not to pursue their analyses of the causes of overt behaviors to more remote but directly manipulable antecedent events. Instead they often turn uncritically to internal cognitions. However, whether those cognitions are real events or not, the independent variables that *are* relevant to practical concerns always remain in the behavior-controlling environment or milieu. Inter-nerve variables can be seen either as playing a mediating roll between independent variables and behavioral manifestations, or as comprising the collateral physiological *basis* of behavior—collateral with respect to the environmental variables of which both are a function. In either case the inter-nerve variables do not lend themselves to feasible intervention even if one could possibly determine, on given occasions, what changes within those nerves might be worthwhile. Thus the capacity for control over behavior cannot accrue through analyses focused on nerve functions.

Some physiologists, apparently influenced by psychology, have accepted mentalistic assumptions. They interpret their work about how bodies mediate behavior as if it sheds some light on an internal genesis of behavioral events without benefit of a natural history. Others uncritically accept concepts such as information processing, relating their physiological findings to this or whichever theories of mind these findings seem to match (Skinner, 1971, p. 195). But as physiologists become apprised of behaviorological principles, they will be in a position to render more useful interpretations about behavior. They can work toward completing the account of what is happening inside an organism when it behaves. They will be able to describe the changes that *are* the internal effects of the environmental variables as these select the behavior that occurs on future occasions. And they could continue to develop a practical behavior-controlling technology at the physiological level to complement that at the behaviorological level.

Skinner (1938) wrote more than fifty years ago:

...not only are laws of behavior independent of neurological support, they actually impose certain limiting conditions upon any science which undertakes to study the internal economy of the organism. The contributions that a science of behavior makes to neurology is a rigorous and quantitative statement of the program before it. (p. 432)

Skinner (1966) had summarized scientific–rift issues clearly in his preface to the seventh printing of *Behavior of Organisms*. He first noted that evaluation of that book should consider its historical setting. This setting mainly included Tolman’s *Purposive Behavior in Animals and Men*, which had appeared six years before Skinner’s book, and Hull’s *Principles of Behavior*, which appeared five years after Skinner’s book. Skinner later continues:

Although all three books are ostensibly concerned with explaining observed behavior in terms of observable conditions and events, both Tolman and Hull quickly became preoccupied with internal states and processes. Some such move is inevitable so long as an effort is made to characterize the interchange between organism and environment as input and output. Output can seldom if ever be related to input in any simple way, and internal activities are therefore invented to make adjustments. It is not surprising that modern cognitive psychologists should have been strongly influenced by information theory, where a system is said to convert input into output by acquiring, processing, storing, and retrieving information. Activities of this sort are modern versions of Tolman’s substitutes for mental processes. Although Tolman insisted that his behavior–determinants were “to be discovered, in the last analysis, by... experiments” and that “they have to be inferred ‘back’ from behavior,” he nevertheless made them his primary objects of inquiry, and this has set the pattern for cognitive psychology. (p. xiii)

With respect to the current alternative initially presented in his first book, Skinner went on to say:

As the power of the analysis has grown, more and more complex behavior has been studied, under contingencies that approach the subtlety and complexity of the contingencies to be found in the environment at large. More of what the organism is doing at any given time is analyzed. Multiple stimuli and multiple responses compose complex systems of concurrent and chained operants. Experiments may last for weeks rather than for the standard hour of *The Behavior of Organisms*. Special environments may be maintained from birth. The apparatus required for all this is necessarily much more elaborate: simple relays, timers, and counters have given way to solid state circuitry and computers. *All these advances were facilitated by a formulation which emphasized behavior rather than supposed precursors of behavior and observable variables rather than inferred causal states or processes.* (p. xv; emphasis added)

Behaviorologists, in organizing their discipline, were acting on the scientific rift demonstrated by those differences in formulations.

Capacity for contributions to the culture. ...The importance of appearing as a master of frontier thinking in some disciplines fuels eager quests for better concepts, even if these are refinished “rip–offs” of other’s well–established concepts which (as one behaviorologist who asked not to be identified puts it) “may be a thing of rags and patches by the time they get through with it.” But perhaps behavioral material becomes less palatable to mainstream psychologists when, moved to behaviorology, it becomes more explicitly identified as the intellectual property of another independent discipline.

From Chapter 4

The relevance of contemporary professional organizations. ...As of this writing, the Association for Behavior Analysis (ABA) is playing out a perhaps necessary role. ABA occupies a transition niche in the evolution of behavioral science. Organized behavioral psychology struggles, in ABA as behavior analysis, between leaning toward remaining a facet of psychology or reorganizing as a separate and independent natural science discipline. As such, ABA and behavior analysis sit *between* the evolutionary branches of organized (mainstream) psychology and the organized independent discipline of behaviorology as these two emerge from their shared historical trunk (Ledoux, 1997b).

A journal for TIBA. ...Lawrence Fraley had accepted the assignment from the Executive Board to develop a journal proposal. Throughout 1988, he investigated various options for a journal for TIBA. Members believed that the coalescing verbal community of behaviorology would benefit substantially from the identity and credibility afforded by a quality journal of behaviorology. But such a project would involve breaking some new ground. In keeping with the its organizational approach, TIBA would not permit economic contingencies to degrade the scientific and philosophical integrity of the organization. So Fraley kept plans and estimates non-contingent on economies of scale. Unlike other journals with which TIBA members were familiar, a transition to desktop publishing would enable less expensive computer technologies, at least in part, to replace costly conventional printing services. An early cost estimate for runs of 100 perfect-bound copies, based on a master copy produced on a desktop computer followed by copy center reproduction, was attractive relative to the cost for comparable journals.

Early discussions about a new journal focused on several related difficulties: With so few early members in TIBA and the designed anticipation of slow growth, initially a limited number of potential authors would be available to furnish high quality articles. Questions were raised about the image of a journal that repeatedly offered the works of a small band of contributing authors, and about how much time would pass before the journal attracted more authors. Could the shortfall be overcome by high quality articles from persons peripheral to the discipline? The option of publishing almost anything just to get started and hoping for high quality submissions later was rejected as inappropriate for TIBA. Yet, why should authors of note contribute their best works to a journal with an initial circulation of only a few dozen copies. What could be said that would persuade them to do so?

In response, some suggested that since all significant scientific movements begin small, smallness *per se* need not be regarded as an image problem as long as excellence is maintained in the quality of the product. The emerging discipline needed a quality journal, and a relatively small number of dedicated behaviorologists could produce one. It might have to be issued at a low frequency, or even be occasional rather than periodical, since the contingency to meet deadlines at the expense of quality would also not be acceptable within TIBA. The reviewing arrangements being contemplated were among the most rigorous and involved in scientific publication circles. They included substantial assistance to authors in shaping high quality articles. Gradually increasing membership and institutional subscriptions would both widen the audience and encourage more authors.

At its 26 May 1989 meeting, the Executive Board reviewed and reiterated elements of journal policy: The journal would feature a balance of article types and would main-

tain quality through careful reviewing procedures. As a member of the incipient publications board, Fraley developed an extensive and detailed proposal for publication operations within a large TIBA organization as envisioned for the future. He also drafted an interim proposal suited to *launching* the new journal and forwarded this to publications coordinator Cheney. By the fall of 1989 about a dozen authors had made commitments to submit specific works, the first of which began to arrive in November. Some people indicated that they were prepared to treat this journal as their principal publication vehicle and promised multiple submissions. But some of those early easy promises would prove difficult to fulfill, especially for behavior analysts who were not really committed to the behaviorology *independence* movement. Several of these would soon commit more explicitly to behavioral *psychology* and subsequently renege on earlier agreements to submit articles.

From Chapter 5

Cultural design and space settlements. An early attempt at cultural control involved the Vicos Hacienda project in the 1950s. In that project anthropologists actually did design and implement new cultural practices. They assisted the residents of the hacienda, who had previously been vassals of the land, to funnel the fruits of their labor back into their own society. The project was deemed controversial within the anthropology community. Colleagues criticized project leaders, perhaps unfairly, claiming they had used their scientific powers to impose their own cultural values on the hacienda community.

Such rifts might presage efforts to design the cultural aspects of off-Earth settlements. Space settlements will require cultural practices respecting new kinds of values that will also have to be conditioned by design. In the years since the Vicos Hacienda project, the interest of the scientific and lay communities in the cultural aspects of off-Earth settlements continually increased, and with it more concern over the values question. The interest arose because the differences in both physical and social environments, between current Earth-based cultures and what might be viable off-Earth cultures, must be considered. But by the 1980s only the physical environment concerns had generally received attention while the social environment concerns had been neglected. Proponents of off-Earth settlements began asking scientists of both anthropology and behaviorology to help design cultural practices respecting new kinds of values that would also have to be conditioned by design—in some cases values not shared by the cultures of the designers, nor, sometimes, by the designers themselves. They asked these scientists to help design cultures whose practices would enable people to thrive, not merely survive, under the alien physical and social conditions, the alien values, of a colony in space, on the moon, on the surface of Mars, or wherever (see Beach, 1991a, 1991b).

Adjustment problems of individuals contemplating the separatist movement. ...For some, the discipline of psychology presented a quandary with respect to its apparent and important mission and its non-natural science. For them, resolution of the quandary was complicated because organized academic disciplines expand or fade on a timetable longer than that used to measure the productive lives of the individuals involved. Did psychology exist to study behavior, psyches, or nerve functions? If it existed to study the behavior that yields environmental effects, its cognitive and mentalistic science was inappropriate and inadequate. Behaviorology was organized to pursue that mission with a more effective science. If psychology existed to study psyches, it focused

on figments that, from a natural science perspective, existed only in the assumptions of those pursuing the studies. If psychology existed to study the nerve functions that occur with behavior, it intruded on physiology.

...The exposure of psychologists to behaviorological science has often been limited to parts of one or two courses, or perhaps only parts of one or two chapters, which are usually out-of-date, from an introductory psychology text. These amounts have been described, by some psychologists in conversations with the authors, as being adequate behavioral training at the undergraduate level. But these amounts do no justice to even a superficial survey of basic principles and practices. The typical learning chapter barely covers the acquisition of new behavior. It says next to nothing on all the possible changes in behavior after acquisition. The suggestion that this is adequate only indicates further those speakers' lack of comprehension of the extent of a minimal behavior science repertoire. That repertoire can hardly even be surveyed *by itself* in a one-semester course. To develop a minimal yet still meaningful, usable, and fluent level of knowledge and skills takes mastery of several comprehensive and systematic courses. (See Ledoux, 1997c, for some details on possible behaviorology curricula.) If these psychologists construed *their* professional tasks to be those that behaviorologists have defined for themselves, they would face a repertoire deficit that would typically take years of new study to remove.

The *implications* of coming to comprehend behaviorological science would prove costly to mainstream psychologists in other ways as well. Many kinds of immediate and implicit deferred costs threaten professional scholars when they come to understand not only that their training has been inadequate *for their tasks*, but also that to gain adequate training would require substantial relearning of basics. Such costs are usually too great for an established professional person to bear. Rationally, a person might instead resist, in some cases ignoring scientific evidence and eschewing proper academic debate. Often the resistance takes the simple form of resort to available counter-controlling arrangements embodied in the politics of scientific verbal communities (e.g., denying tenure or promotion to behaviorally oriented faculty) or to remedies available through appeal to other cultural agencies (e.g., law) for help and protection.

From Chapter 6

Disciplinary boundaries, and control. ...Both predictive and *controlling* sciences of *group-produced* effects are possible. Although group effects are necessarily produced by the summation of the behavior of individuals, a science of group effects *can* support intervention technologies in which the analytical repertoire of the cultural engineers does *not* penetrate to the level of individuals. For instance, composers and conductors can reliably produce prescribed group effects without knowing the details of how any particular orchestra member plays his or her instrument. That is because the audience-appreciated properties of the group effect are characteristics of the combined products or activities of the behaving performers. The contributing individual producers of that group effect are not each producing a small one-person version of the group effect appreciated by the audience. What an individual contributes is different—often extremely so—from the appreciated group effect.

Still, the level of the behavior of those individual players is the only level at which interventions pertinent to the group effect can be *functionally* effective. Only when intervention attempts, intended to alter the properties of the hearing response of listen-

ers, reach down to affect the behavior of all or some of the individual players can those group level interventions possibly work. And sometimes the interventions to alter the group effect *must* address changes in just *one* individual's behavior which otherwise would radically alter the group-produced effect. Envision a composition that requires string players to pluck their strings for a certain section of the piece. Consider the effect if, during the period when all other string players are appropriately plucking their strings, one player continues to bow his or her strings. The resulting group effect would be altered in such a way as to evoke very different emotional responses from the audience than would occur if all were plucking. In this instance, an intervention strategy to alter that total group effect need only, but must, reach down to alter just the behavior of that individual player. Culturologists would have few options if their understanding of human behavior were limited only to group effects, and if their intervention technologies were limited only to group-level controls. They would have few alternatives if they were unenlightened as to the workings of events at the individual level.

From Chapter 7

Conclusion. ...Having demonstrated needed contributions in the vast range of applied behavioral fields from advertising to zoo keeping—including child care, education, work, leisure, art, academic pursuits, even science itself—behaviorological scientists see further contributions as not only probable but likely vital. Assuming behaviorologists turn out to be correct about the vital importance of behaviorology, if they do not prevail in the effort to put behaviorology into wide use, then the culture—and perhaps eventually the whole planetary biosystem—will likely suffer, possibly approaching extinction. Conversely, if they succeed in that effort, then the culture and bio-system will benefit, becoming more able to survive. 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, of course, that that was what was at stake, and so they incurred the costs of organizing the behaviorology movement and discipline. ✎

Appendix 3: Experimental and Applied Course Sequences

The paper on behaviorology curricula in higher education (Ledoux, 1997c) left the sequences of courses in the experimental and applied areas to the reader's surmise. These sequences are presented here.

Experimental Course Sequences

The sequences of experimental courses begin after the completion of the requirements for the *Affiliate of Behaviorology Certificate* (ABC). Each possible sequence consists of four courses. The first two of these are the same for each sequence; a pair of "topics"

courses comprise the remaining two. Here is the basic sequence pattern (with prerequisites in parentheses):

Beh. 345: Experimental Behaviorology: A Survey (ABC).

Beh. 350/450: Behaviorology Research Lab: General (345).

Beh. 346/446, or 347/447, or...: The Experimental Analysis of [a Selected Topic] (345).

Beh. 351/451, or 352/452, or...: Behaviorology Research Lab on [a Selected Topic] (350/450, and the corresponding 346/446, or 347/447, or..., topic course).

Applied Course Sequences

The sequences of applied courses also begin after the completion of the requirements for the ABC. However, they also require the completion of Beh. 345 to assure that the student has at least a basic repertoire in *what* is being applied. Each possible sequence consists of four courses. The first two of these are the same for each sequence; a pair of “settings” courses comprise the remaining two. Here is the basic sequence pattern (with prerequisites in parentheses):

Beh. 385: Behavior Technology: A Survey (345).

Beh. 390/490: Behavior Technology Fieldwork: General Experience (385).

Beh. 386/486, or 387/487, or...: Behavior Technology in [a Selected Setting] (385).

Beh. 391/491, or 392/492, or...: Behavior Technology Fieldwork in [a Selected Setting] (390/490, and the corresponding 386/486, or 387/487, or..., setting course). ❖

Appendix 4: TIBA Conventions VI through IX

*T*he Hotel Parador San Javier in Guanajuato (Mexico), was the site of TIBA-6 on 18–20 March 1994. Rebeca Garcia coordinated the site while Nyla Lamm organized the program and chaired the sessions. The convention drew about 21 attendants. Victor Arredondo (*Director General De Education Superior* of Mexico) presented “Reflections on Mexico’s higher education system” as the B.F. Skinner Memorial Lecture (see Arredondo, 1995). At this convention, TIBA took the perhaps counter-intuitive step of capping membership at 150 (see Vargas, 1994; some dangers inherent in such a policy are discussed in the “Science club versus cultural mission” section of Fraley & Ledoux, 1997, Ch. 5).

The University Centre Hotel in Gainesville, Florida, was the site of TIBA-7 on 10–12 March 1995. Anne Kupfer coordinated the site, organized the program, and chaired the sessions. The convention drew about 20 attendants. William Baum presented “Radical behaviorism and the concept of agency” as the B.F. Skinner Memorial Lecture (see Baum, 1995). After this convention, TIBA decided that the convention would at least overlap B.F. Skinner’s birthday and, preferably, the B.F. Skinner Memorial Lecture would be presented on Skinner’s birthday.

The University Inn at Utah State University in Logan, Utah, was the site of TIBA-8 on 17–20 March 1996. Carl Cheney coordinated the site, organized the program, and chaired the sessions. The convention drew about 26 attendants. John Falk presented “Schedule induced behavior” as the B.F. Skinner Memorial Lecture. By this convention, TIBA had added a full day of workshops prior to the regular program.

The John Carver Inn in Plymouth, Massachusetts, is the planned site of TIBA-9 on 20–22 March 1997. Julie Vargas is coordinating the site and the program. The program is to include a special address by Murray Sidman, and Gerald Holton is scheduled to present the B.F. Skinner Memorial Lecture. ❧

Appendix 5: Possible Geographically Based Behaviorology Associations

The natural science of behavior needs to be organized formally and independently if it is to emerge fully to take its place at the natural science roundtable and meet the urgent demands of its cultural mission (see Fraley & Ledoux, 1997). So the development of more geographically based behaviorology organizations can be anticipated. This could parallel the rise of, for example, state and regional organizations affiliated with the Association for Behavior Analysis (ABA) that share in ABA's important political (coalition-and-advocacy) mission. For example, several cooperative regional behaviorology disciplinary organizations would be possible, perhaps based in different countries or sorted on the basis of members' native languages. These organizations may be composed of new as well as established behaviorological scientists from basic laboratories and applied fields around the world. Purposes would include furthering the cultural mission, science development and support, and general disciplinary organizing for all facets of the independent natural science of behavior.

Those groups could also serve to anchor the discipline in each group's geographic area, with mutual support worldwide. Their existence, by extending formal organizing of this independent natural science discipline, could be vital to the success of behaviorological science and its cultural contributions. Natural scientists of behavior everywhere are well-served by continuing to work toward more complete and formal disciplinary integrity. While disciplinary groups around the globe could address the task of formal organizing, The International Behaviorology Association (TIBA) is itself addressing a more precise and exclusive emphasis on the important mission of maintaining and developing the quality and productivity of the experimental science component of the behaviorology discipline through the medium of a small scientific society (see Vargas, 1996). (Also, TIBA may be changing its name to reflect this emphasis; see TIBA, 1996).

Still, behaviorological professionals around the world need organizational structures that support the other facets of their formal independent discipline. These structures could carry out programs of support for the world's increasingly numerous behaviorological scientists and practitioners. Organizational support is needed to consolidate the independent, natural science status of their discipline and thus to promote vigorously their professional activities. The consolidation of behaviorology could redirect—to those professional activities—the energy that is currently directed either to dealing with “survival in disciplinary limbo” (as some experience in “behavior analysis” where disciplinary status is compromised by the competing claims to ownership of that label) or to dealing with “survival in contradiction” (as some experience in psychology, a disciplinary entity that accepts non-natural events in its explanatory accounts and so is called a social science). (Why other existing organizations do not fill these needs was comprehensively addressed in Fraley & Ledoux, 1997, Ch. 4.)

The kind of geographically based organizations of behaviorological science professionals mentioned here could well serve those needs. Another chapter in the history of the organizations of and for behaviorologists—as they more formally organize and support the independence of their natural science discipline and so more capably contribute to world behavioral health—is beginning. Behaviorological scientists must have one or more organizations that promote the reality of behaviorological science and scientists worldwide, and that work to establish officially the accouterments of independent disciplinary status including behaviorology's own academic homes and programs. Developments such as these may not make that much difference in the future of behaviorology; the fact of its natural science status may carry enough momentum. Then again, such developments may make all the difference in the world. [See the Addendum of these appendices for where to find an introduction to subsequent developments.] ❧

Endnotes

These appendices are either new, short information pieces, or they are parts that were originally in one or another of the papers in *Origins and Components of Behaviorology* (Ledoux, 1997a) but which were more appropriately included here. Regardless of their origin, they were prepared for, or revised before, inclusion in this volume.

The author thanks David Feeney, Lawrence Fraley, and Glenn Latham for comments on some of these appendices. Address correspondence regarding these appendices to the author at SUNY-CTC, Canton NY 13617-1096 USA. ❧

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Addendum

*F*or some subsequent developments, see the *Afterword* written for this year–2002 Second Edition of Ledoux, 1997a. This *Afterword* appears at the end of the book, after the Index. It extends the concerns of Appendix 5, and reports some of the initial activities that begin to address these and other concerns.✱

Quoted*

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...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. (p. 313)

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*From Appendix 2 (Ch. 7 section) of Ledoux, S.F. (1997). *Appendices*. In S.F. Ledoux. *Origins and Components of Behaviorology*. Canton, NY: ABCs.

Quoted*

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...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.... (p. 158)

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*From Ch. 7 of Fraley, L.E. & Ledoux, S.F. (1997). *Origins, status, and mission of behaviorology*. In S.F. Ledoux. *Origins and Components of Behaviorology*. Canton, NY: ABCs.