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The Journal of Animal Behavior Technology (JABT) is a fully peer-reviewed journal of essays of interest to behaviorologists, behavior analysts, and animal behavior technologists. JABT publishes essays of all kinds, including review of topics, original research papers, short communications, critical reviews, persuasive essays, theoretical works, technical articles, and commentary.

The Association of Animal Behavior Professionals (AABP) was founded to promote excellence and a strong commitment to nonaversive methods among behaviorologically oriented technologists of companion animal behavior. The AABP seeks to establish a community of members aspiring to and sustaining these principles.

Audience: Behaviorologists, behavior analysts, animal behavior technologists, animal trainers.

Editorial

Welcome to the 2018 issue of the *Journal of Animal Behavior Technology*. In this issue we have three articles. Continuing our efforts to expand the reader's repertoire with respect to behaviorology as a discipline, we reprint two articles previously published in *Origins and Components of Behaviorology* (1997, and subsequently in the second edition, 2002, and the third edition, 2015) available through the TIBI web site at: https://www.behaviorology.org/oldsite/origins_book_complete.htm. These are printed with permission. We believe these articles will provide an expanded depth of appreciation for the discipline in which we operate as animal behavior technologists. The next issue will include more current developments. The third article is a discussion on primary and secondary reinforcers.

Enjoy!

Dr. James O'Heare

JABT Editor

Ethical Treatment of Animals in Research

This is the stance of the *Journal of Animal Behavior Technology* on the use of animals in research.

Animals should not be harmed in the study of their behavior. *Journal of Animal Behavior Technology* recognizes a higher standard of ethical responsibility to the rights of animals under scientific investigation than is common. Full informed consent should also be secured from the guardian(s) of any animal used in any study. For any study that makes use of aversive stimulation, full informed consent must be secured from the participant themselves (this is only possible with human participants). With regards to harm, broadly speaking, an animal is harmed if he or she is caused nontrivial aversion, distress, significant loss of opportunity, or physical harm. All reasonable precautions are to be taken to prevent the causing of harm to any animals and human participants. The Journal of Animal Behavior Technology will not publish essays based on research carried out by the authors or those under their direction that caused harm to the subjects.

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An Introduction to the Origins, Status, and Mission of Behaviorology: An Established Science with Developed Applications and a New Name

Stephen F. Ledoux*

Behaviorology? What's that? Where does it come from? How does it differ from other disciplines and fields that evince some interest in why people do what they do? How is it related to other disciplines and fields? How much is encompassed by behaviorology, such as its contributions? Why should anyone learn anything about behaviorology? Questions such as these typically arise when people first come across the term *behaviorology*. This paper presents some initial answers to these questions by surveying an analyzed history of the origins of the discipline of behaviorology and the behaviorology movement. The survey includes some corroborative evidence from the status of behaviorological science in China. (For elaboration of the points introduced in this paper, see Fraley & Ledoux, 1997, and Ledoux, 1997a, b, c.)

What is Behaviorology?

Put too simply, behaviorology is the science and technology of behavior relations. This may appear similar to the way some other disciplines define themselves. A more elaborate definition, an expansion of the definition written for the By–laws of TIBA, The International Behaviorology Association (an expansion undertaken when experience indicated some lingering confusion over the discipline's range and depth of coverage), should help discriminate between behaviorology and those other disciplines: Behaviorology, a comprehensive discipline with philosophical, experimental, analytical, and technological components, is the natural, life science, emphasizing the causal mechanism of selection, that discovers, interprets, and applies the simple and multiple variables that are in functional relations with the simple and complex, overt and covert behaviors of individual organisms (especially people) during their lifetime (and beyond, with respect to cultural practices), and that takes into account socio–cultural and physical variables from the environment as well as variables from the biological history of the species. Here is a simpler way to present that thorough definition:

The discipline of behaviorology,

- being a comprehensive discipline with philosophical, experimental, analytical, and technological components,
- is a natural, life science;
- emphasizes the causal mechanism of selection;
- discovers, interprets, and applies the simple and multiple variables that are in functional relations with the simple and complex, overt and covert behaviors of individual organisms (especially people) during their lifetime (and beyond, with respect to cultural practices); and
- takes into account socio–cultural and physical variables from the environment as well as variables from the biological history of the species.

* This paper was originally part of an invited address to faculty and graduate students of the School of Management, Xi'an Jiaotong University, Xi'an, Shaanxi, People's Republic of China, 20 March 1991, as well as to other Chinese audiences. It was prepared for those and other audiences, including students, who wanted a brief introduction to the concept of a discipline of behaviorology separate from psychology. As indicated by its title, it also serves as the introduction to a larger work (i.e., Fraley & Ledoux, 1997).

Where Does Behaviorology Come From?

As a current *discipline*, behaviorology comes from the interaction of the previously developed behaviorological science and technology with the current behaviorology movement. The conditions under which the original behaviorological science and technology developed gave rise to the current behaviorology *movement*, and that movement has enabled recognition of the current disciplinary status of behaviorology.

Science and Technology Origins

The science, and the technology originally developed from the science, began about sixty years ago, early in the career of B.F. Skinner. Paradigmatically, Skinner never really was a psychologist in the sense of accepting the *transformation* paradigm of psychology. Sometime during his work in the 1930s, he began using the life–science *selection* paradigm, typical of the natural science of biology, in the task of developing a natural science of behavior, especially the behavior of people. Skinner was operating within a department of psychology, a social science, at Harvard University. However, he did much of his pre–graduation work under W. J. Crozier, the head of the physiology branch of Harvard's biology department (Skinner, 1979, p. 16). Crozier had been a student of the biologist Jacques Loeb, and both Crozier and Loeb had emphasized the causal mechanism of selection in their natural science work. Skinner, perhaps without initially realizing he was doing so, transferred the concept of selection from biology to behavior relations. He thereby brought a particular, natural science paradigm to bear on the questions of a scientific study of behavior.

With respect to behavior, selection refers to the lasting effects, on a person's or other organism's behavior, of the consequences of that kind of behavior. For example, a child who must ask loudly and repeatedly for a cookie (the response) before receiving one (the consequence) is a changed person. He or she is changed physically and thus behaviorologically by the occurrence of the consequence. The consequence alters the bodily structure in a manner that can be observed at the physiological level and at the behaviorological level. At the physiological level the alteration can be seen as nervous system changes the specifics of which physiologists are making better known. At the behaviorological level it can be seen as a changed behavior repertoire in that, in the future, asking–for–cookie responses will be even louder and more persistent. These inseparable effects occur because receiving that past cookie selected for loud and persistent asking. Selection causes physical changes now that are seen as altered behavior later.

Behaviorologists address those altered behaviors by referring to the probability of behavior and changes in that probability occasioned through selection by consequences; consequences select behavior to occur more often or to occur less often. In selection causality on the behaviorological level, a response A is followed by (and usually has actually produced) a consequence B. The occurrence of B leads to responses of *class* A being more, or less, likely to occur again in the future. That is, in the selection causal mode, B *affects* (*class*) A. Selection is thus a type of causal mode different from the more familiar mechanical causal mode where A *leads* to B. In the mechanical mode, for example, too high a temperature in cooking (A) burns the food (B), or, in reflexes, an increase in light (a stimulus, A) elicits a decrease in pupil size (a response, B). With selection causality, at the level of the behavior of organisms during their lifetime, behavior is selected *by its consequences* to occur again or not; from this arises the more common term, *selection by consequences* (see Ledoux, 1997d, for more detailed terms).

The specifics of selection causality operate differently at other levels of life science (while the shared use of selection causality attests to the interrelationships of the disciplines at all levels of life science). On the biological level, selection causality affects species through natural selection. On the level of cultures, selection causality involves selection of cultural practices. (Among natural sciences, a mechanical causal mode is emphasized in physical sciences while the causal mode of selection is emphasized in life sciences.) Behavior is functionally related to many other variables as well, but the selection mechanism is usually a necessary component of those relations

(e.g., stimulus control variables). The relationships found in nature between all these variables and behavior are described by, and often as a group referred to as, the natural laws, or nature's laws, of behavior.

In conjunction with the philosophy of science called radical behaviorism, decades of research by Skinner and those trained in this new approach followed Skinner's use of the paradigm of selection by consequences as the fundamental component of studying behavior. (See Ledoux, 1997a, for an introduction to some basic elements of radical behaviorist philosophy; the selection paradigm is not included as one of those elements even though, for behaviorologists, this philosophy and paradigm may have become inseparable.) The efforts of Skinner and those other researchers produced discoveries of the elementary natural laws involving the behavior of organisms. By the 1950s, those researchers were developing technologies to change accessible environmental variables and so produce behavior change. Further, they were applying these technologies to improve various aspects of the human condition. These applications help people to do more, act better, and behave more effectively in all facets of life, for example, in child care, health care, education, daily living, work, leisure, art, entertainment, academic pursuits, and even science itself.

Ever since those early discoveries and applications, new generations of researchers have continued to make further advances, discovering more complex principles and interactions and developing more complex technologies. The application of these technologies extends into ever wider areas, continually improving personal and cultural practices. The term *behavioral engineering* provides a general description of these applications for it suggests both the technical process of changing the relevant environmental variables and the resulting changes in behavior. (This name, behavioral engineering, supersedes an earlier name, behavior modification, partly because the older name is misleading. The older name implies that behaviorological practitioners directly manipulate behavior whereas they actually arrange—engineer—changes of the particular environmental variables related to the behavior of concern, with the result that the behavior changes.)

Movement Origins

The behaviorology movement encompasses the efforts of behaviorologists in developing professional organizations and academic homes to preserve and extend the behaviorology discipline and its contributions to humanity. This movement arose from the conditions under which the original behaviorological science and technology developed. Those conditions involved incommensurable differences between a discipline of behaviorology and the discipline of psychology, especially concerning their respective paradigms. Incommensurable differences are differences that are incompatible, and that cannot be compared like those between apples and ghosts.

Lett (1987) explains paradigmatic incommensurability as "the question of whether rival theories can be compared and evaluated according to a standard measure" (p. 35). Lett also points out that alternative paradigms need not be incommensurable:

> If two paradigms agree about the nature of the problem to be solved and about the appropriate means of solving that problem, they are commensurable. Furthermore, paradigmatic commensurability is a relative matter. Two paradigms may agree about the problems to be investigated but disagree as to the means of solution. Scientific evolution and "scientific creationism," for example, are both concerned with the origins of the human species, but the two paradigms have radically different epistemological principles. If one paradigm chooses to rely upon experience as its epistemological foundation, it can make no impact upon a paradigm that appeals ultimately to revelation. (pp. 35–36)

So, "if the participants in such a debate restrict themselves to the terms and assumptions of their own paradigm, they can have nothing to say to one another" (Lett, 1987, p. 36). They are incommensurable (see Ulman, 1992, for elaboration).

Behaviorological science had arisen and existed for some decades, mainly within the realm

of psychology, before the accumulating effects of incommensurable differences required independence–oriented actions. During this time, behaviorological professionals had become accustomed to trying (and failing) to change psychology fully into a natural science of behavior. By the 1960s the differences between the two became more openly incommensurable. Events in later decades showed more and more why the earlier strategy of trying to remake psychology was inappropriate (see Fraley & Ledoux, 1997). But the momentum of decades of that strategy was hard to break. Behaviorological professionals found considering other strategies difficult. Some of them finally did so however, and thereby initiated the behaviorology movement.

Effects of incommensurability. By the 1970s, behaviorological professionals were experiencing the effects of the incommensurability of their science and psychology. (By this time they were called behavior analysts, the name still used by, among others, some professionals trying to engage in behaviorological science, with its selection paradigm and radical behaviorist philosophy of natural science, within the social science of psychology.) The effects of incommensurability are varied. Some concern the extent to which behaviorology can make its contributions to the culture. Others concern employment opportunities and the control of disciplinary infrastructures. And still others exist also (Fraley & Ledoux, 1997).

One effect of incommensurability was that within psychology the science of behavior was increasingly underrepresented, underfunded, de–emphasized in most departments, and simply dropped in others. As a result, students were less and less able to receive training in the science of behavior. (Ledoux, 1997b, provides a description of some behaviorology curricula in higher education. The contents of these curricula reflect the depth and range of the behavior science training unavailable in psychology.) Students covered few courses related to the science of behavior in their degree programs because few were offered by psychology departments. More likely, the student's required exposure to the laws of behavior involved a single chapter, or part of a chapter, from the twenty or so included in standard introductory psychology textbooks. And those chapters not only typically misrepresented behavior science (e.g., the usual confusion between negative reinforcement and punishment) but also they were increasingly out–of–date; researchers were reporting advances in behaviorological work mostly in journals *outside* those of the usual psychology literature perused by textbook authors.

With so little exposure, substantial interest in the science developed in fewer students. Those who did become interested usually did so due to studying under a behaviorologically oriented faculty member. But the opportunities to do that were also decreasing. After reducing the number of behavior science courses, programs reduced the number of behavior science faculty. So even interested students could not easily be fully trained in the science of behavior and its applications; not enough courses or teachers were accessible. But these students still had to take plenty of courses covering unparsimonious, non–natural science in their programs. As a consequence these fewer, newer natural scientists of behavior were less trained in the available behaviorological science than they might have been (and perhaps less trained than the earlier generations of faculty and personnel whom they replaced). So they were likely to be less effective than they could have been as scientists, as teachers, and as behavioral engineers, and so were *their* students, and so on.

If those effects of incommensurability were the only ones, and no effective actions occurred to change that trend, the long term result could have been the practical disappearance of an effective and advancing scientific and systematic approach to people's behavior and how to change and improve it. Fortunately, positive effects of incommensurability were also detectable, along with appropriate and supportive actions to consolidate and further advance the science. For instance, personnel in various cultural agencies were increasingly looking specifically toward behaviorological science as the provider of effective behavioral engineering, relevant to their concerns. To mention but a few, these personnel included (a) educators looking beyond the typical resources of their field, (b) workers and managers in

business and industry looking for ways to increase productivity and job satisfaction, etc., and (c) government units, especially at state level, responsible for services to citizens with disabilities. (For example, in the 1970s some California state officials wanted to spot whether or not applicants for certain jobs had specific training in behavior management. To make this easier to do, Joseph Morrow, a behaviorological scientist at California State University, Sacramento, arranged for students to receive a "Certificate in Behavior Modification"—using the designation common at the time—if their studies included a particular pattern of courses that specifically expanded their skills in the area of behavioral engineering.)

Early independence actions then the behaviorology movement. From the 1960s through the 1980s, both positive and negative effects of incommensurability prodded some initial actions, early moves towards independence. Behavior analysts founded numerous behavioral journals and their own professional organizations. Most of these were separate from psychology's literature and organizations. None of them, however, openly espoused the disciplinary status implied by the incommensurable differences with psychology. Some behavior analysts and behaviorological professionals also founded academic programs (especially at the graduate training level). Many of these programs were also organizationally independent of psychology through their association with academic departments representing various applied behavioral *fields* which could be informed by various disciplines, including behaviorological science (e.g., special education). A few programs functioned with the status and structure of a separate *discipline* by forming a natural science training alternative in a distinct department separate from their respective university's psychology department. These departments, typically describing themselves with the term *behavior analysis* (the term behaviorology not yet being in use at their founding), did begin to reflect the independent disciplinary status implied by the incommensurable differences with psychology.

The effects and implications of incommensurability made the need for separation into independent disciplines increasingly clear. Some behaviorological professionals finally began to take the necessary actions. They contributed to the academic debates (which began in earnest in 1984) in the behavior–analytic literature about an appropriate name and directions for the comprehensive natural science discipline concerned with behavior relations. In 1987 they (a) formally recognized the separate and independent status of that discipline, (b) accepted *behaviorology* as the name denoting that discipline, and (c) founded the scientific organization now called The International Behaviorology Association. By the end of 1992, their organizational efforts were reflected in (a) a continuing series of annual conventions with, as a sample, the second in Mexico at the Los Horcones community in January 1990, and the fourth in New Orleans, USA in January 1992, (b) a newsletter, originally called the *TIBA Newsletter*, and now called *Selections*, in its fourth volume, (c) a carefully planned, comprehensively peer–reviewed disciplinary journal to appear in 1993, called *Behaviorology*, (d) a non–copyright–retaining journal, with short–process peer reviewing, in its third serial, called *Behaviorological Commentaries* (which was later to be renamed *The International Behaviorologist*) for articles that fall between the respective domains of the newsletter, *Selections*, and the journal, *Behaviorology*, and (e) the allocation of one–fourth of all dues explicitly for the support of behaviorological research.

How Does Behaviorology Differ From Other Disciplines and Fields?

The original question was longer: "How does behaviorology differ from other disciplines and fields that evince some interest in why people do what they do?" An initial response is that behaviorology is interested in more than this. It is also interested in *what can be done* about what people do. Encompassing this difference, and substantiated by other differences (in philosophy of science, subject matter, methodology, etc.), is the fundamental and incommensurable difference in paradigms between behaviorology and these other disciplines and fields. So most of this answer focuses on the paradigm difference. (See Vargas, 1991, whose names for the paradigms are used

here; also see Fraley & Ledoux, 1997, for additional details, including consideration of the other differences as well.)

Of Paradigms and Eclecticism

The two paradigms are the *selection* paradigm Skinner had adopted from biology and the *transformation* paradigm of psychology and some other disciplines. Psychological subscribers to the transformation paradigm are most interested in positing (with emphasis on a hypothetical–deductive model) the causes of behavior chiefly in the transformations that they believe occur inside the person. These are the transformations that external variables (inputs, to use current cognitive terminology) seem to undergo before becoming apparent as behavior (outputs) in a basically mechanical causal mode. Since this paradigm does not support much interest in the inputs or outputs for their own sake, the possibility of, and consequently concern for, effective control is diminished. In contrast, behaviorological subscribers to the selection paradigm are most interested in discovering (with emphasis on an inductive model) the causes of behavior chiefly in the genetically affected, potentially manipulable behavior–environment interactions, with selection by consequences as the fundamental causal mode. Since this paradigm explicitly supports interest both in behavior for its own sake and in the variables of which behavior is a function, the possibility of, and consequently concern for, effective control is enhanced.

Psychology's transformation paradigm has played a particular role regarding eclecticism. Psychologists have generally considered their discipline as an eclectic aggregate. Their eclecticism seemed originally pragmatic. It allowed them to search along multiple paths for an effective approach to the general question of "Why do people do what they do?" But could it allow them to find such an approach? They were convinced that multiple, eclectic paths constituted the best course for them to follow. However, their eclecticism had no built–in need for resolution. They could continue working under eclecticism indefinitely (and have been doing so). Eclecticism actually *does not* require either ultimately adopting, or even looking for, an effective approach. Indeed, the notion of an effective approach, as in a single, substantive, systematic, comprehensive approach, seems to be anti–eclectic by definition. In addition, as psychologists were to discover, they already *shared* a paradigm, the transformation paradigm, that allowed them their eclectic differences. So the availability of a more effective approach, especially one with a different and incommensurable paradigm, evoked little interest. Psychologists' paradigm and eclecticism remain thoroughly intertwined.

The fact that different schools and approaches divided psychology merely masked its otherwise characteristic transformational paradigmatic unity. Psychologists' stress on eclecticism focused on differences in emphases and particulars of the various schools and approaches. Consequently they have only gradually apprehended their common transformation paradigm. Outside psychology critics often complained about the apparent lack of a unifying paradigm in psychology, thereby casting doubts on the disciplinary status of that aggregate. The critics also seemed influenced by the differences in emphases in the various psychological approaches. So they too were distracted from apprehending the basic paradigm. In defending against these criticisms, psychologists stressed their eclecticism since they had not yet clearly recognized their paradigmatic unity. They stressed it to the point that the previously pragmatic eclecticism became an inherent aspect of their self–description (as passed on by countless repetitions throughout a psychology student's training).

Conveniently, however, the transformation paradigm not only encompasses the similarities of psychologists' perspectives but also allows them their eclectic, even contradictory differences. Most psychologists, regardless of eclecticism or perspective, seem little interested in behavior or the variables of which it is a function. They are little interested in inputs (the variables) and outputs (the behavior). They try to relate these mainly for other reasons. They try to relate these as a social "science" adapting an older (and changing; see Chiesa, 1994) natural science *x leads to y* (or *x is followed by y*) type of mechanical

causality. But psychologists cannot easily relate a given input to a given output as cause. So psychologists presume something must happen to the inputs before outputs occur. Somewhere and somehow the inputs must be changed, *transformed*, into outputs. Those changes, as they do not seem apparent elsewhere, must be happening inside the organism, possibly because of something the organism can be seen as doing. Psychologists try to relate the inputs and outputs to learn something about what they presume is behind them. Psychologists then undertake to tell the world, from their various perspectives, all the things they assume are happening inside the organism, perhaps because of the organism. In placing their interests inside the organism, they keep to their familiar mechanical causality. Now, however, this causality takes the form of *x leads to O leads to y*. Here, O (for organism) represents the various *transformations* different psychologists believe occur inside the organism as inputs are then said to *become* outputs. However, the transformation paradigm does not address the makeup of transformations; it addresses only their position between the inputs and outputs. Psychologists hypothesize, from various perspectives, numerous types of transformations and these can be contradictory and even mutually exclusive. The result is the interplay between the transformation paradigm and eclecticism. The paradigm supports transformations in general, whether agreeable or contradictory, while under eclecticism the latter are automatically tolerated.

Even though the many, specific psychological approaches differ among themselves, they all adhere to the transformation paradigm. Giving them a label like "school" does not change this characteristic. None of them adheres to the selection paradigm. At this level of analysis, only the science of behavior founded by Skinner adheres to that paradigm (but see Ulman, 1991, also). And the selection paradigm is as different from the transformation paradigm as evolution is from creationism.

The Skinnerian Alternative

Skinner was doing research in the 1930s using the selection paradigm; but he was operating *within a unit of organized psychology*. The location of this research may simply be a product of what evolutionary biologists call historical contingency (see Gould, 1989) since Skinner could, and under Crozier's influence almost did, pursue his work officially from within a unit of organized biology. Yet the venue of this research constitutes the beginning of an historical trunk, shared by both behaviorology and psychology. This trunk lasted only for about three decades and has since divided, forming two distinct branches, each with its own continuing disciplinary history.

The roots of this trunk are also as different as the two paradigms and disciplines that shared the trunk before diverging. The history of these roots traces back, in Western culture, to various early Greeks and their philosophies and approaches, and the ideologies of those and other times. As Lerner (1991) reports, the early Western versions of "The empirical and the deductive methods...both arose around 500 B.C. They emerged from a fierce social conflict to determine what sort of society would succeed Bronze Age civilization—a society of free labor or one of slave labor" (p. 62).

The characteristics of the selection paradigm and behaviorology have their Western roots in the preferences of the Ionian Greeks (e.g., Thales, circa 550 B. C. E., and Anaxagoras, circa 450 B. C. E.). With navigation and other needs prompting developments in science and technology, the Ionians preferred the empirical method, its associated philosophical approach known as materialism (which takes matter, nature, as primary, that is, reality exists whether or not people are around to think about it), and the inductive (observation– and measurement– and action–based) approach to knowing. These Ionian roots are found to be ascendant or prevalent in societies during periods of increasing social progress, for example, during the time of the Ionian trading cities when "new societies of traders, craftsmen, and freeholding peasants—the first limited attempts at democracies and republics" (Lerner, 1991, p. 63) were forming, as well as during the Renaissance and the nineteenth century (Lerner, 1991, p. 419) and to some extent the present (e.g., in Japan around the 1980s).

In contrast, the characteristics of the transformation paradigm and psychology have their Western roots in the preferences of the dualist Greeks (e.g., Plato, circa 350 B.C.E., and Aristotle, circa 325 B.C.E.). The dualist Greeks preferred the deductive method, its associated philosophical approach known as idealism (which takes ideas, thoughts, as primary, that is, reality does not exist apart from what people think exists), and the hypothetical–deductive (pure reason and little observation) approach to knowing. These dualist roots are found to be ascendant or prevalent in societies during periods of decreasing social progress, for example, during Greek slave–holding society, during the Western middle ages, and in many ways during much of the 1900s (e.g., see Carl Sagan's 1995 book, *The Demon–Haunted World: Science as a Candle in the Dark*).

Both these roots and their differences have some impact on most disciplines. The branching of the psychology–behaviorology shared historical trunk is not an isolated instance in academic history. It may be part of an ongoing scientific revolution. For example, in his 1991 book *The Big Bang Never Happened*, Eric Lerner argues that these roots and differences are the basis of the competition between big bang cosmology and plasma cosmology. He argues that big bang proponents continue in the Plato–Aristotle tradition, and that observational evidence seems to show their position to be less parsimonious than their competitor's position. Plasma proponents, he argues, continue in the Ionian tradition, and their position seems to be more consistent with observations. Some parallels with psychology and behaviorology, and their paradigm clash, are evident. Even so, whether or not a Big Bang happened is irrelevant to any clash between psychology and behaviorology, and in any case is still unresolved.

Sometimes in paradigm clashes, one position ultimately eclipses or subsumes the other, as when quantum mechanics superseded Newtonian mechanics in physics. The positions co–exist for a time as one develops and advances while the other declines. With other paradigm clashes, such as the one between behaviorology and psychology, the positions co–exist for a time, sharing their history. Then they branch off, going their separate disciplinary ways.

Figure 1 illustrates the shared history, its roots, and its later branching for behaviorology and psychology:

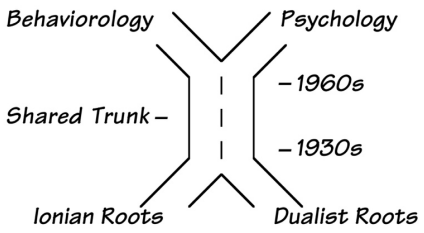

Figure 1. Branching disciplinary tree and philosophical roots.

Different kinds of reasons bring about those different paradigm–clash scenarios. Regarding behaviorology and psychology, how could the shared historical trunk come about? How was Skinner's beginning behaviorological science from within psychology possible, given the incommensurable differences between the paradigms?

Skinner's doing such work from within psychology was possible because the psychology of the time was much more sensitive to differences at the level of schools and approaches than to differences between paradigms. Various schools of thought were already contending within psychology. All these shared the transformation paradigm. But no school was able to demonstrate itself to be better than the other schools nor could they show that other schools were inadequate. So they all had to tolerate each other and co–exist, which they did under the rationale of eclecticism. When Skinner originated the operant approach, it also could not be shown to be inadequate and so it also was tolerated. The fact that the operant approach did not share the transformation paradigm with the psychological schools but was based in the selection paradigm did not originally occasion much comment.

By the 1960s, however, circumstances had changed. Those who continued to advance the science and technology Skinner had originated had come to be known first as operant behaviorists

and then as behavior analysts or *radical behaviorists.* (The latter was more concise since usage of the other names had become blurred over time; now, behaviorologists is the best descriptor, at least for those who are part of the behaviorology movement.) By using the criterion of *effectiveness in action regarding subject matter*, radical behaviorists were demonstrating the value of their science. That same evidence was also showing the various psychological approaches to be less effective and unparsimonious. Yet parsimony receives scant attention in psychology, and *effective action regarding subject matter* was then—and still among psychological approaches today is— but a minor criterion for adopting explanations (see Fraley & Ledoux, 1997, Ch. 5). As a result, the stage was set for various substantial changes in the positions of both psychologists and radical behaviorists. The effects of incommensurable paradigms were paradigmatic differentiation and the subsequent historical separation into officially independent disciplines.

The Reaction for a Non–Natural Science Tradition

Also during the 1960s, psychology was undergoing the "cognitive revolution" (or, depending on one's perspective, "cognitive counterrevolution"). One aspect of that development was psychologists' increased acceptance that they had little interest in behavior for its own sake or in demonstrations of effective control. So they could not convince themselves of much need to heed the concerns of radical behaviorists. But they were not unmoved by those concerns. They were paying more and more attention to the paradigmatic similarity among the various psychological approaches and less attention to their eclectic differences. They began to apprehend the role of their paradigm in emphasizing their similarities without threatening their differences. As a consequence they began to disassociate from any group that did not share their transformation paradigm. This especially meant disassociation with radical behaviorism since several other forms of behaviorism do operate under the transformation paradigm (including interbehaviorism, methodological behaviorism, paradigmatic behaviorism, and Watson's original behaviorism).

Radical behaviorists, after decades of a history shared with psychology, experienced the disassociation as the effects of the incommensurability of the respective paradigms. Before the cognitive movement, psychology had tolerated and benefited from radical behaviorism. However, since the cognitive movement began, psychologists have labeled and treated as dead any non–transformational positions regardless of the facts (see Wyatt, Hawkins, & Davis, 1986). Radical behaviorism was a common target of such unjustifiable slurs because its paradigm was fully incommensurable. Consequently the demonstrated quality and quantity of its research and applications were given less and less consideration. Accumulated scientific evidence for its more parsimonious and practical accounts of behavior no longer received the attention their effectiveness had earned. Instead, political and economic concerns prevailed, with programmatic emphases, funds, and other resources being more emphatically directed towards hypothetical cognitive transformations. The result of these developments was the objective, though not necessarily immediately recognized, differentiation of the transformation and selection paradigms. And this differentiation provided the foundation for the separation of the independent disciplines of psychology and behaviorology.

Also, psychology is not entirely consistent in these matters. It continues to claim *that behaviorism is dead*. However, this is only true within psychology and only in the sense that psychology all but ignores transformational behaviorisms while the work of radical behaviorists is no longer advancing in psychology (although this work does continue to advance in behaviorology and in the efforts of behaviorological scientists who remain employed in units of organized psychology). Yet psychology also claims *that behaviorism still is part of psychology*. This also is only partly true in that various transformational *behaviorisms* continue to exist within psychology. Also, the principles and practices of the first few decades of radical behaviorist research did occur mostly in units of

organized psychology and so are a part of that historical time shared with psychology. Introductory psychology textbooks still faithfully report, usually as part of the chapter on learning, this outdated material, and little beyond it. While over thirty years out of date, that material is presented as though it were the latest material available, which it generally is—*in psychology*. However, the years of advances since the paradigm differentiation of the 1960s are arguably not part of psychology and are rarely covered in those textbooks. (As an exception the text by Poling, Schlinger, Starin, and Blakely, 1990, is somewhat more up to date.) Again, the advances were generally reported in journals (beginning, for example, with the *Journal of the Experimental Analysis of Behavior*) that, being independent of psychology's principal disciplinary literature, are seldom perused by most psychology textbook authors. The benefits from those advances accrue mostly according to the extent to which one has acquired and maintains a verbal and skill repertoire in behaviorology or, at least, in behaviorological science.

The experience in China. The situation of behavior science in the People's Republic of China provides corroborative evidence for the separateness and independence of behaviorology and psychology. Chinese behavior science professionals in Xi'an, Shaanxi, provided commentary on the situation in China to the author while he was there, as part of a faculty exchange, teaching courses on Verbal Behavior, and Behaviorology and Education, during the 1990–1991 academic year.

The discussions uncovered several points of mutual interest. The Chinese use a word they translate as *psychology* to encompass the three sources they currently see for their discipline: traditional Chinese views on why people do what they do, the views adopted from the discipline in the Soviet Union (especially the work originating with Pavlov on reflex/emotional, that is, respondent, behavior), and Western perspectives. The Chinese have included three parts in the Western component of their discipline: psychoanalytic (i.e., Freud), cognitive/mentalistic (e.g., Maslow and Piaget), and behavioral (i.e., the science of behavior originated by Skinner).

The Chinese report a special preference for the Pavlovian and Skinnerian work based on the natural science approach and experimental methods these two share. In part, this preference for Pavlov and Skinner may be due to a particular aspect of Chinese history. The Chinese culture has been less burdened than Western culture has been by philosophically idealist dualism, a dualism that pervades Western culture. So Chinese culture has suffered less from the unscientific separation of phenomena into the different realms of mental and physical (soul/body, spiritual/material, mind/reality) that results from philosophical dualism. Western psychology traditionally prefers the non–physical aspect. (The Chinese language, while it has a rich variety of terms for most of the Western usages of the term *mind*, actually lacks a direct translation of mind as Western psychologists use that term—as a dualistic, uncaused metaphysical cause. Instead, for that usage, Chinese professionals generally use a word that, less appropriately, translates back into English better as "brain.")

However, in the 1950s, Chinese behavior science professionals lost contact with Western developments. They spent the decade of the 1980s trying to update, and thought the update complete. But they were disturbed by what they saw as very few advances in principles and practices, from those missing years, relevant to solving practical, behavior–related problems.

In beginning to look elsewhere for solutions, they are discovering that their update is not complete. It involved little beyond the traditional Western psychology sources (literature, texts, personnel) and these contain little of the substantial behaviorological–science advances from those years. The Chinese are discovering that they have overlooked virtually all the post–1950s advances in principles and practices in the science originally founded by B.F. Skinner. This occurred because at about the same time that the Chinese lost contact, the greatest proportion of those advances began to be, and have since been, increasingly reported and supported *outside*

psychology, greatly reducing access to them from within psychology. The Chinese are also considering the possible reasons for this situation, including the incommensurable paradigm differences indicating and validating separate disciplines. (See Ledoux, 1997c, for a more complete discussion of behaviorology in China.)

Transformations and eclecticism revisited. Are hypothesized, internal transformations a reasonable alternative to behavior–environment interactions? The variety of psychology's transformations may indeed be more initially captivating than the patient discovery and tested application of complex behavioral laws. Transformations seem smoothly consistent with the philosophically dualist cultural history that pervades the milieu of Western society. Are these a rationale for the many people attracted to psychological theorizing? This attraction occurs in spite of the much greater difficulty in later making use of theorized transformations to help solve society's various general and personal problems, compared to the more technological applications of behavioral laws to such concerns. Is this another example of being affected more by short–term variables (e.g., the fun of discussing competing transformational accounts) than by delayed variables (e.g., the later, improved effectiveness, in helping situations, after having studied behavior–environment interactions)?

In any case, some of these transformations are simply mentalistic inventions that violate a basic premise of the natural sciences, namely respect for the continuity of events in space and time that accumulates, link by related link, in a researchable natural history. Hence the scientific status of those transformations is questionable and unparsimonious. The transformation paradigm allows and invites such untestable, metaphysical inventions to enter the chain of space–time events, breaking and thereby disrespecting the continuity of those events. Such transformations, their related paradigm, and disciplines or parts of disciplines supporting that paradigm have thus removed themselves from consideration as part of any *natural science* discipline or field.

Many other transformations are not transformations at all but rather are the physiological bases of behavior, an appropriate subject matter for a natural science such as physiology. For example, neurons firing (in the central or peripheral nervous systems), in ways often but not necessarily connected to muscles contracting or glands secreting, etc., are physiological aspects of the same fact whose behaviorological aspects observers might witness as, say, salivating or the movements of a hand or of the vocal cords under particular conditions and with particular consequences.

Sometimes the physiological and behaviorological aspects cannot be separated, such as when the behavior is covert. Muscles or glands may not even be involved. For example, due to having learned to observe and verbally report the occurrence of private responses (Skinner, 1953, Ch. 17), people may observe and report themselves seeing something, regardless of whether the thing seen is present to be seen or not; yet all that the properly instrumented physiologist observes about this *seeing* is neurons firing at the back of the brain. These neurons firing (physiological level) and the behavior of seeing (behaviorological level) are inseparable aspects of the same fact, the same phenomenon. Neither overt nor covert behavior can occur without nervous system activity; but the nervous system activity aspect may sometimes occur only along with a covert behavior aspect. Exactly what is happening physiologically when behavior (overt or covert) occurs and when related variables occur (the particular conditions and particular consequences) are important questions to which the natural science discipline of *physiology* can provide answers.

Is psychology's crossover to physiology an appropriate one? Psychologists who take this route show some preference for natural science, physiology in this case. However, psychologists may be the only ones viewing the crossover as a reasonable disciplinary activity. For the crossover interrupts physiology's mission and further compromises the status of psychologists' own discipline by shifting their subject matter into areas legitimately claimed already by a different

discipline. Psychologists may find academic turf battles with physiology far more common than with behaviorology.

As for eclecticism, observers can already note a decrease in its importance even in psychology. The shift in emphasis under the cognitive movement to stressing similarities (e.g., the transformation paradigm) seems to have prompted the decrease in eclecticism which is evident in the relative coverage of the psychological perspectives. Perusal of various psychology film series (e.g., the *Discovering Psychology* series) and any number of introductory psychology textbooks shows the cognitive perspective to be filling the stage. Next in coverage is information derived from another discipline, physiology. Other perspectives (e.g., psychoanalytic, humanistic, or gestalt) often receive little more than lip service. Unfortunately, this waning of eclecticism has not paralleled any commitment for changing toward an effective science concerned with why people do what they do and what can be done about it. (The Poling, et al., 1990, text is one exception in that it provides some natural science standards with which to compare and evaluate the several alternative perspectives it includes at appropriate points.)

The Historical Division

Psychology's increasing stress on similarities like the transformation paradigm had contributed substantially to the differentiation of the established disciplinary paradigms. That *differentiation*, in the 1960s, objectively created two separate and independent disciplines out of the previous shared history. The historical trunk divided into separate branches. These disciplines differ not only in paradigms but also, in associated ways, in subject matters, philosophies, methodologies, etc. Not until the 1980s, though, did the resulting changes in contingencies (the effects of incommensurability) begin affecting people enough for them to emit behavior consistent with the fact of different disciplines. Not until the 1980s did they begin to name and reorganize behaviorology.

How is Behaviorology Related to Other Disciplines and Fields?

The distinction between social science and *natural science* is relevant because the interest behaviorologists have in people is taken by some professionals as placing behaviorology in the social science arena. However, social sciences not only derive from an interest in people (an interest shared by many natural sciences), but from another commonly acknowledged characteristic as well; social scientists easily reach contradictory conclusions after following the same scientific procedures. This is partly because social scientists allow metaphysical events to enter their explanatory accounts. In contrast, natural scientists more easily reach consistent conclusions after following the same scientific procedures. This is partly because natural scientists disallow the inclusion of metaphysical events in their explanatory accounts, for such events are untestable. Natural sciences respect the continuity of events in space and time that accumulate in a researchable natural history. These are defining characteristics of natural science which behaviorology shares. [A later, more refined view has natural science opposing mysticism instead of opposing social science. See the *Afterword* for details and references.]

Among the natural sciences, behaviorology is one of the foundation life sciences (along with biology) rather than one of the foundation physical sciences (such as physics or chemistry). Figure 2 illustrates behaviorology's position along a life science continuum (see Fraley & Ledoux, 1997, about the term *culturology*).

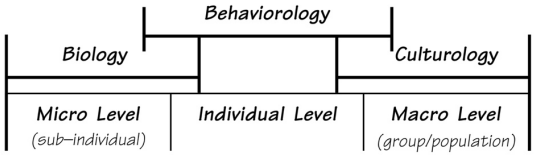

Figure 2. Disciplinary coverage for the three main levels of analysis in the life sciences.

(The study of ecosystems, species evolution, and the behavior of animals in groups by some animal biologists implies that a disciplinary overlap also exists between biology and culturology. So Figure 2 might be redrawn as a

triangle with extended sides that cross each other. Each side would represent one of these domains and its associated discipline. The areas where the lines cross would then represent the overlap in the interests of the intersecting disciplines.)

As a basic science, behaviorology provides the foundations that inform the considerations and technologies of various applied behavioral fields (e.g., organizational behavior management) as they seek to fulfill their respective cultural missions (a *field* is where one applies a foundation science *discipline*). Such fields range from advertising to zoology, with many currently in a scientific limbo without an appropriate basic science informing their efforts. Fraley (1987), in a paper addressing the cultural mission of behaviorology, stresses the role of behaviorology as the appropriate science to inform these areas (also see Fraley & Ledoux, 1997).

How Much is Encompassed by Behaviorology, Such as its Contributions?

Plenty, but thorough coverage goes beyond the bounds of this paper. The point of this paper was to introduce an analyzed history of the emergence of the discipline of behaviorology through the behaviorology movement. In the process the status and mission of behaviorology were introduced as well. One must still address the basic and advanced natural laws involving behavior as discovered by behaviorological scientists as well as behaviorology's philosophy of science, interpretations, and analyses, plus its technologies and applications. All of these constitute parts of behaviorology's past, current, and potential cultural contributions.

For instance, here are some basic components of the radical behaviorist philosophy of science; these components have value beyond the boundaries of behaviorology itself, and some have been mentioned already: (a) Radical behaviorists respect behavior as a natural phenomenon as part of respecting the continuity of events in space and time which accumulates as a natural history. (b) Radical behaviorists emphasize experimental control over dependent variables and the application of that control in culturally beneficial ways. (c) Radical behaviorists recognize private events, such as thinking or emotions, as covert behaviors involved in the same lawful relationships that involve overt behavior. (d) Radical behaviorists acknowledge that scientists are also behaving organisms whose behavior, scientific or not, is affected by the same variables that affect other behavior, and that those variables include scientists' philosophy of science. (See Ledoux, 1997a, for some elaboration; see Chiesa, 1994, for extensive discussion.)

The laws involving behavior essentially reflect the functional relations between behavior and the variables inherent in an organism's (a) species history, (b) personal history, (c) current situation and, for people, (d) cultural setting. These contain the variables a behaviorologist addresses when trying to analyze, understand, predict, control, and interpret the behavior of organisms. A peek at some of the advances in researching and applying these laws (advances arising since the paradigm differentiation in the 1960s) would involve describing numerous topics: (a) the distinction between event–shaped and verbally–mediated behavior (Vargas, 1988), (b) the analysis of verbal behavior (Skinner, 1957), (c) the recombination of repertoires (Epstein, 1981), (d) establishing operations (Michael, 1982), (e) multi–term (n–term) contingencies (Sidman, 1986a, 1986b), (f) the function–altering effects of contingency–specifying stimuli (Schlinger & Blakely, 1987), (g) stimulus equivalence relations (Sidman, 1994; Stromer, 1991), (h) the general level of reinforcement (Cautela, 1994), and (i) behavioral engineering and cultural design (Skinner, 1971; Ulman, 1991; West & Hamerlynck, 1992). These topics highlight some of the state–of–the–art aspects, in the 1990s, of the scientific comprehension and handling of complex human behavioral relations.

Other cultural contributions involve continuously developing and extensively tested behavioral engineering technologies applicable to all facets of life, with particular value in resolving both personal and cultural concerns. These range from preventative measures in child–rearing practices, to making education effective (e.g., Johnson & Layng, 1992) including the critique of developmentalism, to enhancing business,

industrial, and organizational management, to the design and redesign of cultures and cultural practices including those related to rescuing the planetary environment and so helping restore the mutually beneficial balance between the Earth and its inhabitants (see Gore, 1993; also see the bibliography at the end of Ledoux, 1997e, for references to other examples, as well as to works covering more of the depth and range of the behaviorology discipline and its cultural utility). Either a general–behaviorology textbook (e.g., Fraley, 1996) or issues of the *Journal of Applied Behavior Analysis* can provide a starting point for reviewing the research on many of these applications.

Why Should Anyone Learn Anything About Behaviorology?

The laws of behavior (that is, the relations described by those laws) do not always produce benefits; at least as often as not, they may produce problems. For instance, many families fall victim to the accidental, unplanned conditioning of various undesired behaviors. Without contact with behaviorological science, parents may never realize that yelling at or even spanking a child may actually strengthen the behavior they are trying to weaken, especially if that is the only or main kind of attention the child receives. Parents may never realize that the general rule to provide appropriate kinds of attention more when children are behaving in the ways parents desire (i.e., to "catch your children being good") is both more effective than just ignoring them, and more effective than just catching and punishing them when they are bad. "Catching them being good" is more effective in increasing desired behavior and thereby reducing the occasions for undesired behavior. (See Christophersen, 1988, for details on this and other behaviorologically based, prevention oriented child–rearing practices.)

As that example shows, the accidental or unplanned operation of behavioral laws having undesirable effects on behavior becomes a tyrant affecting people's lives. Until after behaviorological research began in the 1930s, few could do much to stop that tyranny for those laws were little understood. Today, through behaviorology, people can increasingly replace that tyranny by designing and redesigning the world in which they live. They can take the responsibility to use the ongoing discoveries about those laws to improve the human condition (and even to evaluate scientifically what words like "improve" mean; see Krapfl & Vargas, 1977; Vargas, 1975, 1982). These actions are possible because one of the behaviors generated and maintained by the operation of these laws is the behavior of people in general taking control of themselves, and the environmental variables that affect them, in informed ways (which also enables them to countercontrol for potential misuses of this science). The greatest initial significance of behaviorology may not be in the management of day–to–day individual affairs nor, perhaps, even in providing solutions to large social problems (e.g., the crisis in American education). Instead the greatest significance may be in providing some critically needed tools to help understand and deal with the world–wide environmental and outer space concerns and crises facing the generations of today and tomorrow.

The basic reason to study behaviorology, then, is to reduce the risks and derive more than the minimal, automatic benefits from the way nature's laws govern behavior. Studying behaviorology expands your repertoire of behavior with respect to those laws and their applications. The more extensive your training in behaviorology is, the greater can be your effectiveness, your success, with its applications to human concerns.

How much behaviorological knowledge and skills is right for you? Everyone should be as familiar with the basics of behaviorology as they are with the basics of biology and physics and other standard natural sciences covered through primary and secondary education. Beyond the basics, "How much is right?" depends on the complexity of the applications appropriate to your areas of concern. The more complex the applications are in a particular area, the greater is the amount of behaviorology study needed if you are to be effective in that area. For instance, look at some of the areas involving children, for these are typical of the complexity levels of most areas of human concern. While a lack of study leaves

anyone's potential for success to accident or chance, parents can, by design, attain a quite reasonable level of informed effectiveness in child–rearing practices with only a basic amount of behaviorology study (the equivalent of one or two courses in behaviorology; see Ledoux, 1997b). Educators, on the other hand, find that teaching effectively requires substantially more study. And working with autistic children requires even more. In each of the two latter cases, the complexity of both the applications and the relevant controlling variables increases (while gaining access to those variables is often more difficult as well). Such circumstances demand a more professional level of training (starting with a Bachelor's degree in behaviorology) if practitioners are to be as effective as possible in areas such as these.

A Matter of Epistemology Also

The "Why study behaviorology?" question need not be answered only with respect to complexity and effectiveness. Consider also this answer: "We should study behaviorology because we are affected by nature's laws anyway; perhaps the more we know concerning these laws, the better off we will be." But what does "know" mean? Let us take a little trek into a scientific *epistemology* (the question of what *knowing* is) to help understand this answer better, including how it relates to the complexity and effectiveness answer.

Nature's laws, the laws of the universe, affect us at different *levels of knowledge*. They affect us (a) whether we like it this way or not, (b) whether we have used those laws or not, (c) whether we "know about" those laws or not (as in "can use the laws effectively," that is, whether our use–skills have come effectively under the control of those laws or not), and (d) whether we "know" those laws or not (as in "can state and use them," that is, whether our talk, or better, our talk and our use–skills, have both come effectively and explicitly under the control of *statements* of the relationships inherent in those laws or not). In these levels, *knowledge* refers to the range and depth of our behavior repertoires. To illustrate these levels of knowledge (repertoire), consider an example from the teaching profession.

Persons untrained in chemistry but trained in English literature (and even trained in teaching literature as well) would be quite out of place conducting a chemistry class. They do not know the names or properties of the chemicals under discussion in that class (i.e., their behavior repertoire does not include responses appropriate to the pertinent variables, such as the discriminative stimuli and consequences, present in that situation). Yet if they mix some of the chemicals before them, they will produce the same chemical reactions that their trained chemistry colleagues would have produced had those colleagues mixed those same chemicals. The laws of chemistry are in force whether they like it or not, *and* whether they use them (by mixing some of the chemicals before them) or not.

What about the trained chemistry colleagues? Are they trained only in chemistry or are they also trained in teaching, and does that make any difference, especially to teaching? After accumulating, usually over years, certain kinds of experiences (which typically occur by accident or chance), the chemistry teachers who are trained only in chemistry come to *know about* teaching. At least, you would say they do so to the extent that they come to be effective in teaching, that is, in expanding the chemistry–related repertoires of their students. Their teaching–related use–skills have come effectively under the control of the laws involved in successful teaching. Yet even after decades of teaching, they are unlikely to be able to describe, in terms of nature's complex laws relating to the expansion of repertoires (i.e., the scientific foundations of teaching and learning), the reasons for their effectiveness. They cannot tell another *chemistry–trained* person how to *teach* chemistry effectively. (They can, of course, make up theories about their successes; but that is a different matter entirely, and something to be avoided if teaching in general is to become effective.) While they can effectively teach chemistry, and can model doing so, they cannot effectively teach teaching, even of chemistry. They do not *know* teaching as they know chemistry.

Effectively expanding the repertoires of students through knowing teaching (an instructional design repertoire), and effectively

handling chemicals (a subject–matter repertoire), are very different behavior repertoires. The latter is informed by chemistry while the former is informed by behaviorology. If the would–be teachers of chemistry or any subject matter want to become effective teachers, want to know teaching, want a comprehensive repertoire of teaching skills, and want it by design in a shorter time rather than by chance over a longer time, then they must study the discipline that informs teaching. To the extent that they learn both to teach a particular subject matter (i.e., to expand their students' repertoires in that subject matter) and to accurately and scientifically explain what it is that they are doing which results in that expansion of repertoires, to that extent you would say not only that they *know* their subject matter but also that they *know* teaching as well. Their talk and use–skills have both come effectively and explicitly under the control of *statements* of the relationships described by the laws of behavior relevant to teaching. (You might even say they *know* teaching even if only their *talk* has clearly come under that control.) And teaching is but one example of the many human endeavors where levels of knowledge/repertoire relate to effectiveness.

Vargas and Fraley (1976; also see Vargas, 1996) discuss some benefits of separating these two major repertoires in education, the repertoires of subject–matter expertise and instructional design expertise. These repertoires might be too complex to expect most individuals to expend the effort to master both thoroughly. However, two experts, each with mastery of one of these repertoires, can combine their efforts and thereby achieve greater overall educational effectiveness. Indeed, one design expert can combine efforts with a dozen or more content experts to achieve such improvements. The subject–matter experts can concentrate on the subject–matter content of the courses or programs of study while the instructional design expert concentrates on the instructional arrangements to teach those contents with scientifically sound methods.

The point Vargas and Fraley make is significant because the usual emphasis on subject–matter expertise generally leads to ignoring the complementary need for instructional design expertise. People presume, incorrectly, that someone who is a subject–matter expert automatically has a thorough enough repertoire appropriate for *teaching* that subject. Yet usually the teaching repertoire is minimal. That is especially common in post–secondary education. However, as our example with teaching chemistry showed, *early* and long–sustained effectiveness requires training in both expert repertoires. Vargas and Fraley refocus attention on the need for employing a scientifically based instructional design repertoire if educational effectiveness is to improve. The question is, will effectiveness arise by chance in the slow, lucky accumulation of the necessary experiences, or will it accrue by design through training and practice, especially in the so–far neglected arena of instructional design? Vargas and Vargas (1992) extend the discussion to current instructional materials and programming.

Our trek into epistemology helps put into perspective the differences between *just being affected* by nature's laws (whether we like them or not, or use them or not), *knowing about* them, and *knowing* them, as these relate to increasing complexity requiring more comprehensive study for effectiveness. This applies especially to behaviorology, as the science of behavior relations, since so many areas of interest involve human behavior. The more extensive your training in behaviorology, the more effective you can be in dealing with behavior in the contexts of concern to you. (For reasons of this sort, TIBA included among its purposes support for a basic "behavior literacy" graduation requirement of appropriate content and depth at all levels of education; at the college level, that would likely involve a couple of courses. See Fraley & Ledoux, 1997, and Ledoux, 1997b; also see the Addendum to the appendices in Ledoux, 1997e/2002.)

A Matter of Hygiene Also

Yet another answer to the "Why study behaviorology?" question is available. With so many human problems (and potential threats to survival itself), the importance of learning and applying behaviorology today is akin to the importance of learning and applying the then new

discoveries of biological science about 150 years ago. The discovery of the relation between micro–organisms and disease formed the basis, in the first half of the 1800s, of today's standards for biological hygiene. (Biological hygiene is that vital ounce–of–prevention whose success, in reducing the frequency of disease, we take for granted today, so many years after the discovery of the relevant scientific facts.)

Learning and applying behaviorology could be called a matter of behavioral hygiene, the next step, especially in problem prevention, after having successfully developed and adopted biological hygiene. We would not consider risking hepatitis by eating without first cleaning our hands after using the toilet. Why should we then continue to risk, for example, low success rates (relative to potential) in education when we can, *if appropriately trained*, bring about consistently demonstrated high success rates in both deportment/emotional and academic/intellectual areas? (See Johnson & Layng, 1992; Latham, 1997; Skinner, 1968; and West & Hamerlynck, 1992.) Why should we take those risks when we can, instead, clean up our *actions* by applying some behavioral hygiene? How many years will pass before we achieve *today's* potential successes and take behavioral hygiene for granted? The sooner we become more informed by this science, throughout society, the less time it will take.

Let us take the responsibility to learn and apply behaviorology's discoveries about the laws of behavior at least for the sake of behavioral hygiene. Let us do so to improve the human condition, to take control of our interactions with our environments, and to master control of ourselves.

Endnotes

From the original address prepared for Chinese and other audiences, this paper was revised for publication (1992) in *Behaviorological Commentarie*s, Serial No. 3, pp. 11–31. Subsequently, it was the first—and principal—part of a presentation at the second Behavior Analysis Around the World Conference, held in 1992 at Keio University in Tokyo, Japan (the other part of that presentation coming from Ledoux, 1997c). Before receiving further minor revisions for inclusion in *Origins and Components of Behaviorology* (Ledoux, 1997e) it received minor revisions for inclusion in the 1992 edition of this book of readings. In any case this paper presents only a starting point for further, more in–depth examinations of behaviorology to be found in other behaviorological–science resources (such as Fraley & Ledoux, 1997).

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An Introduction to the Philosophy Called Radical Behaviorism

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The work of B.F. Skinner simply did not follow the majority views of his time. In this he was not the first. He stood, as the saying goes, on the shoulders of giants. He advanced another major step in a trend whose continuity in the West began nearly 500 years ago. That trend is one of replacing what could be characterized as humanity's self–centeredness with an increasingly more effective natural science perspective about people's place in the order of things. This trend got a big push when Copernicus reiterated what Aristarchus of Samos and the ancient Ionian Greeks had discovered much earlier but which had been lost in the intervening centuries: the Earth, and thus humanity, were not the center of everything. Later, Darwin showed that our bodies (our physical forms, structures, and functions) are also products of the same natural laws that apply to all other living and non–living things. Then Skinner, through the behaviorology discipline arising from his work, demonstrated that our very being, our consciousness, our conduct, our behavior, is also necessarily and properly within the reach of natural science. From that demonstration, and its associated applied technologies, arises an increased opportunity for humanity to solve its problems: from day–to–day personal difficulties, through challenges such as the crisis in education, to the global problems threatening survival itself. To benefit from that opportunity, people must expand their behavior repertoires with respect to behaviorology, the discipline responsible for the relevant science and technology. An appropriate starting point is the *philosophy* of science that informs that discipline. This paper introduces that philosophy.

Skinner gave the name radical behaviorism to the philosophy of science under which he operated. That philosophy now informs the behaviorology discipline, a continuing extension of Skinner's work. The term, *radical*, in radical behaviorism, means thoroughgoing or fundamental (Ulman, 1991). Radical behaviorists use this term to distinguish this form of behaviorism from other forms of behaviorism such as Watson's original behaviorism (Watson, 1913), methodological behaviorism, interbehaviorism, or paradigmatic behaviorism (Ulman, 1992a). The distinction is necessary because the criticisms commonly leveled at behaviorism are not applicable to all forms. Those criticisms have become appropriate only for the other forms of behaviorism because radical behaviorism developed partly as a corrective response to some legitimate concerns raised in the criticisms. Skinner provides a comprehensive discussion of these issues in his 1974 book *About Behaviorism*.

Simply as a name originating in an historical context, the name radical behaviorism has little problem itself as an acceptable name for the philosophical position informing the behavior science Skinner started, at least to practitioners of that science. However, some authors have expressed legitimate concerns over misunderstandings caused outside that science and historical context by the terms used in this name. Schneider and Morris (1987) try to reduce the misunderstandings by providing a thorough history of the use and evolution of the terms radical and behaviorism. Meanwhile Vargas (1990) argues for avoiding the misunderstandings by using a different term, such as *selectionism*, to replace the older terms in naming this philosophy of science. Ulman (1992b), while not insisting that a change was unnecessary, questioned selectionism as a good choice for an appropriate name. He pointed out a particular problem with selectionism: one can be a selectionist without holding to a radical behaviorist philosophy (for example, Hegel or Tielhard de Chardin). Since no alternative name is as yet generally accepted, this paper continues to use the name radical behaviorism.

[†] This paper was originally prepared as a basis for discussions in a graduate *Behaviorology and Education* class at the Xi'an Foreign Languages University, China.

This philosophy of science, radical behaviorism, has many components. These components are at the core of the science and movement that became the natural science behaviorology discipline (several applicable components are shared by other natural sciences). As radical behaviorists and natural scientists, behaviorologists respect these components. Four of these components arise regularly in discussions of radical behaviorism and were especially important in the emergence of behaviorology. Rather than saying "radical behaviorism does this or that," certain behaviors of radical behaviorists represent these components: (a) Radical behaviorists respect behavior as a natural phenomenon as part of respecting the continuity of events in space and time which, in natural sciences, accumulates as a natural history. (b) Radical behaviorists emphasize experimental control over dependent variables and the application of that control in culturally beneficial ways. (c) Radical behaviorists recognize private events, such as thinking or emotions, as covert behaviors involved in the same lawful relationships that involve overt behavior. (d) Radical behaviorists acknowledge that scientists are also behaving organisms whose behavior, scientific or not, is affected by the same variables that affect other people's behavior, and that those variables include scientists' philosophy of science. Other concerns are inseparably intertwined with these components of radical behaviorism. Some of these concerns include the preference for single–subject experimental designs rather than group statistical designs, the refusal to allow metaphysical events to enter explanatory accounts, and the question of parsimony in accounts of human behavior (see Chiesa, 1994).

Any and all of these components might be covered in discussions of radical behaviorism. Some authors take (a) and (b) for granted and mainly cover (c) and (d) in descriptions of this philosophy (e.g., Hake, 1982). Similarly, this brief introduction concentrates on (c) and (d), only mentioning (a) and (b) in passing. But this is partly because (a) and (b), and their implications, are extensively covered in a paper by Fraley and Ledoux (1997; also, see Ledoux, 1997a).

Private Events: Covert Behaviors

Radical behaviorism has been misunderstood and misrepresented concerning private events, their evaluation, and their place in a science of behavior (e.g., Mahoney, 1989). Radical behaviorists do not deny that such events occur inside the skin. They ungrudgingly accept the reality of the physiological events occurring within the body, some as behavior. They take private events into account. But in so doing, they also insist that, in any serious scientific endeavor, private events be considered in ways respectful of the natural science continuity of events that accumulates as a natural history. That is, they insist that private events be considered without appeal to metaphysical causality or metaphysical implications. Since this precludes mentalistic and cognitive explanations, those who court such explanations resist radical behaviorism. Skinner (1974) addressed this issue in *About Behaviorism*:

> But if a behavioristic interpretation...is not all we should like to have, it must be remembered that mental or cognitive explanations are not explanations at all. (p. 106)

Adherents of radical behaviorism assume that the same natural laws prevail on both sides of the skin. This, of course, does not change the nature of either the person, the events inside the skin, the events' effects, or the events' independent variables. The skin is not any special sort of boundary to the laws of the universe. Furthermore, radical behaviorists recognize that a person may at times be the *only* observer in a position to detect or discriminate the occurrence of certain events within his or her skin (words like "detect" and "discriminate" need not imply agency; see Baum, 1995). So radical behaviorists invest scientific consideration also in events detectable by only one person. They do not restrict scientific consideration to events detectable only by more than one person. And they are willing to work with the resulting increase in technology required to manage the greater inaccessibility of such events.

Radical behaviorists find that the most effective way to handle private events is to

recognize them as covert behaviors under the same laws affecting overt, public behaviors. Private events are lawful in the same way that one would regard public events. Radical behaviorists cannot grant scientific status to private events invented to be causes of behavior. Nor do they use real private events as primary causes of behavior. They do not need to, because they analytically pursue any causal chain to other, outside events. They do this for the sake of control in their subject matter. They do not treat private behavioral events as indicators of internal hypothetical constructs conjured up, or conveniently given just the right characteristics, to explain those events.

Instead, radical behaviorists see behavior, on the overt level, as neurologically based actions of the glands and muscles (both smooth and striped). They see private events as covert behaviors, under the same laws as overt behaviors. These covert behaviors are usually less accessible than overt behaviors, often being observable and reportable only by a public–of–one (see Ledoux, 1973). And sometimes these covert behaviors involve only the neurological–level events; the behavior of "seeing in the absence of the thing seen" is one example (see Ledoux, 1997a; also see Skinner, 1953, Ch. 17).

The Behavior and Philosophy of Scientists

When considering the behavior and philosophy of scientists, perhaps radical behaviorism has been more overlooked than misunderstood, as well as confused with other behaviorisms. That is unfortunate, because the practice of science itself, and philosophy of science, are both effectively addressed by the principles of radical behaviorism.

The Behavior of Scientists

The work of scientists is twofold. It is (a) to be exposed to precise and controlled contingencies that are unlikely to have affected others in this controlled way. It is also (b) to pass along descriptions and applications of those contingencies to others.

Scientific work is initially the behavior of the scientist under direct control of the contingency relations (that is, under the direct control of the complex of multiple stimuli, behaviors, and consequences) experienced in research. This is the point of science. After extensive study and preparation, scientists are exposed to the contingencies of the unknowns in their disciplines. Due to their study and preparation, they derive the maximum benefit from that exposure. And that is what doing science is all about.

The rare and precise arrangement of contingencies experienced in research generally limits the availability of these particular contingencies to scientific contexts. Hence only people operating in those contexts have their behaviors effectively shaped by those contingencies. The subsequent steps a scientist takes are largely determined by the consequences of the previous steps. The result is a unique, contingency–shaped expansion of the scientist's scientific behavior repertoire. Due to this expansion, scientists can behave more effectively with respect to the subject under their study than others who lack that exposure to those contingencies.

The disciplinary behaviors of scientists also include the behavior repertoires of summarizing, reporting, and applying their expanded scientific repertoires. These disciplinary repertoires involve verbal stimuli. And these verbal stimuli provide *rules*. The rules are statements of the contingencies the scientists have experienced. These rules affect the behaviors of others. Colleagues, disciplines, fields, and the public benefit from using these rules because when their behavior is affected by these rules (that is, when their behavior comes to be rule–governed—i.e., verbally mediated) their behavior often becomes more effective than it would be without the rules. As a result the rules become responsible for much of the behavior of these groups. In essence, such benefits accrue by expanding the repertoires of those people without each of them having to await the unlikely experience of the research contingencies themselves. In this way they benefit from scientists' work. While much of scientists' scientific behavior is contingency–shaped in vital ways, the behaviors of these other groups is to a large extent rule–governed (see Skinner, 1969, about this distinction).

Vargas (1988) has recently recast the distinction between contingency–shaped and rule–governed behavior as the distinction between event–governed and verbally–governed (or mediated) behavior. This distinction and its implications, and several other advances, provide the current state of the art for the scientific comprehension and handling of complex human behaviors. Some of the other advances include (a) the analysis of verbal behavior (Skinner, 1957), (b) recombined repertoires (Epstein, 1981), (c) establishing operations (Michael, 1982), (d) multi–term (n–term) contingencies (Sidman, 1986a, 1986b), (e) the function–altering effects of contingency–specifying stimuli (Schlinger & Blakely, 1987), (f) stimulus equivalence relations (Sidman, 1994; Sidman, Wynne, Maguire, & Barnes, 1989; Stromer, 1991), (g) the general level of reinforcement (Cautela, 1994), and (h) behavioral engineering and cultural design (Skinner, 1971; Ulman, 1991; West & Hamerlynck, 1992). Indeed, the radical behaviorist and behaviorological perspectives encompass a far wider domain than that denoted traditionally as "respondent and operant conditioning in the learning of new behavior."

The Philosophy of Scientists

Scientists, like everyone else (including radical behaviorists), are behaving organisms whose behaviors, scientific or not, are affected by the same laws that affect other behaviors. Those laws essentially reflect the functional relations between behavior and the variables inherent in an organism's (a) species history (e.g., genetics), (b) personal history, (c) current situation and, for people, (d) cultural setting. These contain the variables which a behaviorologist addresses when trying to analyze, understand, predict, control, and interpret the behavior of organisms.

A scientist's philosophy of science is itself among the variables affecting his or her work. The philosophical repertoire derives partly from the history and setting variables. This repertoire later affects the scientist's work as a part of those variables. The philosophical repertoire includes various underlying assumptions. Comprised mostly of verbal behaviors, a discipline's philosophy of science is usually learned at advanced stages in disciplinary training, although precursors are present long before that (parts of the personal history variables). This repertoire is behavior, and as such continues to be subject to the laws of behavior. But, through the scientist's colleagues and discipline which share it, the philosophy itself becomes one of the variables affecting the scientist's subsequent work (part of the cultural setting variables).

A scientist's philosophy of science affects her or his work in several ways. One way involves the philosophy evoking investigations of certain variables and not others. Cooper, Heron, and Heward (1987, p. 12) provide some examples:

> ...the philosophical decisions to ignore all private events or to use explanatory fictions as the causes of behavior may both produce a similar effect on research and practice. Both positions restrict practice and research even though for different reasons. Methodological behaviorism is restrictive because it ignores areas of major importance for an understanding of behavior. Mentalistic positions are also restrictive, for as noted by Skinner (1974), "Mentalistic explanations allay curiosity and bring inquiry to a stop. It is so easy to observe feelings and states of mind at a time and in a place which make them seem like causes that we are not inclined to inquire further."

A philosophy of science can also affect a scientist's work by playing a role in the conditioning of a scientist to be reinforced by certain classes of events and not others. Hake (1982, p. 24) provides some examples:

> The issue here is what the radical behaviorist believes the reinforcement contingencies for the scientist should be. The most common view and that of the methodological behaviorist is that inclusion of a finding in the body of knowledge or theory is based on acceptability to the scientific community in the terms of (1) the research procedures used (e.g., agreement among observers, replicable individual data, precise measurement and control) and (2) the relation of the content to the existing theory (e.g., related to a productive content area but an extension of it). The radical behaviorist would

> not believe those contingencies alone to be totally desirable, because they include insufficient reinforcement for innovative content and procedures, and thereby delimit the growth of science... The radical behaviorist would suggest workability, stimulation, and contribution to society as additional worthwhile contingencies that would encourage innovation of content and method. The major contention is that scientists should recognize that all aspects of their scientific behavior are shaped by the reinforcers of some scientific community and that this control of their behavior affects the science.

Conclusion

The philosophy of science called radical behaviorism played a fundamental role in B.F. Skinner's determination that our very being, consciousness, conduct, and behavior is necessarily and properly within the reach of natural science. (Regarding these concerns radical behaviorists have addressed relevant aspects as far–afield as ethics and religion; see Krapfl & Vargas, 1977; Schoenfeld, 1993; and Vargas, 1975, 1982.) The result has been increased opportunities for humanity to solve its problems through the science informed by that philosophy, namely, behaviorology.

A question that often arises in discussions of philosophy of science concerns how the radical behaviorist philosophy differs with the philosophies of science in other disciplines, most notably psychology. That question was not covered in this introductory paper. Extensive coverage can be found, for example, at appropriate points in a paper by Fraley and Ledoux (1997) which weaves its comprehensive way through the origins, status, and mission of behaviorology. However, comprehensive coverage of the radical behaviorist philosophy of science is beyond the scope of either that paper or this one. (For comprehensive coverage, see Skinner, 1953, 1974. For more recent comprehensive coverage, see Chiesa, 1994.)

Endnotes

The author thanks the members of the graduate *Behaviorology and Education* class in Xi'an, China (see Ledoux, 1997b), for whom this paper was originally prepared, for their discussions which provided a good foundation for subsequent improvements. The author uses this material to help students and colleagues begin to understand the philosophical basis of behaviorology in a way that encourages them to seek details from more extensive sources (e.g., Skinner, 1953, 1974; Chiesa, 1994). The paper was revised for publication (1992) in *Behaviorological Commentaries, Serial No. 4*, pp. 3–10. Then, before receiving further minor revisions for inclusion in *Origins and Components of Behaviorology* (Ledoux, S.F. [1997]. Canton, NY: ABCs.), it received minor revisions for inclusion in the 1992 edition of this book of readings.

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Formal Definitions of "Primary" and "Secondary" Reinforcers Promote More Efficient Animal Training

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Abstract

There is widespread misunderstanding regarding primary versus secondary reinforcers within the animal training community. In this article, I will clarify the definitions: primary reinforcers being "independent of their correlation with other reinforcers," and secondary reinforcers as "initially neutral and dependent on their association with other reinforcers." Secondary reinforcers lose their effectiveness if that correlation, or pairing, is discontinued. By returning to these formal scientific definitions, secondary reinforcers used by animal trainers would be essentially limited to clickers, whistles, or other marker signals. For trainers currently operating under alternative interpretations of this definition, returning to well-established formal definitions would lead to more efficient training in terms of speed and persistence of conditioning, a larger selection of reinforcers in different situations, avoiding prospective satiation or extinction of available reinforcers, and a better understanding of potential distractors as well as unwanted behavior.

There are different schools of thought within animal training communities with respect to what constitutes primary and secondary reinforcers. Several training terms have multiple—and divergent—interpretations in various training circles, and many training procedures are carried out in different ways by different trainers. This variation likely reflects the expansion of the animal training industry in the last 70 years, and the diversity in educational backgrounds and theoretical orientations. Practical training techniques have evolved in the dog training community, zoos, and aquaria, sometimes with little support from basic science, resulting in a variety of definitions and practices. The art of training has evolved under a variety of contingencies.

When defining primary and secondary reinforcers, some contemporary animal trainers have strayed from well-established formal scientific definitions. While the deviation may be considered minor, the resulting ramifications may potentially have a major impact on training efficacy, and the prevention and resolution of problem behavior. In this article, I elucidate the various ways that this deviation could be problematic in animal training. To facilitate the discussion below, I term trainers adhering to the well-established scientific formal definition (see Chance, 1998) "formal trainers," and trainers advocating the deviation "alternative trainers."

Formal Definition and Alternative Interpretation

Stimuli that will effectively reinforce behaviors when presented contingent upon the behavior, and for which no previous conditioning history exists, are called unconditioned positive reinforcers, unconditioned added reinforcers, or primary reinforcers. They are not dependent on a correlation, or pairing, with other established reinforcers. There are a large number of stimuli that can potentially function as primary reinforcers (table 1), at least for some animals some of the time.

Table 1. Potential primary positive reinforcers according to the formal definition: resources or stimuli that some organisms are innately willing to work for to gain access to without prior conditioning to other reinforcers. Species

differences, individual preferences and current motivational states will determine whether or not a specific stimulus is a functional reinforcer at any particular time (Laraway et al., 2003; Schneider, 2012).

Type of primary reinforcer	Species example	Reference
Food	Rats	Skinner (1938)
Drink	Pigeons	Jenkins & Moore (1973)
Play opportunities	Raccoons	Davis (1984)
Companionship / petting	Dogs	Feuerbacher & Wynne (2015)
Attention / approval	Humans	Gewitz & Baer (1958)
Reproductive opportunities	Rats	Everitt et al. (1987)
Aggressive opportunities	Fish	Hogan (1967)
Sensory stimulation (auditory, visual, olfactory, tactile, gustatory)	Sparrows, mice, dogs, etc.	Schneider (2012)
Shelter	Pythons	Stone et al. (2000)
Favorite locations and activities	Primates	Premack (1959)
Control	Humans	Finkelstein & Ramey (1977)
Variety	Primates	Hollerman & Schultz (1998)
Learning	Humans	Biederman & Vessel (2006)
Brain stimulation	Rats	Pliskoff et al. (1965)
Drugs (e.g., cocaine)	Mice	George et al. (1991)

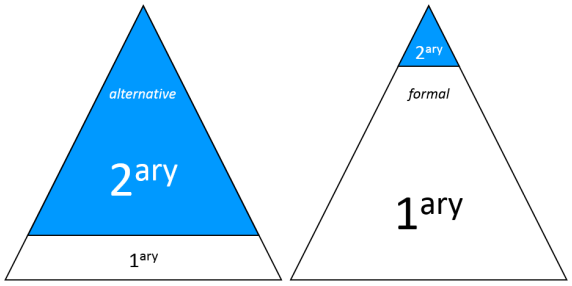
Secondary reinforcers, in turn, are "dependent on their association with other reinforcers" (See e.g., Chance, 1998). Such stimuli are initially neutral with respect to the response in question, and become conditioned when paired with unconditioned stimuli or already established conditioned stimuli (c.f. Holland, 1992). Thus, secondary reinforcers take on the reinforcing properties of the primary reinforcer with which they were paired (Feng et al., 2016), and, importantly, lose their reinforcing properties if at least occasional pairing with the unconditioned stimuli is discontinued. In contemporary animal training, secondary reinforcers might be the sound of a clicker or a whistle, or moving one's hand towards a food pouch.

However, some animal trainers, particularly those within the marine mammal training community, misinterpret the distinction between primary and secondary reinforcers. To the best of my understanding, alternative trainers typically misconstrue primary reinforcers as only those stimuli argued to be essential for basic survival. This opens up for some subjective interpretation; some alternative dolphin trainers consider a fish a primary reinforcer, but not consumables other than fish. Other alternative trainers will consider all types of food primary reinforcers but not play, gentle touch or praise.

In other words, rather than classifying stimuli as secondary reinforcers based on whether they were previously neutral, if they take on the reinforcing properties of the stimulus with which they were paired, or lose their effectiveness if the pairing is discontinued, alternative trainers seemingly classify secondary reinforcers based on whether they are subjectively deemed to be essential for survival or not. They then establish stimuli judged to be non-essential as secondary reinforcers by an explicit pairing procedure (e.g. play-treat, touch-treat, or praise-treat).

Thus, interpretation of the terminology will have a large effect on the relative distribution of "primary" and "secondary" reinforcers (Figure 1).

Figure 1. The relative distribution of potential primary and secondary reinforcers for alternative and formal trainers. Alternative trainers only consider stimuli essential for survival to be primary reinforcers; most other stimuli are conditioned and then regarded as secondary reinforcers, regardless of whether they were initially neutral or not (typically toys, petting, praise etc.). Formal trainers consider all unconditioned stimuli that may effectively reinforce behaviour primary reinforcers (Table 1). Secondary reinforcers are stimuli which were previously neutral and have been conditioned to predict the delivery of a primary reinforcer (e.g., the sound of a clicker).



Secondary reinforcers have two uses in contemporary animal training. First, they may be used as Keep-Going Signals (KGSs) during a behavior requiring duration (Pryor, 1999), such as remaining immobile during the time required to take a blood sample. As such, KGSs signal that the ongoing behavior is correct and will eventually lead to primary reinforcement.

Second, secondary reinforcers are used as "event markers" (Feng et al., 2016). Typically, a clicker or a whistle will be sounded to pinpoint a criterion or target behavior, or terminate a long-duration behavior, and will typically be immediately followed by a treat or some other reinforcer.

In the case of their use as event markers, secondary reinforcers develop properties both as a reinforcer, as well as a discriminative stimulus. This dual nature of the secondary reinforcer will likely influence training outcome, as explained below (Figure 2). Indeed, some of the early experiments found that after an animal had been trained to exhibit a response following the onset of a stimulus (illustrating discriminated responding), that stimulus could be used to strengthen other responses preceding its onset (illustrating a reinforcing effect)(Wyckoff, 1959). In the early days, a lot of effort went into investigating how the secondary reinforcing effects of a stimulus were related to the strength of that stimulus as a cue (e.g., Schoenfeld et al., 1950). A key feature of secondary reinforcement, discussed in the scientific community many years ago, is this dual nature of predicting the availability of primary reinforcers, (an antecedent effect) as well as reinforcing preceding operant responses (a postcedent effect)(summarized in e.g., Wyckoff, 1959). It seems that this discussion has waned from the scientific community, but was raised at a recent international training conference (Bartlett, 2017).

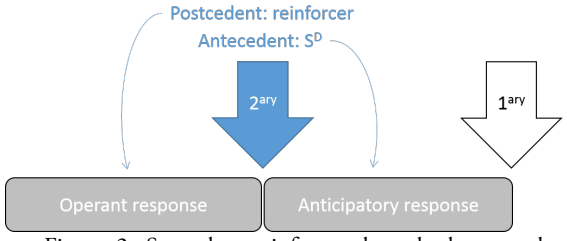


Figure 2. Secondary reinforcers have both postcedent properties, reinforcing the preceding operant response class, as well as antecedent properties, being a discriminative stimulus for behaviours yet to occur. Anticipatory responses, covert or overt, occurring in the interval between the secondary reinforcer and the delivery of the primary reinforcer, will likely influence training outcome.

As will be discussed below, it seems that many alternative animal trainers focus primarily on the reinforcing properties of their secondary reinforcers, and overlook the antecedent facet. This may be, in part, because they've deviated from the original formal definition. In keeping with this alternative interpretation, they condition certain stimuli and call them "secondary reinforcers"—regardless of whether conditioning is required. Thus, they don't seem to consider that these stimuli may also have innately reinforcing properties.

Let's take the example of using playing as a reinforcer for an animal. For the formal trainer, playing can be used as a primary reinforcer from the outset, provided that the trainer is familiar with the types of play opportunities that are reinforcing to that particular animal. For the alternative trainer, playing is typically conditioned before being used during formal training: play-treat, play-treat, play-treat.

Object play and social play have been shown to reliably reinforce behavior without this explicit pairing procedure (indeed in one experiment, a raccoon no longer reliably accepted food as a reinforcer) (Davis, 1984). Thus, there are two categories of questions to be asked with regards to the alternative conditioning procedure. What happens to "play" as it acquires properties of a conditioned reinforcer in addition to already having properties as a primary reinforcer? Secondly, how does the alternative trainer use "play" as a reinforcer henceforth?

Below I will explain why I believe that adhering to formal definitions would help trainers explain and control behavior more efficiently, and communicate more effectively with other trainers; I expect inexperienced formal trainers will be more efficient in their training than novice alternative trainers.

Outcomes of using alternative versus formal approaches.

Relationships and Reinforcers

The main reason why many alternative animal trainers go through the procedure of actively pairing, for example, tactile reinforcers with food, is to ensure that the animal will accept touch as a reinforcer by that person. Some common physical human–animal interactions may be aversive for the animal, at least if initiated by a stranger, as shown in a study on dogs by Kuhne and colleagues (2014). Indeed, many animals will not accept touch from an unknown person, and may show fearful or aggressive behavior. The formal trainer thus risks overestimating the likelihood that a particular stimulus, such as touch, is a functional primary reinforcer when delivered from that person to that animal.

It is likely that the risk of the animal not accepting a potential primary reinforcer would be increased if there are interfering competing contingencies, for instance fear of novelty or unknown persons. Once the initial fearful response has subsided, chances are increased that the no longer novel stimulus would positively reinforce behavior. The conditioning of alternative secondary reinforcers, such as touch, can thus be construed as a counter-conditioning procedure geared to prevent or eliminate fear. Is this pairing procedure necessary, or would respondent extinction suffice? Gentle touch may not be a functional reinforcer when delivered from a stranger, but may be a reinforcer when delivered by a familiar person without the explicit pairing procedure. This has, to my knowledge, not been systematically assessed.

Strength of Conditioning

Using the alternative procedure to establish conditioned stimuli (CS) involves pairing a potentially large number of diverse stimuli (e.g., play, tactile reinforcement, clapping, etc.) with food. In laboratory studies, it has been shown that variable CSs produce less robust conditioning than unchanging CSs (e.g., Kirkpatrick, 2014). Additionally, if play or tactile stimulation is used outside of this explicit pairing procedure, such interactions may be subject to latent inhibition, also reducing the degree of conditioning (Lubow, 1973). Taken together, it is plausible that conditioning playing as a secondary reinforcer paired with food is rather ineffective unless novel toys and novel treats are used, and in a consistent manner. Indeed, it might be that the reason why the animal starts to play vigorously and seek the trainer's company has nothing to do with previous conditioning to food, but the inherently reinforcing properties of play. Over-reliance on the pairing procedure (e.g., tactile interaction paired with food) may be superstitious behavior on the trainer's part. Since conditioning occurs best when stimuli are novel and highly stereotypic, we may expect alternative conditioning procedures paired with food (play–treat/clap–treat/pet–treat/click–treat) to be less efficient than formal conditioning procedures paired with food (click–treat/click–treat/click–treat/click–treat). Thus, the clicker, as event marker, may be less effectively conditioned for alternative trainers than for formal trainers.

Secondary Reinforcers as Event Markers and Antecedents

In alternative training, stimuli that are innately reinforcing (e.g., playing), rather than neutral, are deliberately paired with food. How does this impact their effectiveness when delivered later, during actual training? Is the animal responding to the unconditioned or the conditioned properties of the alternative secondary reinforcer, to the postcedent or the antecedent (Figure 2)? Does it matter?

Different brain areas are involved in processing secondary positive reinforcers than in the processing of primary positive reinforcers (O'Doherty et al., 2002). Secondary reinforcers predict the imminent arrival of a primary reinforcer, and spark a dopamine cascade in

central parts of the brain (Panksepp, 1998, Schultz, 1998)—the covert anticipatory reaction in Figure 1. In contrast, there is no dopamine surge above baseline when the animal receives the primary reinforcer (Schultz, 1998). To my knowledge, the effect of the combination of innately reinforcing and conditioned properties in reinforcers (as in most alternative secondary reinforcers) has not been systematically studied.

Secondary reinforcement for the formal trainer typically consists of an event marker such as the sound of a clicker, established by having been paired with a primary reinforcer. Typically, the trainer clicks during or following the criterion behavior, and then follows up with one or several primary reinforcers (Table 1), such as a short play opportunity. How does an alternative trainer distinguish between the clicker and other alternative secondary reinforcers? Some novice alternative trainers may assume that any "secondary reinforcer," including for instance praise, may be used interchangeably—replacing the click. However, play followed by petting consists of two primary reinforcers in succession, rather than one secondary and one primary: this order of events would impact brain chemistry, and thus covert behavior, differently. If alternative secondary reinforcers contain innately reinforcing properties per se, it is difficult to know whether the animal responds to the conditioned or unconditioned facet of that reinforcer. This may seem like hair splitting, but is important, as brain chemistry, overt behavior and conditioning could be very differently impacted (Arias-Carrión & Pöppel, 2007).

Alternative trainers may use event markers and their alternative secondary reinforcers interchangeably and thus stimulate dopamine release less effectively. This in turn might reduce learning speed, elation, and retention, three desirable consequences from activation of specific dopamine neurons in the amygdalae (Langbein et al., 2007; Pryor, 2009; Smith & Davis, 2008).

Overreliance on explicit pairing procedures

Supposing that at least some novice trainers, whether formal or alternative, assume that secondary reinforcers are always established through an explicit pairing procedure carried out by the trainer (e.g., click-treat versus play-treat); this would affect the trainer's position regarding how many potential reinforcers are made available. It is thus plausible that a novice trainer may not consider using secondary reinforcers that have not been explicitly established by that person with that animal. For the alternative trainer, play or touch would thus not be considered unless the pairing procedure had occurred. An alternative trainer would thus initially recognize a smaller number of available reinforcers to choose from, and satiation may end the training session sooner for the alternative trainer training a novice animal.

Assuming secondary reinforcers are inferior

The nomenclature itself suggests that secondary reinforcers might be interpreted as less important than primary reinforcers. The novice trainer might thus make training decisions based on the assumption that secondary reinforcers are less effective than primary reinforcers. For alternative trainers, there will only be a handful of effective reinforcers available (food, drink), others (e.g., play), would be considered ineffective. Alternative trainers might thus be less inclined to use play than food as reinforcers.

For formal trainers, most reinforcers are considered primary; no a priori distinction will be made as to the reinforcing properties of, for example, food versus play. Alternative trainers might therefore potentially recognize fewer effective reinforcers for a given situation. However, the effectiveness of any given primary reinforcer, whether seen from the formal or alternative perspective, remain conditional based on motivating operations operative at the time (Laraway et al., 2003).

Jackpot options

Variety is reinforcing (Hollerman & Schultz,1998), and is typically used by both formal and alternative trainers. However, jackpots are typically selected from among primary reinforcers—perhaps due to beliefs about effectiveness mentioned above.

When choosing jackpots, alternative trainers have a reduced option compared to formal trainers.

Satiation and respondent extinction

Reinforcer potency (i.e., effectiveness at any given time) will decline differently depending on which training school to which you adhere. For the formal trainer, secondary reinforcers will undergo respondent extinction if the secondary reinforcer is allowed to occur too many times without the primary reinforcer following it (e.g., Clayton & Savin, 1960), whereas primary reinforcers may momentarily lose their reinforcing properties through other means, such as satiation (an abolishing operation). For the alternative trainer, this distinction may get blurred, as some alternative secondary reinforcers, such as petting, may retain their reinforcing properties even in the continued absence of follow-up tidbits, whereas the sound of the clicker will not. Vice versa, the clicker will not satiate no matter how many times it is sounded in one training session, as long as it is followed by a primary reinforcer, but petting may. However, the alternative trainer may consider petting and clicking to be equivalent, since they identify them both as secondary reinforcers, and establish them through the same procedure. The alternative trainer may thus inadvertently risk satiating the subject with respect to one type of secondary reinforcer, and respondently extinguishing responses maintained by another secondary reinforcer. Formal trainers will find it easier to make the distinction between reinforcers that stop working because of satiation or respondent extinction.

Additionally, novice trainers may keep offering the same primary reinforcer beyond satiation, oblivious to the fact that it is no longer functioning as a reinforcer. During initial training, before conditioning any secondary reinforcers, alternative trainers would be at greater risk of doing this since their choice is more limited.

Distractors in the environment

Trainers would potentially differ in how aware they are of other reinforcers available in the environment that may compete for the animal's attention during training. Arranging the environment to limit distractions (concurrent competing contingencies) is a huge part of preparing for a training session. An alternative trainer may not consider the potential implications of whether the environmental arrangement includes competing contingencies of various kinds due to the factors discussed above. An inexperienced alternative trainer may have a weaker understanding of potential distractors interfering with training than a novice formal trainer.

Understanding Unwanted Behaviour

Animals are conditioned from natural consequences occurring in the environment, and may start exhibiting non-criterion behaviors as a result. By recognizing primary reinforcers other than food, water, air, and sex (Table 1), one may better analyze situations involving unwanted behaviours and the contingencies of which they are components. If trainers consider only food, water, and air as primary reinforcers, they may believe that there is just a small number of reinforcers available, especially if they adhere to some of the other misconceptions listed above. Alternative trainers would thus be expected to have more difficulties identifying obscure reinforcers that maintain problem behavior.

Tuning in with the Scientific Community

Scientific studies have found that primary and secondary reinforcers are processed in different parts of the brain and through distinct neural mechanisms (Beck et al., 2010). However, different primary reinforcers (e.g., food, sex) may also be processed in different parts of the brain (Sescousse et al., 2013), thus supporting the formal perspective on reinforcers as opposed to the alternative. So, alternative trainers are not in alignment with the experimental scientific community and may thus misinterpret scientific findings.

Conclusion and Recommendation

I found ten lines of argument in favor of the formal approach and one in favor of the alternative approach to using secondary reinforcers. My suggestion is thus to teach the

formal definitions to novice trainers but add the cautionary tale that a trainer should never make assumptions that a certain stimulus is actually a functional positive reinforcer when delivered from that person to a particular animal. Simply observing the animal may be one way of finding out: is the stimulus in question a reliable reinforcer or not? Does the subject exhibit behaviors that function to enhance access to it or not? In case of doubt, pairing the stimulus to a known primary reinforcer may be one solution to reduce the risk of frustration-induced or fear-induced aggressive behavior. The latter may occur if one uses, for example, attention and petting as putative reinforcers under circumstances in which these are not in fact effective reinforcers, and indeed may elicit aversive emotional reactions (particularly if delivered by a stranger), causing aversive arousal that disrupts continued training.

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