

## The Aim, Progress, and Evolution of Behavior Analysis

Edward K. Morris  
University of Kansas

The title of this paper celebrates three of B. F. Skinner's most fundamental contributions to behavior analysis. In "The Concept of the Reflex in the Description of Behavior" (Skinner, 1931), he established "prediction and control" as the aim of the science of behavior (see Hayes & Brownstein, 1986). In *The Behavior of Organisms* (Skinner, 1938), he proposed the three-term contingency as the unit of analysis with which we would make effective progress (see Moxley, 1984). And in "The Operational Analysis of Psychological Terms" (Skinner, 1945), he introduced radical behaviorism as the philosophy for the successful evolution of our science (see Schneider & Morris, 1987) (see Table 1).

Skinner made these contributions relatively early in his career, after which he expounded on their every permutation and implication. What have the consequences been 50 years since the beginning? In that mere nanosecond in the history of science, behavior analysis has emerged as a discipline of three robust branches: (a) the experimental analysis of behavior—for basic research (Skinner,

1966a), (b) the applied behavior analysis (which is also experimental)—for clinical and community interventions (Baer, Wolf, & Risley, 1968), and (c) the conceptual analysis of behavior—for philosophical, theoretical, and historical investigations (e.g., Day, 1980). (For accessible and thoughtful overviews, see Michael, 1985; Reese, 1986.)

With the development of behavior analysis as a scientific system and a discipline, we have come to understand better the nature and complexity of behavior, as well as the nature and complexity of our basic and applied science and our philosophy. As robust as these three branches may be, however, they are not mainstream in psychology; worse yet, behavior analysis is sorely misunderstood and mischaracterized. The reasons for these unhappy developments (or lack thereof) are, of course, myriad. For present purposes, I parse them into external and internal conceptual problems (see Laudan, 1977).

### *External and Internal Conceptual Problems*

Our external conceptual problems—reasons why behavior analysis is not mainstream or accurately represented—pertain largely to differences and conflicts between behavior analysis and other ways of knowing in the behavioral and social sciences and in the culture at large. The internal conceptual problems reflect tensions and inconsistencies within the discipline.

*External conceptual problems.* The most pervasive external problems are philosophical, cultural, and linguistic. The philosophic problems encompass our "theory of mind" (Dennett, 1978; see Skinner, 1977a) and the counter-intuitive approach we take to human agency, for instance, to the causal efficacy of thoughts and feelings (Bandura, 1982;

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This manuscript is based on my 1991 presidential address to the Association for Behavior Analysis. I dedicate it to B. F. Skinner and his empirical epistemology; to my students, who have taught me more than I have taught them; and to my earliest and still-current mentors in scientific psychology—Sid Bijou, Parker Lichtenstein, and Irv Wolf. Because the original address has been revised for archival publication, some issues have been formalized, some material added and deleted, and some figures altered or omitted. I thank Bernard Guerin, Bryan Midgley, Pete Peterson, Susan Schneider, Ilene Schwartz, and Jim Todd for their comments on earlier drafts of the manuscript. As for the figures, here and in the address, I thank Shelley Marcotte and Roger Poppen. Shelley was generous to a fault in assisting with graphics; Roger sent camera-ready copies of his cartoons. Reprints of the present manuscript are available from me at the Department of Human Development, Dole Human Development Center, University of Kansas, Lawrence, KS 66045-2133.

TABLE 1

## Aim, Progress, and Evolution

	Skinner's contributions	Behavior analysis misrepresented	Behavior analysis unpacked
Aim	Prediction and control	Arbitrary control	Understanding
Progress	Three-term contingency	S-R psychology	Context
Evolution	Radical behaviorism	Mechanism	Contextualism

see Skinner, 1974). The cultural problems include our nontraditional perspective on such social values as freedom and dignity (Black, 1973; see Skinner, 1971). And the English language itself is an impediment. Its ubiquitous actor-as-agent syntax—for instance, “The rat pressed the bar” or “Ed delivered an opaque presidential address”—supports common-sense causal attributions of the actor as causal agent of action—as in “The rat caused the bar to be pressed” or “Ed is responsible for his opaqueness” (Hackenberg, 1988; Himeline, 1980). In learning to speak the English tongue, we are implicitly taught a romance-language theory of mind (and its accompanying phenomenological impression and expression)—not a scientific one.

Behavior analysis is also not mainstream and is sorely misunderstood and mischaracterized because of, dialectically, the perpetuation of its misrepresentation in the professional literature (e.g., Chomsky, 1959; Mahoney, 1989; see Andresen, 1990; Catania, 1982, 1991; MacCorquodale, 1970; Morris, 1990), the popular press (e.g., Leo, 1983; see Morris, 1985), and classroom teaching materials (see Todd & Morris, 1983). As Charles Darwin (1872/1962) once wrote of similar difficulties, “Great is the power of steady misrepresentation” (p. 476; see Todd & Morris, in press).

Misrepresentation, of course, is nothing new. Indeed, only a tart tongue and a little cynicism are required, as the following not entirely original story illustrates. (Actually, when I first practiced my address, I called this a joke, but my preview audience did not laugh, so now it is a story.) It is about two airline passengers:

The first passenger asked what the second one did for a living. The second replied, “I’m a behavioral psychologist.” “Ah,” quipped the first, “I’m ok, you’re ok—huh?” “No,” said the second, this time with emphasis, “I’m a *behavioral* psychologist.” “Oh, I see,” said the first, “All M&Ms and shock therapy.” The second passenger paused a little dejectedly and asked what the first passenger did. The first replied, “Oh, I’m an astrophysicist.” “Ah,” said the second, seizing the opportunity, “Twinkle, twinkle, little star.”

*Internal conceptual problems.* Behavior analysis is not mainstream or accurately represented solely because of external conceptual problems, but also because of internal problems. It would be short-sighted of us to overlook the possibility.

Within any discipline, internal conceptual problems are found in tensions concerning the conduct of science (e.g., its level of analysis, see Himeline, 1986), its application (e.g., as science or technology, see Deitz, 1982), and its conceptual program (e.g., radical behaviorism and interbehavioral psychology, see Morris, 1982)—and sometimes in purview and name (e.g., behavior analysis, behaviorology, emergent behaviorism, paradigmatic behaviorism, praxics). Such tensions have not gone unnoticed in behavior analysis, nor are they always deleterious. Indeed, the evolution of scientific practices requires conceptual variability on which selection by consequences may operate.

In any event, one reaction to these tensions has been to pursue doggedly more of what we have always done—research and application—which is a conservative and a right thing to do. Another reaction has been to separate into alternative intellectual communities, which become, for instance, special interest groups (e.g.,

the Interbehaviorists in ABA) or independent societies (e.g., The International Behaviorology Association)—a liberal, sometimes useful, and sociology-of-science thing to do. Still another reaction has been the emergence of the confused eclecticism of cognitive behaviorism (e.g., Mahoney, 1974; see Biglan & Kass, 1977) and even the wholesale defection to cognitive science (e.g., Wasserman, 1981; see Morris, Higgins, & Bickel, 1982). The last two are wrong (or at least not useful) things to do, depending on what we take cognition to be: process or product, cause or consequence (see Deitz & Arrington, 1984).

So what is a behavior analyst to do to make behavior analysis more mainstream and more accurately represented? The philosophical, cultural, and linguistic problems are beyond much immediate control, although we should expend every effort to change the world around us. As for the misrepresentations, the Association for Behavior Analysis has an administrative board and a special interest group, as do other behavioral organizations, that proactively and reactively disseminate information and materials.

These latter activities are good directions to be moving in, but they may not be sufficient. Indeed, in some regards, their effects are unknown and may be of questionable value in promoting generalized and persistent change among those who misrepresent or criticize behavior analysis, or among those who seek to make it over or simply withdraw. (By the way, to criticize behavior analysis is not necessarily to misrepresent it. If criticism were only misrepresentation, then behavior analysis would be above criticism—bad science at worst, arrogant behaviorism at best; see Neuringer, 1991.) As for the effects of dissemination, they are unknown because we have little empirical evidence of any. And they are questionable because our approach may not reflect the best in applied behavior-analytic practice: We have generally sought to change the behavior of our critics without analyzing the variables controlling their behavior.

This last point suggests an obvious (and

a painful) conclusion: The critics of behavior analysis are right. They are right in Skinner's sense, in the sense that the organism is always right. Skinner, of course, said this first, perhaps initially through T. E. Frazier, in *Walden Two*: "The subjects were always right" (Skinner, 1948, p. 271). In other words, behavior is lawful—including that of our critics. If it is lawful, we might analyze it and alter its controlling variables. In the process, we might ask what our critics see and hear or, perhaps more importantly, what they do *not* see and hear that causes their misunderstanding and mischaracterization. This is a humbler behaviorism (see Chance, 1991).

In other words, in addition to engaging in and disseminating more science and application, we might improve our place in the mainstream and reduce the misrepresentation if we examine the variables that control our external and internal conceptual problems, that is, the behavior of our external and internal critics. If we could identify these variables, and alter them, this might lessen our problems, as well as enhance and enrich our discipline. At least this is another approach. It is also an approach congruent with Skinner's dictum that we take an experimental attitude not only to our subject matter (Skinner, 1938), but also to our lives (Skinner, 1981; Skinner & Vaughan, 1983) and our cultural practices (Skinner, 1948, 1953, pp. 333–436). Why, then, not to our science and our profession? They, also, are cultural practices.

#### *Economical to a Fault*

What might some of the controlling variables be for our external and internal problems and for the misrepresentation? One factor, and the basis for much of what follows, is that we have sometimes—not always, but sometimes—been economical to a fault in describing the aim, progress, and evolution of behavior analysis. We have sometimes been economical to a fault in describing Skinner's three contributions, describing them to ourselves and our students, to our col-

leagues and cohorts, and to a culture at large. We have sometimes—not always, but sometimes—not been explicit enough about important conceptual nuances and assumptions. As a consequence, we have invited segregation from the mainstream, as well as misrepresentation, both from without and within.

I realize, of course, that economy of style is prized in science, and it should be. Echoing the physicist-philosopher Ernst Mach (1883/1960), Skinner wrote that the criteria for judging the worth of a scientific system are its “usefulness and economy” (Skinner, 1938, p. 438; see also Skinner, 1957, p. 45; cf. Marr, 1985; Smith, 1986, pp. 259–297). My point is that the criterion for judging the worth of “economy” in science is also its usefulness. No particular level of economy—in expression, illustration, or metaphor—is necessarily or inherently the most useful level, at least not for every purpose, every occasion, or every audience. The usefulness of any particular level of economy will change with the questions we ask and the challenges we face.

This is a lesson that my students have taught me. They taught me, for instance, that telling them that “believing in God is [economically, after all] only operant behavior” is economy run amuck. It does not play in Peoria; it does not play in Lawrence, KS—at least not in the first week of class. The chilling effect such economy has on the classroom atmosphere reflects the social invalidity of certain economical forms of expression, as well as the inappropriateness of certain sequences of academic programming. The social invalidity of some ways of “speaking behavior-analytically” (not necessarily technically) and poor programming may contribute to our difficulties.

If our economy of style is at fault, either externally or internally, then we have gained an understanding of our problems and misrepresentation, and an understanding of what some of the solutions might be. In what follows, I argue that our economy of style in describing our aim, our progress, and our evolution has

had three pernicious effects, or at least has contributed to them. First, our economy of style sometimes makes us appear more interested in arbitrarily controlling behavior, as our aim, than in understanding behavior. Second, it suggests that the three-term contingency is a context-free stimulus-response psychology. And third, it has maintained an image of our world view as mechanistic and inherently unable to address interesting or complex behavior (see Table 1).

*Unpacking behavior analysis.* These consequences are not inherent in the aim, progress, and evolution of behavior analysis, nor are they inherent in Skinner’s contributions. So, again, what is a behavior analyst to do? I think the answer (or at least an answer) is that we need to open up—not loosen up or necessarily add to—but to unpack Skinner’s contributions. We need to unpack prediction and control, the three-term contingency, and radical behaviorism. In the process, we will affirm the cumulative progress we have made to date and enrich the possibilities of our future. That enrichment, I might add, will not usefully include any form of mentalism or physiological reductionism. It will include, though, cognitive phenomena as part of our subject matter and biology as it participates in all behavior, issues I address later.

In the language game of these post-modern, poststructuralist times, then, we need to “deconstruct” what we mean by the aim, progress, and evolution of behavior analysis. In so doing, I think we will uncover the kind of understanding, the context, and the world view that are the basis of our science of behavior. Our survival—as scientists, as a discipline, and as a culture—may depend on this.

I think Skinner would have wanted us to do this, to unpack his contributions. Indeed, I think he knew we had to, for at the end of *The Behavior of Organisms*, he wrote of his own system of analysis:

It would be an anomalous event in the history of science if *any* current system should prove ultimately the most convenient (and hence, so far as science is concerned, correct). (Skinner, 1938, p. 438)

Later, in *Contingencies of Reinforcement*, Skinner (1969) wrote:

Behaviorism, as we know it, will eventually die—not because it is a failure, but because it is a success. As a critical philosophy of science, it will necessarily change as a science of behavior changes. (p. 267)

Behavior analysis, then, will progress and evolve; its form was never foreordained or immutable. Skinner's philosophical pragmatism and his empirical epistemology would never have allowed that (Skinner, 1945, 1957; see Smith, 1986, pp. 259–297). As Marr (1985) has described these matters:

Science is not some exalted, incorrigible, Platonic domain of Truth, but a *human* activity after all, controlled by history and circumstances and consequences. (p. 137)

The truth of my foregoing assertions lies not in any agreement about them (i.e., a correspondence theory of truth), but rather in their consequences (i.e., a pragmatic theory of truth)—two consequences, especially. First, after unpacking Skinner's contributions, will we be able to take more effective action as behavior analysts doing behavior analysis—basic, applied, and conceptual? And second, will we be able to take more effective action relative to (or better, in cooperation with) psychology, philosophy, education, sociology, ethology, business and industry, linguistics, rehabilitation and training, law, anthropology, medicine, corrections, pharmacology and toxicology, gerontology, and the like? The answers, I think, are “yes,” though for now more as promise than proof. But we should not be overly circumspect: Where we have been allowed to take action, we have been extraordinarily successful (e.g., behavioral pharmacology, developmental disabilities). I see no reason this should not continue.

Making these arguments takes me, first, to the aim of behavior analysis as the prediction and control of behavior, which, unpacked, become “understanding.” Second, it takes me to the three-term contingency as our unit of analysis, which, unpacked, is found embedded in context. And third, it takes me to radical

behaviorism as our philosophy of science, which, unpacked, is contextualistic in world view (see Table 1). When I am through, I hope you will see the aims I see, share with me my optimism about our current and future progress in analyzing behavior, and be as enthused as I am about the evolution of behavior analysis.

### THE AIM OF BEHAVIOR ANALYSIS

In his 1931 article, “The Concept of the Reflex in the Description of Behavior,” Skinner argued, via historical analysis, that “prediction and control” was the aim of behavior analysis (see Skinner, 1938, 1953, 1956; cf. Hayes & Brownstein, 1986). To this aim, we should add “description” because observation is prerequisite for effective prediction and control and because Skinner's behaviorism is a “descriptive behaviorism” (see Smith, 1986, pp. 259–297).

“Prediction and control,” however, should not be construed too narrowly. As the aim of behavior analysis, they are an economical way of stating our pragmatic criterion of empirical truth—“effective action.” In the world view of contextualism, this is rendered as “successful working” (Pepper, 1942/1960). Effective action and successful working are, in turn, our economical way of saying that we “understand” behavior. “Understanding behavior,” then, is our natural language way of stating the aim of behavior analysis. Or, put in behavior-analytic form, what are the occasions on which we say—or on which we tact—that we “understand” behavior? What are the stimulus controls over saying such a thing? One answer: when we can predict and control behavior.

This seems economical enough, but it is possibly economical to a fault because prediction and control lend themselves to both a weak and strong sense of what it is to understand behavior. As a result, the two have sometimes been conflated by the social invalidity of how we sometimes describe our aims. The misrepre-

sensation this can cause sometimes makes our critics right in accusing our epistemology of being shallow.

### *Description and Prediction*

I will not pursue a deconstruction of how description and prediction lend themselves to the weak and strong senses of understanding, except to make one point. When description and prediction comprise mere speculation about how it is that behavior is as it is, then our understanding is weak. But when they are based on *and constrained by* the principles of behavior, then understanding becomes stronger and deeper. Our understanding is deeper still if it is based on the experimental analysis of behavior of the late 1900s, not of the mid-1900s (Hayes, 1987; see, e.g., Epling & Pierce, 1983). Describing and predicting behavior in terms of behavioral history (Wan-chisen, 1990), the matching law (Davison & McCarthy, 1988), establishing operations (Michael, 1982), and stimulus equivalence (Sidman, 1986), for instance, offer a deeper behavior-analytic understanding than we had in an earlier era. Description and prediction of this sort, not mere speculation, are what Skinner meant by behavior-analytic "interpretation" (Skinner, 1974, pp. 228–229)—whether verbal (descriptive), organismic (analogue), or formal (logical or mathematical) (see Donahoe & Palmer, 1989).

### *Control*

The weak and the strong senses of understanding with respect to control are graver matters because it is here that our economy of style has led to more serious misrepresentations. For William James (1892), founder of functionalism, and John B. Watson (1913), founder of behaviorism, the goals of the science of psychology were prediction and control—prediction and control of experiential and experimental analysis, somewhat respectively. Watson, however, later construed control more narrowly and literally (see Buckley, 1989): Behaviorism would be

true to the extent that behavior could be controlled for the sake of social engineering.

Just because we can control behavior, however, does not mean we understand behavior. It never did, at least not in any strong sense of an ordinary language meaning of "understanding." With control defined so narrowly, no wonder people have found (and find) behaviorism shallow and frightening (e.g., Black, 1973). We owe Watson many debts, but we should be careful about what we accept of his legacy. You may recall, for instance, that Watson (1913) once suggested that thinking takes place in the larynx. If so, then pity the poor behaviorist with laryngitis—a sore throat would be fatal to mentation.

In modern behavior analysis, control better serves our epistemology, our theory of knowledge. Here, we say we understand behavior in the strong sense when we have *discovered* what controls behavior, not simply when we have *demonstrated* how to control it by any means possible or even reasonable (Morris, 1991). That is, we understand behavior when our experimental analyses (basic or applied) tell us how it is controlled or, more accurately, what it is a function of. Our economical description of the aim of behavior analysis as "prediction and control, period" has sometimes hidden this distinction. It has hidden the distinction between arbitrarily imposed control and control discovered via experimental analysis. That is, our economy of style has hidden the distinction between the weak and the strong senses of understanding. Overlooking the distinction is, I think, a controlling variable over why behavior analysis is sometimes misunderstood and mischaracterized.

*Implication 1: Applied behavior analysis.* This is not mere epistemological quibbling but has, I think, implications for applied behavior analysis, where the issue of control, especially aversive control, raises the most serious objections of all—as well it might (see Johnston, 1991). (I say, "I think," because I am no applied behavior analyst. I am more an anthro-

pologist among those who are, which my colleagues in Kansas have been kind enough to allow.)

The pivotal issue, it seems to me, turns on what we mean by "analysis" in applied behavior analysis (see Morris, 1991; Pierce & Epling, 1980). Is analysis the experimental *demonstration* that behavioral technology (for instance, time-out; see Brantner & Doherty, 1984) can modify socially inappropriate activity (for instance, disruptive classroom behavior or self-injury)? Or is analysis the experimental *discovery* of the functions (or of some of the causes) of the inappropriate behavior, followed by the demonstration that changing those variables (for instance, curriculum design, classroom scheduling, teacher attention, or modes of communicating) will change the behavior?

Demonstration alone, that is, control without discovery, will sometimes, maybe often, be necessary, especially when discovery wastes time or harms clients or is impractical or impossible, or when the difference makes no difference (see Baer, 1970; Horner et al., 1990). But of the two, the discovery of controlling variables—not the demonstration of control alone—gets at the strong sense of what it means to understand behavior. Demonstration and discovery also differ with respect to their consequences. Demonstrating practical solutions promotes the *growth* of applied behavior analysis, largely through systematic replication. Discovering and then demonstrating solutions promote its *development*. Growth and development are not the same thing.

*Implication 2: Philosophy of science.* This distinction between discovery and demonstration is not, in turn, mere clinical quibbling, but turns back on epistemology because the distinction parallels that between the "context of discovery" and the "context of justification" in the philosophy of science (Reichenbach, 1938/1961). The context of discovery refers to the relationship between scientist and subject matter in terms of the *contingencies that shape* and maintain scientific behavior. This includes what we

do when we do science and how our subject matter affects our behavior as scientists. In contrast, the context of justification refers to relationships among scientists in terms of the *rules that govern* the justification of claims of discovery. This includes what we do when we report and publish the results of the science we have done, and how, in turn, our colleagues and professional standards affect our behavior as scientists.

*Implication 3: Teaching.* Unfortunately, of the two, demonstration and discovery, the latter is less well understood in the philosophy of science and in applied behavior analysis, as it may be more difficult to teach and to learn. For instance, discovering the conditions that control inappropriate behavior and altering them may take more time and effort than demonstrating that a particular behavior-change technology (e.g., time-out) can suppress disruption. Teaching discovery involves the difficult task of bringing our behavior under the control of client behavior in context. In contrast, in demonstration, the behavior of behavior analysts need only be controlled by those for whom the inappropriate behavior is inappropriate and by the scientific standards of proof. The latter are easy grist for rule-governed instruction and application, especially with our robust, ready-made, and easy-to-learn research designs (Baer, Wolf, & Risley, 1987, pp. 319–320; Michael, 1980, pp. 8–9; see, e.g., Barlow & Hersen, 1984). This differential effort required for understanding, teaching, and engaging in discovery may partially explain the (sometimes boring) proliferation of demonstration/justification in the philosophy of science, psychology, and applied behavior analysis (e.g., reinforcement modifies yet another behavior).

*Implication 4: Empirical epistemology.* These points, of course, are assertions—assertions that need to be put to the empirical test. We could, for instance, analyze how we train applied behavior analysts (if and when we ever do analyze our training). We could assess whether discovery is more difficult to teach and

to learn than demonstration. If so, and if discovery is important, we could intervene. We could intervene, for instance, at the level of graduate instruction or at the level of journal acceptance policies (e.g., requiring functional analysis where warranted). In other words, we could teach and differentially reinforce discovery, should we so choose.

This is philosophy of science gone applied: the application of our science to the conduct of our science, to the experimental analysis of the teaching of our scientists, and to the maintenance of their behavior. It is, in part, Skinner's empirical epistemology.

### *Discovering, Ordering, and Resolving Puzzlement*

All this said (and aside), we have still not fully unpacked the aim of behavior analysis. Not only has our economy of style sometimes led critics, and ourselves, to overlook the deep, strong sense of understanding meant by control, but it has also led us to overlook what it means to understand understanding. I am not trying to be opaque in saying such a thing. Rather, I simply want to point out that we, as behavior analysts, have as rich and varied, and as finely and deeply structured, a cognitive and metacognitive life as any cognitive psychologist (cf. Baer, 1989).

In other words, understanding is not just effective action in the first two branches of behavior analysis, basic and applied research. Effective action also applies to the third branch of behavior analysis, the conceptual analysis of behavior. Here, effective action concerns not the behavior of "the other one" as our subject matter, but the description, prediction, and control of the behavior of "the one"—of our behavior as sentient, reflective, and introspective scientists. Again, Skinner (1979) said this first, this time in a 1945 reference to the work that would become *Verbal Behavior*. He wrote:

I decided to leave out all experimental data. (An interesting question then arose: what survived to reinforce writing or reading the book? . . . My re-

inforcers were the discovery of uniformities, the ordering of confusing data, the resolution of puzzlement.) (p. 282)

These are among the consequences of effective conceptual (as well as empirical) analysis. These are the consequences that, for example, positively and negatively reinforce researching the literature; reading books and manuscripts, published and unpublished; taking, keeping, and organizing notes; talking and arguing with colleagues at conventions and departmental colloquia; and writing and revising outlines and preliminary drafts of manuscripts. In some cases, these consequences directly enhance our effectiveness in predicting and controlling behavior. In other cases, the effects are more indirect, acting through social and professional contingencies in the psychology and sociology of science. In either case, they are part of science as a process (Hull, 1988).

Discovering uniformities, ordering confusion, and resolving puzzlement are economical ways of defining effective conceptual action and successful working. In turn, effective conceptual action and successful working are economical ways of stating what it is to "understand" behavior. This is the stuff of thinking and problem-solving—describing, predicting, and controlling the variables of which our own behavior is a function (see Skinner, 1953, pp. 227–294). More formally, this is the stuff of epistemology and logic. But thinking and problem-solving, much less epistemology and logic, are not captured well by our economical rendering of the aim of behavior analysis as "prediction and control, period." They are, however, very much part of Skinner's contribution. We should unpack it further.

*Implication 5: Empirical epistemology again.* Let me conclude this section by suggesting again that we conduct science on our science, this time, for instance, analyzing how the aim of behavior analysis is understood and misunderstood, and might be changed—an appropriately behavior-analytic strategy for understanding understanding. We will have to select convenient experimental prepara-



tions, of course, such as college classrooms or workshops. One intervention across comparable preparations might be to describe the aim of behavior analysis first as “understanding,” before refining it into “control,” assessing all the while how people evaluate the aim of behavior analysis. Or perhaps we could run a control group in reverse, or pretest-posttest—whatever. This is the stuff of social validity, in this case, an experimental analysis of the social validity of our science with respect to whether people find our aim acceptable or not (cf. Wolf, 1978). In an era of tight federal funding and an influential mass media, we need to consider the social validity of how we take our science into the marketplace of ideas and use our empirical methods to ascertain best how to do that.

Someone might object that this would only change verbal behavior, not understanding—but not necessarily so. If we do it right, if we bring verbal behavior under the control of what we take behavior analysis to be, then we will change understanding. If we do it right, if we teach students that their textbooks are a combination of loosely programmed instruction and rhetoric that modifies their behavior, then some students, I have found, will begin to question and protest textbook misrepresentations of behavior analysis (see Todd & Morris, 1983)—sometimes to the consternation of my colleagues. More practically, if we want to understand how people understand behavior analysis, why they understand it as they do, and what controls their assessment of its social validity, then we should phrase our questions empirically. Empirical questions are the best kinds of questions we know how to answer. (The new “Teaching of Behavior Analysis” ABA special interest group is one of the best places to take our answers.)

In summary, when we unpack the aims of behavior analysis, we find them far richer and deeper than what prediction and control might superficially imply. Behavior analysis entails understanding—understanding grounded in effective empirical and conceptual action. In the words of Ernst Mach (1905/1976):

TABLE 2

## Five senses of contextualism

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1. Pragmatic criterion of truth
  2. Functional relationships
  3. Context dependent
  4. The “historic event”
  5. A world view
- 

The worth of scientific inquiry can be judged by the extent to which an investigator’s behavior really leads to practical [empirical] and intellectual [conceptual] advantages. (p. 11)

This is the criterion of the truth (and worth) of philosophical pragmatism—the pragmatism of William James (1907) and Charles S. Peirce (1940), of John Dewey and Arthur Bentley (1949). This is the pragmatism that is the truth criterion of the world view of contextualism, “successful working” (Pepper, 1942/1960), which is also the truth criterion of behavior analysis (Hayes & Brownstein, 1986). This, then, is one sense in which behavior analysis is contextualistic in world view (Table 2). That contextualism is the world view of behavior analysis is a point I address again later.

### THE PROGRESS OF BEHAVIOR ANALYSIS

Having described the aim of behavior analysis as the prediction and control of behavior, I now turn to what Skinner offered in *The Behavior of Organisms* (Skinner, 1938, p. 178) as a unit of analysis—the three-term contingency (Figure 1a). In what follows, I examine its utility in the progress we have made and may continue to make (or not) in understanding behavior. I begin with some historical background.

#### *Historical Background*

Prior to *The Behavior of Organisms*, and for some time thereafter, much of psychology was a two-term S-R psychology (Figure 1b). Skinner’s genius was to see the inadequacy of such a unit of analysis and to propose a three-term psychology in which the Ss and Rs func-

$$S^D - R_o - S^R \quad (a)$$

$$S - R \quad (b)$$

$$S^D \leftrightarrow R_o \leftrightarrow S^R \quad (c)$$

$$S \rightarrow R \quad (d)$$

$$S \leftrightarrow R \quad (e)$$

$$S^D \rightarrow R_o \rightarrow S^R \quad (f)$$

Figure 1. Varieties of two- and three-term contingencies. The subscript "O" for the R is for "operant." The superscripts "D" and "R" for the Ss are for "discriminative" and "reinforcing," respectively.

tioned differently. First, his consequent S is a reinforcing or punishing stimulus for responding. It is a cause of behavior but not a mechanical push or a pull; rather, it selects behavior (Skinner, 1981).

Second, the antecedent S is a discriminative stimulus; it sets the occasion for responding but does not mechanically impel it. One might say that a discriminative stimulus is "context" for the response-reinforcement relationship, but that is not the sense of context in contextualism, which I address later.

Third, the response is an instance of an operant class of behavior, not a reflex. It is functionally defined with respect to its consequences and antecedents, just as those consequences and antecedents are functionally defined with respect to it. To use a homey example (my students call it a "homely" example, one borrowed from Irv Wolf), were you to see me running along the streets of Lawrence, you would not necessarily know the meaning

or function of the form of my behavior unless you knew something about its antecedents and consequences, past and present. I could be either running for a train or training for a run—two different behaviors when functionally, not formally, defined.

This last point describes a second sense in which behavior analysis is contextualistic (Table 2). As a unit of analysis, the three-term contingency comprises co-defining functional relationships among stimuli and responses (Reese, 1986). As such, it is actually better depicted as a relationship among terms that is reciprocal and functionally defined (Figure 1c). The function, meaning, or definition of a stimulus (for instance, a reinforcer) lies in its relationship to a response and vice versa. This was exactly the point of Skinner's 1931 paper mentioned earlier. He said it first there and many times thereafter (e.g., Skinner, 1935, 1938, 1953, 1974). Although functional relationships are categorical to contextualism, they, by themselves, do not constitute that world view. Contextualism is more than functional relations alone.

The three-term contingency aside, we might note that behavior analysis is actually an S-R psychology because those are the only two types of terms we have (see Kantor, 1933; Skinner, 1935). Our critics are right about this, but for the wrong reasons. They are wrong in construing behavior analysis to be an S-R psychology in which stimuli lie in a causal relationship to and physically impel responding, such that S and R are linked by a one-way arrow that transfers energy through time and space (Figure 1d).

In contrast, Skinner conceptualized stimuli and responses as co-defining functional relationships. This is an S-R psychology without spatial or temporal gaps that have to be filled with physical or psychic energy (Ringen, 1976; Figure 1e). It is an S-R psychology of functional relationships described in terms of a two-way arrow. It is a theory of direct (non-mediated) behavior, a theory of direct behavioral relationships. In this sense, it is similar to Gibson's (1979) theory of "direct perception" (see Costall, 1984) or

Watkins' (1990) nonmediational theory of "direct memory." We might also note a similar relationship to the developmental systems perspective, a theory of direct development (see Oyama, 1985). Likewise, the parallel distributive processing or neural network theory is a theory of "direct adaptation" (see Donahoe & Palmer, 1989). These approaches, like Skinner's, take the environment seriously (see Leahey, 1991, p. 377, on Neisser, 1984; see also Wilcox & Katz, 1981).

All of this is fine, but difficulty accrues to any S-R psychology, be it Skinner's generic S-R psychology or the older (but still current) mechanistic S-R version, because Ss and Rs are variable. That is, the presence of one does not always predict or control the other. Skinner handled this deftly by expanding his 1931 formula,  $R = f(S)$ , into  $R = f(S, A)$ , where his A was what he referred to as "third variables," that is, as conditions that change the relationships between stimuli and responses (Skinner, 1931, p. 452). By including third variables, Skinner had three generic terms in his psychology: response and stimulus (the first and second variables) and third variables. I refer to the last—to third variables—as "context" in what follows, meaning by it only what Skinner meant by third variables.

Context is a third sense in which behavior analysis is contextualistic in world view (Table 2). Stimuli and responses have no inherent or immutable functions. Their functions depend on their context, that is, all causes have contexts. Even though context so construed is categorical to contextualism, context by itself does not wholly define that world view. That we take context into account, either empirically or conceptually, does not make us contextualists in world view. It does not make us contextualists any more than taking behavior into account necessarily makes anyone a behavior analyst.

Between 1930 and 1938, context was central to Skinner's research program (Morris, in press). He investigated how historical and current factors (e.g., conditioning history and deprivation, respectively) produced and accounted for

$S \longrightarrow O \longrightarrow R$  (a)

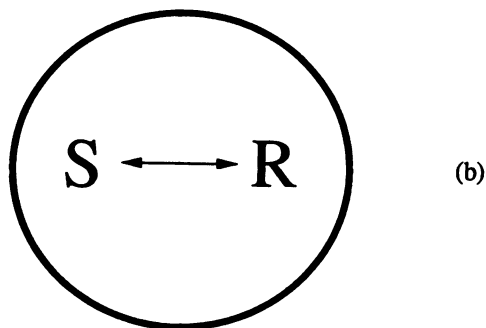


Figure 2. The O in psychology as (a) "organism" or (b) "context."

variability among stimuli and responses, variability Skinner took to be lawful and orderly. The mid-1930s was also the conceptual and temporal point at which Skinner's behaviorism began to break with the other behaviorisms of the day. The functionalist, Robert S. Woodworth (1929, 1940), for instance, had developed an S-O-R psychology (much like that of contemporary cognitive behaviorism) in which the O was the organism full of individual factors, namely, mediating structures, states, and other activities (Figure 2a). Molar behaviorist, Edward C. Tolman (1938), had proposed a formula in which  $B = f(E, I)$ , which was similar to Skinner's, but different. Although Tolman's E stood for environmental variables, his I stood for "individual" variables, which, for Tolman, were intervening variables and eventually hypothetical constructs—not context (see Morris et al., 1982). This was the sort of neobehaviorism from which cognitive psychology would emerge, as Skinner (1969) noted later:

[Tolman] put the "third" variables inside the organism, where they "intervened" between stimulus and response. . . . His intervening variables quickly assumed the function of mental processes (as they

were essentially designed to do), and it is not surprising that they have been warmly taken up by cognitive psychologists. (p. 28)

The S-O-R formula is necessary in order to account for variability in a mechanistically conceived S-R psychology. With nothing besides the S, R, and O, unexplained variability between the S and the R can only reside in the O. Skinner, too, had to account for variability, and he did so with his third variables or context. Context, however, does not exist between S and R, but rather, S and R exist within it. In other words, Skinner approached unexplained variability by having context encompass the relationship between S and R (Figure 2b). What the O accounts for in most of psychology (hypothetical motivational and cognitive structures and processes), context or third variables largely account for in behavior analysis.

What does any of this have to do with Skinner's three-term contingency? Unfortunately, as the behavior of my students tells me, too little, perhaps because of our economy of style. As the three-term contingency is typically depicted, it is a temporally ordered, linear sequence of stimuli and responses. Sometimes, though, it seems little more than a three-term contiguity in which one term seemingly impels the next through time and space (Moxley, 1982; Figure 1f). With temporal ordering such as this, contextual variables cannot affect the stream of behavior without intervening between S and R, except as hypothetical constructs.

Lacking either hypothetical constructs or context, the three-term contingency appears inadequate as a unit of analysis. It appears incapable of accounting for the variability in and variety of behavior, that is, the individual differences between organisms and within them over time. But whose fault is this? The fault of critics and former behavior analysts who fail to appreciate the three-term contingency (e.g., Mahoney, 1989; Molloy, 1984; Wasserman, 1981)? The fault of our economy of style in depicting a unit of analysis? Or both? Probably both, because misunderstanding, like all behavior (e.g., understanding), is an interaction. Context is pivotal here. We never

denied its importance, and Skinner's research program of the 1930s systematized and celebrated it. We may need to go back to that future to unpack our unit of analysis. This is what I turn to next.

### *Contextual Determinants of Behavior*

Context is a funny word. As a non-technical term, it can be vague and imprecise. As a technical term, it can be vague and imprecise—and has been throughout the history of psychology. In what follows, I mean to use it technically and precisely, much in the sense of Skinner's third variables (see Morris, in press).

First, the historical context—phylogenetic and ontogenic, biological and behavioral—establishes the current structure and function of biology (anatomy and physiology) and behavior (form and function). Second, the form or structure of the current context, organismic or environmental, affects (or enables) what behavior can physically or formally occur. Third, the current context affects (actualizes) the functional relationships among stimuli and responses (i.e., their "meanings" for one another). From this, two points follow.

First, context is first and foremost a generic third term. Like the other two terms, stimulus and response, it does not, in itself, specify a particular function. Stimuli, for instance, may have discriminative or reinforcing functions, and responses may be either respondent or operant, or members of multiple response classes. As for context, it might function as an establishing operation for reinforcement (Michael, 1982) or as a conditional discriminative stimulus for stimulus control (Sidman, 1986)—or more. (As an aside, whether "context" is the right word for a third term is beside the point. I make no claims or suggestions in this regard. I simply use it as a generic term, as Skinner did with "third variables.")

Second, as a generic third term, context is not being equated with either (a) multiple control (e.g., multiple discriminative stimuli; see Skinner, 1953, pp. 204–224) or (b) metacontingencies (see Glenn, 1988). That multiple control and meta-

contingencies do not fall under the purview of context defined functionally does not, however, make them unimportant. Indeed, all behavior is multiply controlled (i.e., every reinforcer has a history and an accompanying establishing operation). Multiple control and metacontingencies are necessary and critical for the analysis of complex cases (e.g., everyday behavior). For the moment, though, I am simply trying to unpack the generic unit of analysis—stimulus, response, and context—not all the possible behavioral relations that they might encompass.

An in-depth treatment of all that context might entail is beyond the scope of what I will address at present, but I can illustrate briefly how we might unpack some of its generic nature.

*Formal Relationships*

*The organism.* Although I tell my students that behavior analysis is not radically environmentalistic, some of them persist in claiming that it is. One reason for this, they tell me, is that the three-term contingency is devoid of an organism. Perhaps it should be for some purposes, but perhaps not for others.

If we look closely enough, though, discerningly enough, we find the organism implicitly embedded in the middle term—in the formal (not the functional) characteristics of the R of the three-term contingency (Figure 3a). The organism, as such, participates in the unit of analysis, influencing what behavior (the behavior of species and individuals) can and cannot occur. By unpacking the three-term contingency, we make biology more obviously something behavior analysis does not overlook (e.g., anatomy and physiology, sensory and neural systems, and the role of phylogenesis and biological ontogeny).

As for biological differences between species and between individuals, these are accounted for by differing phylogenetic and biological ontogenic histories, which can be made explicit with an arrow of time up from the past (Figure 3b). Skinner (1966b, 1975, 1981) wrote often about

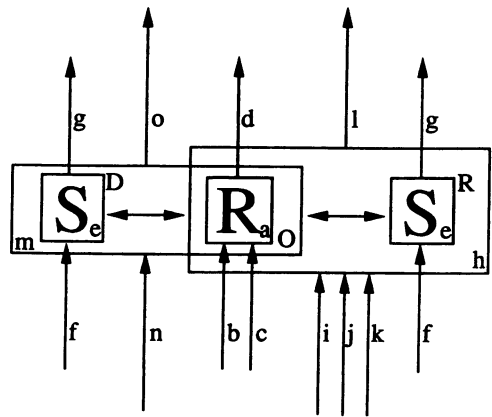


Figure 3. An illustration of various properties and functions of the generic nature of context.

the relationship between phylogenetic contingencies and behavior, of course, so this is not new, just more explicit. As for biological ontogenic history, that is, biological changes from conception to death, Skinner had less to say (Midgley & Morris, in press). Presumably, though, he conceptualized it as a third variable, for among his examples of the latter were injury, illness, and maturation (Skinner, 1938, pp. 18, 416; Figure 3c). Other examples of historical context so construed include, for example, the effects of early stimulation and toxins on subsequent behavior (e.g., Greenough, 1975; Rosenzweig, 1984).

Within-species and within-individual differences over time are also accounted for by biological ontogenic history that extends into the future (Figure 3d). Biological ontogenic history is an ongoing process, forever altering the ways biology participates in behavior. These changes can be made explicit with an arrow of time out toward the future and would include, again, the examples cited above.

That phylogenetic and biological ontogenic context were integral to Skinner's system, albeit not explicit in the three-term contingency, is evident in his well-known comment:

No reputable student of animal behavior has ever taken the position "that the animal comes to the laboratory as a virtual *tabula rasa*, that species differences are insignificant. . . ." (Skinner, 1966b, p. 1205)

*The environment.* The formal effect of the environment on behavior might also be unpacked (Figure 3e). It is embedded in the formal (not the functional) properties of the two stimuli within the three-term contingency. Here I refer to the form, structure, or ecology of the environment. The environment, too, participates in the unit of analysis, affecting (enabling) what behavior can and cannot occur. This ranges, for example, from the ecological features of playgrounds for children with developmental disabilities, to the architectural arrangements of group homes for independent living, to the physical form of walkers and door handles for clients with physical disabilities (i.e., ergonomics). Context as such has decided effects on the play, social interactions, and the independent living of the individuals living, working, and playing with and within it (see, e.g., Horner, 1980; Nordquist, Twardosz, & McEvoy, 1991).

Differences across individuals are, in part, accounted for by their differing histories in differing physical settings (Figure 3f). Likewise, differences within individuals over time will be partially attributable to future changes in the form or physical structure of their environments (Figure 3g).

*Organism and environment.* These formal effects of the organism and environment allow, deny, alter, and facilitate the formal characteristics of behavior. But, because they represent classes of controlling variables outside the basic behavioral processes, they have sometimes been taken for granted or left to the purview of other biological and environmental sciences. Divisions among the sciences are, of course, useful, but so too is interdisciplinary or transdisciplinary collaboration. Behavior analysts may have to take the lead in such collaboration, however, because anatomists, physiologists, and architects and manufacturers of prosthetics are not likely to see their interests represented in a context-free (naked) three-term contingency. Explicitly altering our unit of analysis might enhance our appearance to these disciplines and invite them to join us in interdisciplinary work.

### *Functional Relationships*

As for the role of context in affecting (actualizing) the behavioral relationships within the three-term contingency, I turn to the function of the current context—as a behavioral process. As depicted, the three-term contingency can appear static and mechanical (Figure 1a). At least this is what students and colleagues tell me. They see no explicit inclusion of terms or processes that account for the dynamic nature of behavior. In addressing this issue, I focus on the reinforcement relationship as my primary exemplar, though I offer a few comments later about stimulus control.

*Reinforcement.* No stimulus is inherently or always a reinforcer—that would be mechanistic. Kantor's (1933) "setting factors," Skinner's (1931) "third variables," Bijou and Baer's (1978) "setting events," Gewirtz's (1972) "contextual determinants," Goldiamond's (Goldiamond & Dyrud, 1967) "potentiating variables," and Keller and Schoenfeld's (1950) and Michael's (1982) "establishing operations" all acknowledge that the reinforcement relationship is contextually determined (Figure 3h). Variables, events, factors, and operations such as deprivation, drugs, schedules of reinforcement, and instructions (see, e.g., Morse & Kelleher, 1977; Schlinger & Blakely, 1987) account for why some consequences are reinforcers on some occasions, but not on others.

We must not overlook the historical context in this regard. Differences in the reinforcement relationship between species and individual are, in part, attributable to differences in phylogenic (Figure 3i) and biological ontogenic contingencies (Figure 3j), as depicted by the arrows of time from past to present (Skinner, 1966b, 1981). These histories give rise to differences in primary reinforcers and their parameters, presumably across species and individuals, as well as variations in the process of reinforcement itself. Not seeing these factors in the three-term contingency (or not reading our literature very deeply), our critics presume that we overlook ethological

considerations or, more specifically, that we overlook what they refer to as the "preparedness of associations" (e.g., Garcia & Garcia y Robertson, 1985; Seligman & Hagar, 1972; contra Schwartz, 1974) and the "misbehavior of organisms" (Breland & Breland, 1961; contra Skinner, 1977b). As Skinner (1966b) notes and to complete the preceding quotation:

No reputable student of animal behavior has ever taken the position . . . "that all responses are about equally conditionable to all stimuli." (p. 1205)

From a behavior-analytic perspective, *behavioral* ontogenic history (i.e., reinforcement history) accounts for the more meaningful, or at least more controllable, individual differences with respect to reinforcement (e.g., conditioned reinforcers) (Figure 3k). In the 1930s, "conditioning," "extinction," and "discrimination training" were third variables for Skinner (1931; see Barrett, 1986; Wanchisen, 1990). Throughout the history of behavior analysis, these have been central to the analysis of behavior (see Catania, 1992; Honig, 1966). However, they have not been analyzed much as independent variables in their own right.

Within-individual differences in the reinforcement relationship are accounted for by subsequent ontogenic behavioral histories, as depicted by the arrow of time from present to future (Figure 3l). Reinforcement histories change continuously, thereby altering the ways and the strengths with which stimuli participate as reinforcers. This is what, in part, makes the functional interrelationships within the three-term contingency dynamic and ever-changing.

The explicit inclusion of the historical context brings me to a fourth sense in which behavior analysis is contextualistic in world view. The root metaphor of contextualism is the ever-changing "historic event" (Pepper, 1942/1960) (see Table 2). Because the present is always becoming past for more present, the historical context of behavior is forever changing, and hence, so too is behavior (i.e., stimulus function-response function interactions). Behavior, then, is

emergent or constructed from the continuous interaction of organism and environment (see Donahoe & Palmer, 1989, on behavior analysis as an historical science).

In concluding my comments on the context of reinforcement, let me diverge briefly with a suggestion about the teaching of behavior analysis. My suggestion is that we teach students how to identify and correct misrepresentations of behavior analysis. With respect to the context of reinforcement, for instance, we might give them passages such as the following from a *Contemporary Psychology* review of Sidman's (1989) *Coercion and Its Fallout*. Here, readers are warned that they

should beware of the notion that when a specific behavior is followed by a reward there is an automatic increase in the likelihood that the behavior will be repeated. Contrary to the usual straightforward effects in laboratory rat behavior, rewards and punishments are interpreted by humans, who then act accordingly to their interpretations. (Brehm, 1991, p. 220)

Not seeing context in the three-term contingency, the reviewer accounted for variability in human behavior in terms of a process called "interpretation." But the "interpretation of reinforcers" is a consequence of the historical and current contexts of behavior, not a cause, either for rats or for humans. Indeed, from a behavior-analytic perspective, most cognitive and motivational terms refer to the consequences of behaving in context, not to the causes. Exercises such as these can hone a more sophisticated understanding of behavior analysis, as well as improve critical thinking about behavior. Such exercises allow students to judge for themselves the validity of the criticisms of behavior analysis and to correct those that are wanting.

*Stimulus control.* Turning back to my main theme and to stimulus control, the general form of my argument regarding the context of reinforcement repeats itself here in similar ways. In this case, context (for instance, conditional discriminative stimuli [Sidman, 1986]) influences the function of stimuli as discriminative stimuli (Figure 3m). The historical context produces and accounts for variabil-

ity in discriminative stimulus control between individuals (Figure 3n) and within them over time (Figure 3o). No stimuli are inherently discriminative stimuli. Only context makes them so.

Conceptualized as such, context is not usefully conceived of as simply more or another source of stimulus control (any more than establishing operations, as context, are another source of "stimulus" control for reinforcement). Should what appears to be conditional stimulus control sometimes be no more than compound or complex discriminative stimulus control, then that is a matter of fact. But the context controlling the discriminating function of this compound discriminative stimulus still remains to be analyzed—all causes have contexts.

These behavioral relationships, and all that came before, now make for a busy and tortuous figure (Figure 3). With this, I conclude this section.

### *Conclusion*

In unpacking the context-dependent nature of the three-term contingency, we discover a unit of analysis that has not two, but three generic concepts. In unpacking context itself, we unpack the various ways in which it can affect behavior, both historically and currently, formally and functionally. If this is disturbingly complex, so it may be with the nature of behavior, though the final arbiter will, of course, be effective action. This brings me to two points.

*Context and contingencies.* First, in unpacking the three-term contingency to examine its context, I am not implicitly criticizing the dominant behavior-analytic concern with contingencies or arguing against their importance (see, e.g., Ferster & Skinner, 1957), nor am I suggesting that our unit of analysis inherently overlooks context. Skinner's (1977b) answer to related criticism bears repeating. When once chided for concentrating on contingencies, to the neglect of motivational variables (see Herrnstein, 1977), Skinner responded:

[I]s this not like saying that in spending so much time on hormones, the endocrinologists tell us they

think relatively little behavioral variance is explained by the nervous system? To stick with one field is not to assert that another field is worthless. (p. 1010)

Likewise, focusing on contingencies does not mean that context is worthless, that it explains little behavioral variance. Indeed, Skinner (1931) once noted that "the question of third variables is of extreme importance in the description of the behavior of intact organisms" (p. 455). Not making context explicit, however, might unintentionally invite misunderstanding and mischaracterization.

The disaffection of some behavior analysts and the requiems we hear for the three-term contingency (e.g., Russo, 1990), along with the emergence of animal cognition (Wasserman, 1981; contra Morris et al., 1982) and cognitive behavior therapy (Mahoney, 1974; Meichenbaum, 1977; contra Biedel & Turner, 1986), may be reactions against the presumed inadequacy of the three-term contingency in accounting for the wonderful variety and troublesome variability in behavior (just as creationism is a similar reaction to bare-bones Darwinism). Not seeing the historical or current contexts of behavior in the three-term contingency, some colleagues posit motivational constructs and processes such as self-efficacy to account for variability in reinforcement (Bandura, 1982; see Biglan, 1987). Likewise, they posit cognitive constructs and processes such as representation, storage, and mediation to account for variability in stimulus control (Hulse, Fowler, & Honig, 1978; see Morris et al., 1982). As mentioned earlier, even though such theorizing may be objectionable (Skinner, 1950, 1977a), we cannot deny the behavioral relationships to which these motivational and cognitive terms refer. These will need explaining in behavior-analytic terms. Perhaps if we unpack the three-term contingency—as we are beginning to do—we will find some useful ways of doing so.

*Linear versus systems analysis.* My second point is that because we have largely restricted our analyses of behavior to relationships within the three-term contingency (perhaps because these were the easiest variables to work with), we have



necessarily—and quite reasonably—sought to eliminate extraneous sources of behavioral variability. Given that these sources are largely the contextual determinants of behavior, then context is what we have controlled. Controlling variability by controlling context, for instance, controlling for species status, deprivation, and the physical environment, is fundamental to our scientific practices, and it should be (Johnston & Penny-packer, 1980; Sidman, 1960). Factors that contribute to behavioral variability cannot be allowed to confound the analysis of the contingencies we analyze. In holding context constant, though, we put aside its role as a controlling variable. And once it is put aside, we may forget that it is a controlling variable. Only those variables we investigate, for instance, reinforcement, may seem to be the causes of behavior.

In unpacking the three-term contingency, we have uncovered a third generic variable—context—and the different properties or functions it may have (Figure 3), all of them interrelated with behavior. If behavior is interrelated with them all and behavior requires them all, then no one of them is more important (or more equal) than any other of them, except on pragmatic grounds. Moreover, the effect of any one variable, such as reinforcement, is dependent on the form and function of all the others. That is, the effects of our independent variables are dependent on the presence and properties of all the other variables that participate in behavior. Conceptualized thusly, we have a behavioral field or system as our unit of analysis, a system of functional relations among factors (see Midgley & Morris, 1988; Moxley, 1987), not a sequence of contingencies structured linearly in time. Behavior, then, is organized, but also continuously reorganized—reorganized in the sense that the functions of each variable (interactant) change as a function of changes in the others.

This is a systems conceptualization of behavior that may, of course, be found in Skinner's work (see, e.g., Skinner, 1931, p. 446). He once commented, for instance, that such an orientation was

“helpful in thinking about the behavior of an organism as a whole” (Skinner, 1979, p. 101; see Krechevsky, 1939, pp. 406–407; Verplanck, 1954, p. 307). Other behavior analysts, too, have pursued analyses of behavior as a system (see Bernstein, 1983; Thompson & Zeiler, 1986) or from an ecobehavioral orientation (see Morris & Midgley, 1990).

If a systems perspective has been acknowledged within behavior analysis, then what of any economy to a fault in depicting the three-term contingency as our unit of analysis? Perhaps none for some purposes; perhaps everything for others. What of the virtues of the complex behemoth I have depicted in Figure 3 for purposes of explication? Perhaps none for some purposes; perhaps everything for others. I plead no special case for my behemoth; indeed, I recognize that it is not even a very pretty picture. Economy of style has a certain beauty and elegance in its simplicity.

Perhaps we should depict something intermediate, a generic behavioral field, for instance, as our unit of analysis for those occasions when the three-term contingency and my behemoth are not quite right. Pared down, one such unit might look like Figure 4: stimulus and response in interaction, transaction, context, and time. That this may resemble J. R. Kantor's (1959) interbehavioral field is, I am sure, mere coincidence—or perhaps not. (Perhaps having an office next to Barbara Etzel has taught me something about stimulus shaping and fading.)

The important point in this discussion is that selecting a unit of analysis is not a matter of selecting the one true unit. It is a matter of selecting the right unit for the right purposes, for the right occasion. It is a matter of selecting the right unit for the prediction and control of behavior—the behavior of organisms, of colleagues and critics, and even of ourselves. As for the last, how we depict our unit of analysis not only exerts stimulus control over the behavior of colleagues and students, but over our behavior as well. It controls, in part, the questions we ask of behavior and how we answer those questions. Like the level of magnification on a microscope, we should select a level

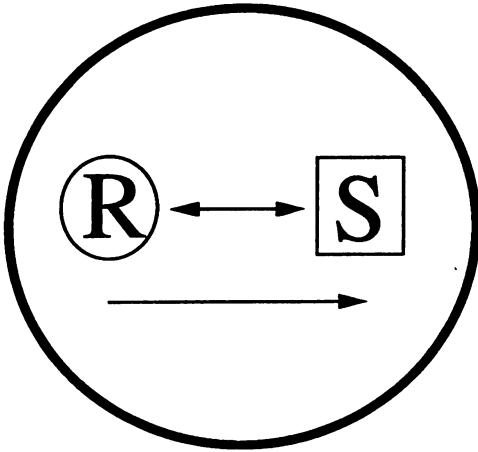


Figure 4. A contextual unit of analysis.

or a unit of analysis that best fits the character and scope of the subject matter that interests us (Morris et al., 1982).

### THE EVOLUTION OF BEHAVIOR ANALYSIS

I turn, finally, to Skinner's third contribution—radical behaviorism—which he offered in "The Operational Analysis of Psychological Terms" (Skinner, 1945, p. 294). Here, I examine the evolution of behavior analysis within the behavioral sciences. I am not concerned so much with the growth of behavior analysis as a body of basic knowledge (see Catania, 1992) or with its breadth of application (see Cooper, Heron, & Heward, 1987). Nor am I suggesting that behavior analysis is undergoing a paradigm shift, although there have been changes in the past 50 years (Coleman, 1984; Schraff, 1982). Rather, I want to point out that behavior analysis today represents one of the few perspectives in the behavioral sciences that is contextualistic in world view (Morris, 1988a, 1988b; see Rosnow & Georgoudi, 1986). The recent growth and change in behavior analysis is that its implicit contextualism is becoming explicit.

In being philosophical, I am not setting the conceptual analysis of behavior aside from (or against) the empirical analysis

of behavior, basic or applied. I do not think we can. Conceptual and empirical analyses are inextricably linked. My concern is with the effect of the cultural and scientific contingencies we call "verbal" or "philosophical" on what we understand about behavior. Just because we may be unaware of (or unable to tact) these contingencies does not mean that our behavior is unaffected by them. In suggesting that we become aware of them, I suppose I am suggesting that we engage in a little behavior-analytic consciousness-raising.

To illustrate what such consciousness-raising might entail, let me first offer some examples from applied behavior analysis. Applied behavior analysts, it seems to me, underwent some consciousness-raising in the late 1970s with respect to unhappy consumers and problems with the replicability of their programs. The consequence was increased systematic concern with the social validity of target behaviors, procedures, and effects (Wolf, 1978; see Schwartz & Baer, 1991). Also, from past to present, applied behavior analysts have confronted the limitations of possibly being technological to a fault (see Geller, 1991), self-consciously seeking a better balance in their work (see, e.g., Morris, 1991). Most recently, applied behavior analysts have had to address ethical issues in the treatment of "deviant" behavior (Johnston, 1991; Sidman, 1989). Addressing these issues self-consciously has made applied behavior analysts more ecologically and functionally oriented (see Favell & Reid, 1988; Horner et al., 1990; Morris & Midgley, 1990). The consequences of engaging these issues, and the consequences of those consequences, yield an ever-evolving, constructed applied consciousness (and conscience) about what constitutes effective and humane treatment.

In a like manner, the *philosophical* consciousness of behavior analysis—its philosophy of science—continues to evolve through the practice of asking such questions as: What are we doing as scientists, why, and how? This philosophical consciousness is also not given. It is constructed. It is the product of inter-

actions between us and our subject matter, between us and other behavior analysts, between us and the broader psychological and scientific community, and among us, our social community, and our culture as a whole. For present purposes, I focus my comments on the place of behavior analysis in the evolution of science and consider what our world view or philosophy might be. This is the consciousness-raising in which I am interested.

### *The Stages of Scientific Evolution*

The sciences are alike in that each is concerned with the world we live in. They are also at one in seeking an account of the behavior of nature, the behavior, for instance, of matter, genes, and organisms. The sciences vary, however, on several dimensions. They vary externally with respect to their subject matter—physics, biology, and psychology. They vary internally with respect to specific content domains, in psychology, for instance, the domains of perception, emotion, social behavior, personality, and cognition. And they vary with respect to their research methods, descriptive, correlational, and experimental.

Most important, perhaps, the sciences vary in the progress they have made in their evolution as sciences. In making this assessment, I borrow a metric from Einstein and Infeld's (1938/1961) description of the scientific evolution of physics, a metric adopted by others, notably Dewey and Bentley (1949) and Kantor (1946), for similar assessments of psychology (see also Pronko & Herman, 1982) (Table 3). I also bring these stages of scientific evolution in line with Pepper's (1942/1960) world hypotheses, doing Pepper the disservice of turning his "relatively adequate" world views orthogonally, from less to more relatively adequate.

These stages, of course, do not evolve neatly, clearly, or evenly. In Kuhn's (1962) view, they represent revolutions in the philosophy of science. The newer, more gradualist and evolutionary approaches to the philosophy of science are more benign (Laudan, 1977, 1984; see

TABLE 3

### Stages of scientific evolution

Physics	Biology	Psychology
Formism	Vitalism	Organicism
Mechanism	Mechanism	Mechanism
Field theory	Systems theory	Contextualism

Batts & Crawford, 1991), but they still suggest a philosophy of science red in tooth and claw—in a dogma eat dogma world.

This observation reminds me of the behavior-analytic poser: What is the difference between Republicans and Democrats? The answer: Republicans don't know about reinforcement; Democrats don't know about contingencies. I am no political scientist, but whatever contextualism looks like politically may be the new world order, with features in common with the broader "communitarian" structure Skinner (1948) depicted in *Walden Two* (see Horcones, 1982).

As for the stages of scientific evolution, the first stage—formism/vitalism/organicism—generally takes nature to be operating on its own, that is, on its own power, self-contained and actualized, essential and vitalistic. The divine right of kings, as it were. In the second stage—the mechanistic stage—nature is seen as controlled by independent physical forces operating on immutable dependent objects. The forces of capitalism run amuck. In the third stage—field and systems theory or contextualism—nature is described in terms of ever-changing dynamic fields or systems of factors or interactants, a synergistic global economy and ecology.

Physics and biology have largely progressed through these three stages, though mechanism is retained where it is effective enough—enough of the time (e.g., Newtonian mechanics). But as we progress from the subject matter of physics to that of biology and finally to that of psychology, the scope of mechanism's effectiveness seemingly declines. This leaves behavior analysis among a hand-

ful of often unsuspecting and unorganized allies at the forefront in the evolution of the behavioral and social sciences as a natural science, contextualistic in world view.

Within behavior analysis, its contextualism is exemplified, for instance, by basic research on molar analyses of behavior (Davison & McCarthy, 1988), by the relativity of reinforcement (Dunham, 1977), and by contextual variables, both historical (Barrett, 1986; Wanchisen, 1990; see, e.g., Johnson, Bickel, Higgins, & Morris, 1991) and current (Michael, 1982; Sidman, 1986; see, e.g., McPheron & Osborne, 1988). In applied research, contextualism is exemplified by current concerns with functional analysis (Carr & Durand, 1985) and setting factors (e.g., Wahler & Fox, 1981), and in ecobehavioral analysis more generally (Morris & Midgley, 1990). And, in the conceptual analysis of behavior, the call to contextualism has been explicit (see Hayes, Hayes, & Reese, 1988; Morris, 1988a)—contextualism is its world view.

Proposing that contextualism is the world view of behavior analysis may seem odd when we already have radical behaviorism as the “philosophy of the science of behavior” (Skinner, 1974, p. 3). However, such a statement—that radical behaviorism is the philosophy of the science of behavior—may be economical to a fault. It does not specify what that philosophy is (see Delprato & Midgley, in press). If someone were to ask, specifically, about the philosophy of radical behaviorism, what would we answer?

The uninformed answer is that behavior analysis is closely aligned with logical positivism and operationism, that it is objectivist, and that it is mechanistic (see, e.g., Mahoney, 1989; contra Catania, 1991). The more informed answer is that radical behaviorism generally assumes a pragmatic criterion of truth (Skinner, 1945; see Smith, 1986). Somewhat more specifically, it is a unique way of conceptualizing verbal behavior (Skinner, 1957; see MacCorquodale, 1969), private events (Skinner, 1945; see Moore, 1980), and the behavior of scientists (Skinner, 1956; see Smith, 1986). But these answers are

not a single coherent whole. Contextualism, however, is. Contextualism organizes these individual perspectives into a unified philosophical system, allowing us to discover uniformities, order confusion, and resolve puzzlement about our conceptual and empirical practices.

### *Contextualism and Mechanism*

In Pepper’s (1942/1960) analysis, the world views are rendered sensible in terms of their underlying root metaphors. In the organismic world view, the metaphor is the maturing, internally self-regulating organism. In mechanism, we have the familiar machine. In contextualism, the metaphor is the “historic event,” which is itself economical to a fault, so let me address a few particulars (see Morris, 1988a).

As a lead-in, though, let me observe that some behavior analysts do not disavow mechanism; indeed, they apparently embrace it. To the good, where mechanism is embraced, it is usually embraced in the sense that to be a mechanist is to take behavior as a suitable subject matter for the natural sciences:

The premise of *mechanism* is the assumption that explanations of natural phenomena must not refer to outside agents [demons or life forces]. This is what is meant by determinism in science. . . . Every scientist must be a mechanist. (Malone, 1990, p. 45; see also Zuriff, 1985, pp. 186–192)

There is no necessary harm in this sense of mechanism largely drawn from 19th century physiology (see Logue, 1988; Thompson, 1984).

To the bad, however, harm accrues from being mechanistic in world view. In the language game of philosophy and of the culture at large, mechanism means something more than naturalism, something different, something pernicious (see, e.g., Reese & Overton, 1970). It entails a metaphysic quite at odds with radical behaviorism in at least five ways (Morris, 1988a).

First, mechanism adheres to an associationism and to an elementarism in which putative basic immutable behavioral atoms have immutable properties. But this does not characterize contem-

porary behavior analysis and the dynamic nature of the three-term contingency (Branch, 1977). Responses and stimuli are not immutable; they are defined functionally as units or classes. Likewise, their functions are not inherent in their forms; stimuli and responses are constantly varying in their functions for one another.

Second, in mechanism, behavior change is little more than change in response weakness or strength. In contextualism, however, we speak of change in "acts," not in mere response forms (Lee, 1988). Behavior exists as a dynamic system or behavioral structure in context.

Third, mechanism assumes contiguous cause and effect, where behavior analysis focuses on functional relationships, interdependencies, or correlations among events (Skinner, 1935).

Fourth, mechanism views change as continuous in a linear, sequential sense, whereas behavior analysis has a discontinuous emergent quality. The continuously altered systems of interrelationships are qualitatively different over time, producing ever-emergent, constructed outcomes (Krapfl, 1977).

Fifth, mechanism takes responding to be inherently passive—dependent on stimuli. In contrast, in behavior analysis the operant is an active, lively unit of analysis in which the functions of stimuli and responses for one another are joint, interdependent, and ever-changing (Pronko & Herman, 1982).

This rendering of contextualism is, of course, economical to a fault. At best, it hints of important differences between mechanism and contextualism. At worst, it obfuscates. This material needs its own hearing, which is beginning to occur (see Hayes et al., 1988; Morris, 1988a, 1988b). For the present, I desist and close.

## CONCLUSION

I apologize if I have sounded ungracious: celebrating Skinner's three contributions to behavior analysis—prediction and control, the three-term contingency, and radical behaviorism—while at the same time criticizing our (and sometimes

his) economy of style. But the problem of economy is, I think, mainly one of style, not an inherent weakness or inadequacy in behavior analysis. We would be mistaken, however, if we were complacent about this and were to blame disaffected colleagues and critics for their every dissatisfaction or misunderstanding, especially where we have contributed to misrepresentation by being economical to a fault in the first place. We should also be careful not to become overly rigid about the political correctness of adhering to certain received ways of construing the aim, progress, and evolution of behavior analysis as though it were a thing and not a process. Finally, to prejudge the truth about how the aim of behavior analysis must be described, how the unit of analysis must be depicted, or how our world view must be conceptualized is the antithesis of behavior analysis. It is a fatal epistemology, not an empirical one; it would yield only an arrogant, not a humble behaviorism. The ironic error would be in not being behavior-analytic enough.

This brings me to perhaps Skinner's most important contribution of all, one I have tried to interweave throughout—his empirical epistemology. We should experiment—experiment not only with behavior as our subject matter, but also with our science, with science as the behavior of scientists, with our teaching of that science, and with our presentation of that science to students, colleagues, and the culture at large.

This may sound heretical because science is supposed to be about Truth with a capital "T," but not on a behavior-analytic account. On a behavior-analytic account, there is no Truth with a capital "T," only little truths with small t's—if there are any "truths" at all. And even these truths are true only in the sense that they are the consequences of effective empirical and conceptual action, where effectiveness is "provisional"—always in context. The empirical epistemology of behavior analysis is what balances humbleness with assertiveness; without an empirical epistemology, assertiveness is but arrogance.

Skinner, of course, said this first, many times and in many ways.<sup>1</sup> The urgency of an experimental attitude is perhaps no better gleaned in his work than in his analysis of cultural practices and cultural survival (Skinner, 1953, pp. 415–449, 1971). I make over one of his cogent observations into a comment about scientific practices and the survival of a science of behavior. In exchanging some of Skinner's (1953) words for mine, his observation would read as follows:

Since a science of behavior is concerned with demonstrating the consequences of [not cultural, but scientific] practices, we have some reason for believing that such a science [of behavior—one that is concerned with demonstrating the consequences of scientific practice—] will be an essential mark of the [science or sciences] which survive. The current [science] which, on this score alone, is most likely to survive is, therefore, that in which the methods of science are most effectively applied to the problems of [scientific] behavior. (p. 446)

These are the conditions under which behavior analysis is most likely to flourish, both externally and internally. Historical precedence, the weight of tradition, and unnecessary political correctness about behavior analysis can only hinder progress, though we cannot know even that unless we experiment. Let me put this more positively: Why wait? Why wait to see how the science of behavior may develop? Why not seek an understanding of its development by engaging the variables of which it may be a function? Why not enhance the development of behavior analysis to its full potential (see Baer, 1973)?

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<sup>1</sup> My locution—"Skinner said it first" throughout this paper—owes much to Jay Moore (1987), who borrowed something like it from Bill Verplanck (1983), who was referring to the contributions J. R. Kantor made to his own thinking, acknowledging that "Kantor's always been there first" (p. xxv). Moore (1987), in turn, was referring to how Kantor's scientific epistemology anticipated Skinner's. I acknowledge equally the contributions of both Skinner and Kantor to the present paper, but the occasion of its delivery was to honor Skinner, so I placed my emphasis on Skinner's work—and apologize to interbehaviorists everywhere. The title of this address, though, is implicit acknowledgment of Kantor's contribution to its content (see Kantor, 1946).

If Skinner were right about this—about taking an experimental approach (and I think that he was)—then we might alter some of the ways we construe the aim, progress, and evolution of behavior analysis. In the process of so doing, we might uncover some important commonalities with like-minded (I say idiomatically) colleagues in the other sciences and in the humanities. Many of the papers delivered at the 1991 ABA convention suggested commonality and overlap with, for instance, linguistics (Andresen, 1991; see Andresen, 1990), rhetoric (Czubaroff, 1991; see Czubaroff, 1988), analytic philosophy (Mason, 1991; see Day, 1969), philosophical pragmatism (Kemp, 1991; see Smith, 1986), developmental systems (Midgley & Morris, 1991; see Oyama, 1985), and cultural materialism (Harris, 1991; see Lloyd, 1985).

Perhaps with such alliances, and through them, we are entering into a new era, perhaps a new intellectual world order. As such, perhaps we should also take advantage of the coming sociological changes in the organization of psychology departments (e.g., cognitive psychology is moving to biology and computer science; see Scott, 1991) and gain a larger niche. Or perhaps we should simply assert the emergence of the "new behaviorism" or the "new look in behaviorism"—a sociology-of-science thing to do. Such moves have been effective elsewhere in promoting rapid change or rapid acknowledgment of change, for instance, in sociobiology (A. C. Catania, 1991, personal communication). Why not in behavior analysis? *Carpe diem*.

Although Skinner is no longer present to guide us toward this future, his legacy lives on through us. We are the locus for a confluence of variables to which his contributions are paramount. These contributions, Skinner's legacy, are not yet fully unpacked—they never will be—making the future full of excitement, full of potential. As we continue with this difficult and exciting work, we will further enrich our understanding of our subject matter and our science. We will illustrate to colleagues in the sciences and humanities that behavior analysis is richer and deeper than commonly ap-

preciated (or articulated). And we will expand (and correct) the views of a culture that largely appreciates our superficialities (e.g., ping-pong playing pigeons), not the sum and substance of our science or its social and cultural implications.

A wise man once told me, though, that potential means you have not done it yet. (It also has a shelf life.) This is true: We have not achieved our full potential, but we are doing so, step by step. The emerging contextual themes common across the three branches of behavior analysis affirm the validity of those changes within each of them. These changes, in turn, are strengthening, and being strengthened by, their integration with one another (see Epling & Pierce, 1983, 1986). These changes are also contributing to behavior analysis as a whole in ways greater than the sum of the changes in the separate branches. But we can accelerate this process and potential, our growth, and our evolution by self-consciously unpacking Skinner's contributions, his legacy. We can do so—and we must—for our ourselves and our critics, but most importantly for the world.

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