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Behavior Analysis and Mechanism: One Is Not the Other

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Behavior analysts have been called mechanists, and behavior analysis is said to be mechanistic; that is, they are claimed to be aligned with the philosophy of mechanism. What this means is analyzed by (a) examining standard and specialized dictionary and encyclopedia definitions and descriptions of *mechanism* and its cognates and (b) reviewing contemporary representations of the mechanistic worldview in the literature on the philosophy of psychology. Although the term *mechanism* and its cognates are sometimes an honorific (e.g., "natural science"), their standard meanings, usages, and functions in society, science, psychology, and philosophy do not aptly characterize the discipline. These terms mischaracterize how behavior analysts conceptualize (a) the behavior of their subjects and the individuals with whom they work and (b) their own behavior as scientists. Discussion is interwoven throughout about the nature of terms and definitions in science.

Key words: mechanism, reductionism, elementarism, S–R psychology, definitions

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For the past several years, I have thought off and on about preparing some version of this paper, for two reasons—one personal and one professional. First, I have sometimes had difficulty articulating for myself the structure of radical behaviorism as a systematic philosophical position (Morris, 1992a; cf. Delprato & Midgley, 1992). I understand its basic assumptions about behavior being a subject matter for the natural sciences (Skinner, 1938, 1966) and I have a grasp of its unique approach to private events (Skinner, 1945; see Moore, 1980), verbal behavior (Skinner, 1957; see MacCorquodale, 1969), and the behavior of scientists (Skinner, 1956, 1957, pp. 418–431; see Smith, 1986, pp. 257–297). My difficulty lies in not always being able to organize these (and other) parts into one philosophical whole. In other words, although I realize that radical behaviorism is, as Skinner put it, the philosophy of the science of behavior (Skinner, 1974, p. 3), I am not always sure just what that philosophy is, how it relates to other conceptual systems, or what more encompassing worldview it might be a variety of, if any. This has produced some personal angst.

I have tried, at times, to relieve this angst by focusing on what radical behaviorism is not, for that has been an easier task. One thing radical behaviorism is not, I have observed, is mechanistic in worldview (Morris, 1982, 1988a, 1992a). This, though, has produced some professional angst: Close behavior-analytic mentors, colleagues, and friends (among them, notably, Don Baer and Jack Michael) have asserted that they are mechanists and that this is surely well and good. My response has been that such talk leaves us open to meanings of *mechanism* that are not in accord with behavior analysis, but I never got around to particulars. Getting to the particulars is the second reason for this paper.

These reasons notwithstanding, the paper went unwritten for lack of impetus until Jack Gewirtz summoned me to participate in his Development and Behavior Analysis Special Interest Group's program at the May, 1992, meetings of the Association for Behavior Analysis. What I had not expected was for Jack to place me on a symposium with—or as it seemed, unhappily, against—one of my scientific idols, Jack Marr. Perhaps you can imagine why I felt (and feel) as though I were dangling naked in the intellectual wind, where a person can develop a bad, bad cold.

This paper is based on a presentation delivered at the 1992 meeting of the Association for Behavior Analysis entitled "What It Would Be Like to Be a Mechanist" (Morris, 1992b).

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In what follows, I describe what I think it would be like to be a mechanist or to be mechanistic. First, I offer an interpretation of what some behavior analysts may mean when they say they are mechanists—interpretation, of course, being an ever-ticklish undertaking. Second, I examine the definitions of *mechanist* and its cognates from our larger social, scientific, and psychological verbal communities. Third, I consider the implications of mechanism as a worldview for understanding the behavior of "the other one," that is, the behavior of our research subjects and the individuals with whom we work. And fourth, I do the same with respect to the behavior of "the one," that is, with respect to our behavior as scientists. Throughout, I interweave commentary on the nature of terms and their definitions.

In taking up these issues, I could be more scholarly than I will be. I could, for instance, inquire into the meanings of mechanism in the history of ideas and the philosophy of science, or in the history and philosophy of psychology. I leave that task to others such as Mecca Chiesa (1992) and Roy Moxley (1992), both of whom have argued cogently about the nonmechanistic lineage of behavior analysis in the special issue of the *American Psychologist* in honor of B. F. Skinner, entitled "Reflections on B. F. Skinner and Psychology." For myself, I look into some contemporary meanings of mechanism in dictionaries and encyclopedias, and into current depictions of the mechanistic worldview in relation to behavior analysis. But let me begin at the beginning, where it is always appropriate to begin, by inquiring into the meaning of mechanism and its cognates within behavior analysis.

A BEHAVIOR-ANALYTIC MEANING OF *MECHANISTIC*

In his book, *Behaviorism: A Conceptual Reconstruction*, Gerald Zuriff (1985, pp. 186–192) listed nine meanings of mechanism and mechanistic, all of them supposedly critical objections to behaviorism. Some of these objections are wrong-headed or irrelevant, but Zuriff's first-listed meaning is a meaning-by-exclusion with which behavior analysts can presumably agree. Zuriff wrote:

> Often the "mechanical" objection means that behaviorists regard behavior as occurring without the causal intervention of consciousness, spirit, or a soul. In this sense, the allegation is correct. (p. 186)

In one sense, then, to be a mechanist is to deny vital forces and transcendental minds. In Skinner's (1938) words, to be a mechanist is "to be free from the intervention of any capricious agent" (p. 433). Put more positively, to be a mechanist is to embrace naturalism.

This sense of mechanistic is commonly associated with a second sense. As John Malone (1990) defines *mechanism* in the glossary of his book, *Theories of Learning: A Historical Approach*, mechanism is the "assumption that explanations must not refer to outside agents, such as demons or life forces" (p. 53). Put another way, explanations of natural phenomena must not refer to supernatural agents, that is, to agents outside of nature. As Malone (1990) notes, "This is what is meant by determinism in science" (p. 45). This sense of mechanism is the assumption that one's scientific subject matter (behavior, for instance) is lawful and orderly. Given this meaning, Malone (1990) concludes, "Every scientist must be a mechanist" (p. 45).

Skinner (1938), of course, said this first about his book, *The Behavior of Organisms*:

> The work is "mechanistic" in the sense of implying a fundamental lawfulness or order in the behavior of organisms . . . ; it is assumed that behavior is predictable from a knowledge of relevant variables and is free from the intervention of any capricious agent. (p. 433)

And he said this later, as well, in *Beyond Freedom and Dignity* (Skinner, 1971):

> Man is not made into a machine by analyzing his behavior in mechanical terms. . . . Man is a machine in the sense that he is a complex system behaving in lawful ways. (p. 202; for more on mechanism and mechanistic, see Skinner, 1969, pp. 294–295; 1974, pp. 122, 245, 262)

At other times, Skinner was more cautious, and, I think, appropriately so. For

instance, in reprinting a passage from a letter sent to him by the physicist-philosopher Percy Bridgman about Skinner's text, *Science and Human Behavior* (Skinner, 1953), Skinner (1983) wrote that Bridgman had caught him on the following point. In Bridgman's words:

> And I would not like to say, as seems implied, that science has to assume that the universe is lawful and determined, but rather that science proceeds by exploiting those lawfulnesses that it can discover. Anything smacking of faith I think we can get along without. (p. 60)

In any event, defined this way, and in the others above, mechanism has been highly productive in the life sciences, as it will continue to be. In biology, Claude Bernard's (1865/1949) program of experimental physiology triumphed over vitalism (see Thompson, 1984). In evolution, Charles Darwin's (1859, 1871) theory of natural selection has largely won out against creationism (see Dawkins, 1986). And in psychology, some form of behaviorism will, I think, eventually replace mentalism (Watson, 1913; see Skinner, 1990). In each case, natural science has been (or will be) ascendant. If by mechanism we mean natural science, then behavior analysis is mechanistic. If this is what it would be like to be a mechanist, then to be a mechanist is about the finest thing a behavior analyst can be.

But to be a natural scientist is not as unambiguous as we might think. Natural science can be conducted in ways such that to be a mechanist is not what it would be like to be a behavior analyst. For instance, Henry Pronko (1969) defines *mechanist* as:

> The view or doctrine that all human activities can be fully explained in terms of the principles of physical mechanics. A reductionist. *See reductionism.* (p. 488)

Reductionism, in turn, is defined as:

> A general view taken by some scientists which holds that all complex phenomena are ultimately explained and understood by analyzing them into increasingly simpler and supposedly more elementary components. Under reductionism, psychology is reduced to physiology, physiology to chemistry, chemistry to physics, etc. (Pronko, 1969, p. 497)

In the next section, I examine the possibility that the term *mechanist* and its cognates may generally have these latter meanings—meanings that distort and misrepresent behavior analysis.

OTHER MEANINGS OF *MECHANISTIC*

Ordinary Language Dictionaries

First, what meanings might *mechanistic* have in ordinary language use? For this, I turn to English language dictionaries. One entry under *mechanism* in my *American Heritage Dictionary* (1976) includes two subdefinitions. First, we find "a. The automatic and consistent response of an organism to various stimuli" and "b. A habitual manner of acting to achieve some end" (p. 780). Here, we have, first, a stimulus–response (S–R) psychology, which is not behavior analysis (Skinner, 1988, p. 460; see Moxley, 1987), and second, purpose, which is something akin to a quality of behavior but not a cause of behavior (Skinner, 1974, 1989; see Lee, 1988), but this is not the issue at hand, at the moment.

In another entry under *mechanism*, we find a more pertinent definition: "The doctrine that all natural phenomena are explicable by material causes and mechanical principles" (p. 780; see also the *Oxford Encyclopedic English Dictionary*, 1991, p. 900). Finally, under *mechanistic*, we come across: "Of or pertaining to the philosophy of mechanism, esp. tending to explain phenomena only by reference to physical or biological causes" (p. 780).

My *Webster's Ninth New Collegiate Dictionary* (1987) lists definitions in accord with these. It defines *mechanism* (first usage, 1662), for instance, as "a doctrine that holds natural processes (as of life) to be mechanically determined and capable of complete explanation by the laws of physics and chemistry" (p. 737).

We may agree that the behavior-analytic subject matter—depicted, for instance, as a three-term contingency among stimuli and responses in context—is composed of physical, chemical, and biological material, and that behav-

ior analysis is thereby materialistic in this sense. But this does not mean that behavior is merely these materials, themselves and only. Nor does it mean that behavior is determined or explained by the same laws that govern the behavior of these materials in their own sciences. This kind of reductionism is anathema to behavior analysis (Kantor, 1969; Skinner, 1938; see Smith, 1986, pp. 257–297).

Dictionaries of Philosophy

That mechanism means much the same thing in philosophy is supported by other standard definitions, such as that found in D. D. Runes's (1983) *Dictionary of Philosophy*:

> Theory that all phenomena are totally explicable on mechanical principles. The view that all phenomena are the result of matter in motion and can be explained by its law. Theory of total explanation by efficient, as opposed to final cause. . . . In general, the view that nature consists merely of material in motion, and that it operates automatically. Opposite of: all forms of super-naturalism. See also *Materialism, Atomism.* (p. 210)

To the good, Runes states that mechanism is opposed to final causes, that is, to teleology, and to the supernatural, with which behavior analysts would agree (see Day, 1980). To this I would add that mechanism and behavior analysis are also both opposed to dualism, idealism, mentalism, and vitalism (see Reber, 1985, p. 436).

But Runes also notes that mechanism implies atomism and "applies to associational psychology." In atomism, behavior is presumed to be composed of basic building blocks—fundamental stimulus and response elements identifiable a priori on the basis of their form, structure, or topography. In associational psychology, what is associated are these atoms or elements. Complex behavior is then but the compounding of these basic essences.

Behavior analysis, however, is not atomistic, elementaristic, associationistic, or essentialistic in these senses (see Branch, 1977; Lee, 1988; Palmer & Donahoe, 1992). As Willard Day (1969a) pointed out:

> [The radical behaviorist] is suspicious of . . . a facile determinism, which views the aim of research as isolating the fundamental elements of nature which are thought of as existing in some kind of [Newtonian] mechanical interrelationship. . . . In particular, he objects to speaking of the events associated in a functional relationship as if they were things and objects having a more or less permanent identity as real elements of nature. (p. 319)

But again, Skinner (1938) said this first, with respect to his book, *The Behavior of Organisms*:

> The work . . . is not necessarily mechanistic in the sense of reducing the phenomena of behavior ultimately to the movement of particles, since no such reduction is made or considered essential. (p. 433; see Skinner, 1935)

Behavior analysis is about function and process, not about form and structure (Moxley, 1992). Behavior analysis takes as its subject matter what we describe in the grammar of verbs—behaving, constructing, and thinking—not what we describe in the grammar of psychological nouns—behavior, constructs, and cognition (see Lee, 1988). On the whole, then, behavior analysis is not mechanistic by Runes's definition.

Dictionaries of Behavioral Science

Moving on to the behavioral sciences, we find material closer to psychology. B. B. Wolman's (1989) *Dictionary of Behavioral Science*, for instance, describes *mechanistic theory* as:

> The doctrine that all aspects of the universe including organisms and their psychological processes can be explained in terms of mechanical laws. Free will, motivation, and purpose are denied as important variables in attaining ends. (p. 211)

Free will and purpose are denied as causes in behavior analysis, as well: They are inside agents and mental way-stations (Skinner, 1974, 1977). So, by part of this definition, behavior analysis *is* mechanistic.

As an aside, I would note that although free will and purpose are denied as causes of behavior in behavior analysis, they are not denied as effects. This is an important distinction (see Skinner, 1989). Early on, Skinner (1938) noted that, "The operant field corresponds closely with what has

traditionally been called 'voluntary' behavior" (p. 112). Later, and often, he commented, for instance, that "operant behavior is the very field of purpose and intention" (e.g., Skinner, 1974, p. 61; see also Skinner, 1953, pp. 87–90).

When construed as causes of behavior, free will and purpose are, as the philosopher Gilbert Ryle (1949) pointed out, in *The Concept of Mind,* "category mistakes." The category mistake, in this case, is to treat a term from the category of concepts describing *qualities* of behavior (e.g., behaving purposively) as if it were a term from the category of concepts describing the *causes* of behavior (e.g., behaving purposefully). From a behavior-analytic perspective, free will and purpose do not describe causes—as in, for instance, Bryan and Pete *purposefully* have lunch on Fridays at Sweetgrass (i.e., a consequence of a purpose). Rather, free will and purpose are terms that describe qualities of behavior or behavioral dispositions in current and historical context—as in Bryan and Pete *purposively* have lunch on Fridays at Sweetgrass (i.e., a quality of their behavior). As dispositional concepts (not explanatory concepts), free will and purpose are amenable to analysis and synthesis (see, e.g., Epstein, 1984). That is, they are amenable to description, prediction, and control as effects of contingencies and context, but not as causes.

Returning to Wolman's (1989) definition, behavior analysis is not mechanistic because he states that, in mechanism, motivation plays no explanatory role. For Wolman, a psychology composed of only stimuli and responses would be mechanistic because it would lack motivational variables—variables that explain, for instance, the dynamic relations among stimuli and responses. Behavior analysis, though, does not exclude such variables. Quite to the contrary: Behavior analysis explicitly includes variables that are, in part, motivational in an explanatory sense, for instance, establishing operations (Michael, 1982) and conditioning history (Wanchisen, 1990; see also Millenson & Leslie, 1979, pp. 391–412; Nevin & Reynolds, 1973; cf. Morris, 1992a). To the degree that behavior analysis includes such variables, Wolman's definition of *mechanistic* is not an apt characterization of our discipline (see also Baum, 1974, on other mischaracterizations of behavior analysis in Wolman's, 1973, first edition).

Finally, we should note that behavior analysis is sometimes said to be mechanistic because it is an S–R psychology and, as an S–R psychology, it admits only to one behavioral process—reflex or respondent conditioning (see, e.g., Harré & Secord, 1973; Taylor, 1964). If this were so, then behavior analysis might well be mechanistic (Ringen, 1976). But it is not: Behavior analysis is focally concerned with operant behavior, that is, with behavior selected by its consequences, not behavior elicited by antecedent stimuli (Skinner, 1974).

Dictionaries of Psychology

Turning at last to dictionaries of psychology, and to the classic of them all—English and English's (1958) *A Comprehensive Dictionary of Psychological and Psychoanalytic Terms*—we find *mechanistic theory* defined as

> the philosophical doctrine that all activities of living beings are completely explicable in terms of the laws of physical mechanics—i.e., in terms of the motions of particles in space-time, or in terms of the kinds of energy interchange known to physics. According to this view, no new or distinctive principles beyond those of physics are required for the explanation of *vital* and psychological phenomena. (p. 313; emphasis in the original)

Similar definitions may be found in more recent dictionaries of psychology (see Chaplin, 1985; Harré & Lamb, 1983), even one said to be among the most useful (Catania, 1989, p. 193)—Reber's (1985) *Penguin Dictionary of Psychology.* Reber notes, for example, that mechanism "implicitly assumes the possibility of *reductionism* to basic principles of physics and physiology" (p. 426; emphasis in the original).

These definitions return us to the elementarism, associationism, and reductionism described in the dictionaries of

ordinary language and philosophical use. If this is what it would be like to be a mechanist, then behavior analysis is not mechanistic.

Conclusions

Meaning and definition. What are we to make of these meanings and definitions? They seem at odds, to be sure, with what *mechanism, mechanist,* and *mechanistic* mean to some of my behavior-analytic mentors, colleagues, and friends. For them, these terms seem to have a natural science meaning, but that meaning does not rule out denotations and connotations that are antithetical to behavior analysis. As for the antithetical meanings, we might argue that they are somehow wrong, that they belong to disciplines other than behavior analysis, that they no longer comport with more modern meanings of mechanism, or that I have simply set up a straw person—but I do not think so. These points are tangential.

On a behavior-analytic account of meaning and definition, two points stand out. First, we should recognize, as Skinner (1957) pointed out, that "dictionaries do not give meanings; at best they give words having the same meaning" (p. 9). What we know from dictionary definitions of mechanism, then, are other words and phrases that have the same meaning. The second, more central issue is: Where are we to find the meaning of words? The answer: In the variables of which verbal behavior is a function. As Skinner (1945) observed in his paper on operationism:

> Meanings, contents, and references are to be found among the determiners, not among the properties, of response. The question "What is length [or what is mechanistic]?" would appear to be satisfactorily answered by listing the circumstances under which the response "length" [or "mechanistic"] is emitted (or, better, by giving some general description of such circumstances [e.g., a definition]). (p. 271; see also Skinner, 1957, pp. 13–14)

Catania (1989) has expanded on this point, noting that

> Among behavior analysts, the authority for a definition comes not from priority of usage but rather from the consistencies that develop between particular usages and the discriminations of behavior upon which they are based. In other words, some definitions work better than others, in the sense that they more effectively help us to see important properties of the behavior that we study. (p. 194)

As for what Catania refers to as the consistencies between (a) particular usages of terms and (b) the discriminations of behavior on which they are based, the entries under *mechanism* and *mechanistic* in the dictionaries of society, science, and psychology are remarkably consistent.

Put behavior-analytically, the consistency of the dictionary meanings and definitions reflects the reinforcement contingencies of our verbal community. These contingencies establish and maintain correlations between (a) certain conceptual, analytic, and explanatory practices and (b) the terms *mechanism, mechanist,* and *mechanistic.* In behavior analysis, however, the correlation between our practices and these terms is low. In other words, whereas linguistic philosophers would argue that these meanings and definitions reflect ordinary-language use and function (Wittgenstein, 1953, 1958; see Costall, 1980; Day, 1969b; Deitz & Arrington, 1984), ordinary-language use and function are largely not behavior-analytic use and function. Finally, as for whether these definitions effectively help us to see important properties of the behavior we study or of behavior analysis as a discipline, I think not. On the criteria of consistency and helpfulness, then, the language of mechanism seems a far cry from being applicable to or useful in behavior analysis.

Not finding these meanings and definitions of *mechanism* and its cognates apt characterizations of behavior and behavior analysis, we could (if we wanted) try a different approach and assign *mechanistic* a definition of our own—a definition that *would* aptly characterize behavior analysis. We could try, but I am not sure how effective that would be. First, our ordinary language, the languages of other disciplines, and dictionary definitions are not much amenable to behavior modification of this sort. Second, borrowing terms from the vernacular, from other sciences, and from psychology, and

making them into technical terms for our own private language is fraught with demonstrable problems, as we have unhappily discovered with terms such as *punishment, control,* and *language* (see, e.g., Deitz & Arrington, 1983). What these terms mean in behavior analysis is not all and only what they mean, denote, and connote in the vernacular (see Deitz & Arrington, 1983; Lee, 1981; Morris, 1992a). This has caused controversy and has contributed to conceptual confusion, especially when vernacular meanings possibly remain tacit among at least some behavior analysts.

Vocabulary and language. The meanings of mechanism I have described are not only part of the language we speak, but are also implicitly embedded in our behavior-analytic vocabulary and in our ordinary-language grammar and syntax. As for our behavior-analytic locutions, Skinner adopted a scientific language from his mechanistic counterparts. He borrowed Ivan Pavlov's and Clark Hull's language of stimuli and responses. In so doing, he invited mechanistic interpretations of his own experimental and conceptual programs, because Pavlov and Hull were mechanistic (see Verplanck, 1954).

As for ordinary-language grammar and syntax, the linguists Edward Sapir and Benjamin Whorf pointed out that a language's grammatical structure influences how those who speak that language think (Whorf, 1956). More specifically, the language Whorf called "Standard Average European" implicitly imparts a mechanistic character to our view of behavior (Hackenberg, 1988; Hineline, 1980, 1992; Williams, 1986; see also Shotter, 1986). Our grammar and syntax imply that simple, one-way cause-and-effect relations exist between agents and their actions (e.g., "Adonis canned a three-pointer"), organisms and their behavior (e.g., "Clay pecked the disk in the operant chamber"), and even between responses and their stimuli (e.g., "The response produced a reinforcer"). Our grammar and syntax predispose and condition us to segment the behavioral stream into a priori elements; into a priori agents and actions, grammatical subjects and predicates, and even into a priori responses and stimuli. Our grammar and syntax prepare and shape us to separate the knower from the known, the organism from the environment, and even response function from stimulus function. In other words, the structure of our language influences how we think about behavior in ways that are incompatible with the nature of behavior. As Phil Hineline (1980) has observed, "English grammar and syntax are fundamentally mismatched with the phenomena that constitute psychology" (p. 80).

SOME IMPLICATIONS OF MECHANISM FOR "THE OTHER ONE"

Having examined what mechanism means in behavior-analytic and dictionary talk, and found some good but more wanting, I turn now to what philosophically minded psychologists mean when they speak of mechanism in characterizing and criticizing behavior analysis. Here, I describe what it would be like to be a mechanist in the philosophical literature on "world hypotheses" as those hypotheses were introduced by Stephen C. Pepper (1942, pp. 186–231) and as they are now instantiated in the contemporary literature on the philosophy of psychology (see, e.g., Cohen & Siegel, 1991; Lerner, 1983; Rosnow & Georgoudi, 1986). My concerns here are with the implications of mechanism for "the other one," for our interactions with the behavior of our subjects and the individuals with whom we work.

What I describe is organized around issues addressed by Hayne Reese and Willis Overton in their renderings of Pepper's work (Overton & Reese, 1973; Reese & Overton, 1970; see Morris, 1988a). I select Reese and Overton's analysis because it is authoritative in four important ways. First, it was seminal in the developmental literature on "worldviews" (see, e.g., Horowitz, 1987; Sameroff, 1983) and is cited as such in related cognitive, social, and personality literatures (see Ros-

now & Georgoudi, 1986). Second, Reese and Overton's descriptions of the mechanistic worldview remain definitive in these literatures (see, e.g., Cohen & Siegel, 1991; Houts, 1991). Third, when behavior analysis is said to be mechanistic, Reese and Overton (1970; Overton & Reese, 1973) are often cited in support (see, e.g., Houts, 1991; Lerner & Tubman, 1991). Fourth, Reese and Overton's meaning of mechanistic closely parallels the meanings found in the standard (and erroneous) critiques of behavior analysis, such as those by Margaret Boden (1972), Kenneth Bowers (1973), Noam Chomsky (1959), and more recently by Michael Mahoney (1989; see also McNeil, 1970; Taylor, 1964, 1970a, 1970b).

Turning to the content of their analyses, Reese and Overton compared and contrasted the organismic and mechanistic worldviews in terms of what they called "corollary model issues." Their exemplars of organicism were Jean Piaget's (1960) cognitive-developmental and Erik Erikson's (1950) psycho-social theories of development. Their exemplar of mechanism was behaviorism in general. Here, they cited O. Hobart Mowrer (1960) and Robert Sears (Sears, Rau, & Alpert, 1965), as well as Sid Bijou and Don Baer's behavior analysis of child development (Baer, 1966; Bijou & Baer, 1961, 1963; see also Baer, 1970).

A point worth considering at this juncture is Reese and Overton's conflating of behavior analysis with philosophical behaviorism and S–R psychology. This alone should give us pause in referring to behavior analysis as mechanistic. The early mechanistic formulations of René Descartes (1637/1972) and Julien Offray de La Mettrie (1748/1912) and the later S–R psychologies of Pavlov (1927), Watson (1924), and Hull (1952) are at such a conceptual distance from behavior analysis that to be equated with them is to be sorely misunderstood and misrepresented (see, e.g., Harré & Secord, 1973; Mahoney, 1989; Taylor, 1964; contra Catania, 1991; Morris, 1990; Moxley, 1984; Ringen, 1976).[1]

Returning to the corollary model issues, these fall into two categories—structural and functional. Structural model issues pertain to a worldview's basic assumptions. Among those addressed by Reese and Overton (1970, pp. 133–145; Overton & Reese, 1973, pp. 71–73) were (a) elementarism versus holism, (b) antecedent–consequent versus structure–function, (c) behavioral change versus structural change, and (d) continuity versus discontinuity. The first of each pair was attributed to mechanism, the second to organicism.

As for the functional model issues, they pertain to the types of explanations acceptable within a given worldview. With respect to mechanism and organicism, these are, respectively, (a) unidirectional versus reciprocal causality and (b) linear causality versus organized complexity (see Overton & Reese, 1973, pp. 74–86).

Taking up all six issues in detail is beyond the scope of the present paper (and my acumen), so I focus on the first structural model issue—elementarism versus holism—and touch but passingly on the others (see Morris, 1988a, for a somewhat fuller treatment).

Structural Model Issues

Elementarism versus holism. What it would be like to be a mechanist is perhaps clearest in the dichotomy between elementarism and holism. Here, to be a mechanist is to adhere to elementarism

[1] In fairness to Reese, I should point out that he later distinguished between (a) adopting a mechanistic worldview as a model for the behavior of the organisms we study (our subject matter) and (b) adopting contextualism as a model for our behavior as scientists (our epistemology) (Reese, 1982; see also Moxley, 1987). Later still, he stated that the behaviorism he described as mechanistic in 1970 was "Watsonian behaviorism," not "Skinnerian behaviorism" (Reese, 1986). The philosophy underlying Skinnerian behaviorism, he wrote, was the pragmatism of Peirce (1940), Dewey (1896), and James (1907), which later "thickened" into the worldview Pepper (1942) called contextualism. These disclaimers notwithstanding, Bijou and Baer's (1961, 1963) behavior analysis of child development *was* an exemplar of the mechanistic worldview in Reese and Overton (1970; Overton & Reese, 1973).

(see Overton & Reese, 1973, p. 71; Reese & Overton, 1970, pp. 136–137), which is akin to the atomism, associationism, and essentialism described earlier. Elementarism holds that behavior and environment (e.g., my checking my departmental mailbox) are but a concatenation of more elementary stimuli and responses. Construed this way, behavior is analyzed by identifying its presumably more basic elements or units (e.g., the turns and steps I take down the hall towards the mailroom or, perhaps, something still more elementary in my nervous system).

The identification of these response and stimulus elements is formal: They are defined on the basis of their physiological or physical form. In this view, topographically identical responses and stimuli (the turns and steps taken toward the mailroom and my mailbox's sensory elements) are presumed to have the same function or meaning, no matter when, where, or under what circumstances they occur. An example from Reese and Overton (1970, p. 136) speaks perhaps more clearly to this point: Elementarism assumes that all upturned lips reflect "the establishment of positive social contact," not that the "smiling response" could also mean gastrointestinal disturbance or nervousness.

This is what it would be like to be a mechanist, but it does not seem like behavior analysis to me (see Bijou, 1979). Behavior analysis is not elementaristic in these senses for at least two reasons (see Branch, 1977). First, checking my mailbox is likely to be an operant unto itself, functionally related to the contingencies with which it covaries (see Skinner, 1935). These contingencies might comprise, for instance, a class of consequences including the receipt of professional correspondence, books and book catalogs, and even the occasional reprint request. In behavior analysis, the unit of behavior is an empirically derived one, not an essentialistic one (Palmer & Donahoe, 1992). Checking my mailbox would lose its meaning if reduced to supposedly independent and more elementary S–R units (see Lee, 1988). As Skinner noted,

> As it stands, I'm not sure that response is a very useful concept. Behavior is very fluid; it isn't made up of a lot of little responses packed together. (Evans, 1968, pp. 20–21)

The second reason behavior analysis is not elementaristic in the aforementioned senses is that behavior analysis does not (or should not) define behavior formally or topographically, but instead, functionally (Skinner, 1935; see Catania, 1992, pp. 112–128). This means that formally similar responses and stimuli are not necessarily functionally equivalent within or across individuals. For instance, behavior that is formally or topographically classified as self-injurious may have a variety of functions or meanings. It may function to escape too thin a reinforcement schedule, to avoid too difficult a task, or to produce adult attention (see, e.g., Carr & Durand, 1985b; Iwata, Dorsey, Slifer, Bauman, & Richman, 1982). Likewise, a formally similar stimulus (e.g., social contact with adults) may differ in its reinforcing and aversive functions within and across individuals (see Carr & Durand, 1985a).

Relatedly, responses and stimuli that are formally dissimilar may have the same function or meaning. We implicitly or explicitly acknowledge this, for instance, when we replace self-injurious behavior with functional communication skills (e.g., manual signing)—for both of these, the reinforcer is task cessation (see Carr & Durand, 1985a). The responses are formally different—one is developmentally inappropriate, the other is developmentally appropriate—but their function (i.e., task cessation) is the same. This is what it would be like to be a behavior analyst, not a mechanist.

Antecedent–consequent versus structure–function. As for the other structural model issues, first, to be a mechanist is to presume that a deterministic, antecedent–consequent relation obtains between stimulus and response elements, elements that are defined independently of one another in chains of contiguous physical or physiological things and events (see Chiesa, 1992). This reflects a kind of S–R psychology that contrasts starkly, it seems to me, with the behav-

ior-analytic emphasis on functional (not physically causal) relations among mutually or interdependently defined classes of stimuli and responses (Skinner, 1931, 1935; see Hackenberg, 1988). As Willard Day (1969a) put the matter:

> [I]n attempting to discover functional relationships the radical behaviorist does not accept any *a priori* logical assumption of the universe that is orderly in a mechanical sense upon which he feels he must base his scientific work. (p. 318)

For instance, behavior analysis makes no a priori logical assumptions about the causal efficacy of "rewards" that are made contingent on behavior. Rather, we examine changes in rates of responding as a function of the responding's consequences, the latter of which we call "reinforcers" if the rates increase, given proper control conditions (see Higgins & Morris, 1985). "Reinforcement" is a relative term; it is defined functionally, and is not essentialist.

Behavioral change versus structural change. As for the second structural model issue, to be a mechanist is to view behavioral change as but change in the number, strength, and association of discrete, formal response elements over time. Behavior analysis, however, is more concerned with behavior change as change in functional relations among stimulus and response classes.

For instance, at one level of resolution, we might be interested in changes in a class of social skills we call a speaker's "initiating a conversation" and a class of social consequences we call a listener's "social reciprocity," which together yield that conversation. At another level, we might be concerned with response covariations in the structure or organization of behavioral relations among classes of stimuli and responses within an individual's response repertoire as a whole (Scotti, Evans, Meyer, & DiBenedetto, 1991). For instance, we might be concerned with what we would otherwise call personality or attitudes (Bernstein, 1982; Thompson & Zeiler, 1986; see Hinson, 1987).

Continuity versus discontinuity. Third, to be a mechanist is to assume that behavioral change is a matter of continuity and quantity, that is, of change in the strength, number, and association of formal response elements. If, however, behavioral change is change in functionally defined classes of stimuli and responses (i.e., in the structure or organization of functional relations, as just noted), then change has a discontinuous quality to it. When one functional relation changes (e.g., when stimulus equivalence is established or when functional communication skills are acquired), the organization of the response repertoire as a whole is altered—sometimes minimally so, sometimes dramatically so, but changed nonetheless. Behavior change, then, is a matter of qualitative—not merely quantitative—change in behavior (see Krapfl, 1977).

Functional Model Issues

So much for the structural model issues. Let me turn briefly to the functional model issues. These pertain to the types of explanations acceptable in each worldview (see Overton & Reese, 1973, pp. 74–86). Here, to be a mechanist is to adhere to (a) unidirectional causality, as opposed to reciprocal causality, and (b) to linear causality, as opposed to organized complexity.

Unidirectional and linear causality. With respect to unidirectional causality, Overton and Reese (1973) stated,

> As the very concept of the reactive organism implies, only the efficient cause is active and productive; the effect is merely the recipient of this activity. (p. 77)

About linear causality, they wrote,

> Cause and effect are viewed as standing in an invariable or unique one-to-one relationship such that any particular cause will result in a specified effect and this effect will be completely determined by the initial cause. (p. 82)

In adhering to unidirectional and linear causality, the mechanist's explanatory model is an asymmetric, one-way relation between independently defined causes and effects. In regards to behavior, this model yields an S–R psychology in which a particular stimulus is the cause of its one and only particular response, and in which a particular response is caused by its one and only particular stimulus. Given how behavior analysis

contrasts with mechanism on the four structural model issues just described, its contrast with these functional model issues resolves into much the same thing, but let me put it differently.

From a behavior-analytic perspective, stimuli and responses are not characterized as invariable causal forces or linear relations among instances or elements of stimuli and responses (Skinner, 1931, 1935). Our temporal arrangement and construal of the three-term contingency in basic and applied research appear to belie this point, but not so. When we come to matters of empirical definition (e.g., stimulus, response, reinforcer), we focus on the functional relations between stimuli and responses as class concepts. The functional relations evolve historically with respect to one another (see Bijou & Baer, 1978), and the stimulus and response classes are defined with respect to one another (Skinner, 1935). Reinforcers, for instance, are not defined independently of their effects on operant behavior. Put more generically, stimuli do not possess independent or inherent power to control responses any more than responses are independently or inherently controlled by stimuli. Their functional relation implies a mutuality (see Costall, 1992), a mutuality that is established through a unique, ever-changing interdependent history. Behavior analysis is an historical science in both its subject matter (Donahoe & Palmer, 1989) and its worldview (Morris, 1992a).

So much for the implications of mechanism for the behavior of "the other one"—for the behavior of the subjects and individuals with whom we work. What of its implications for the behavior of "the one"—for our behavior as scientists, for what we know and how we know it? For this, I delve into some technical matters in the history and philosophy of science and psychology.

SOME IMPLICATIONS OF MECHANISM FOR "THE ONE"

Mechanism

In mechanism, knowledge is knowledge of the world as it exists in a realist ontology of independent things and events, and their relations. It is knowledge supposedly independent of the world that is known. With knowledge so construed, the goal of the scientist is to discover the laws of how the world works—laws that are presumed to be extant things and relations independent of the scientist.

Correspondence theory of truth. What of the truth of knowledge so construed, the truth of the laws purportedly discovered? In mechanism, truth is ascertained through correspondence. In the correspondence theory of truth, that is, in truth-by-agreement (Pepper, 1942, pp. 221–231; see Hayes & Brownstein, 1986), the truth of a scientific statement is evaluated in terms of predictive correlations and consistencies found between (a) theories and hypotheses and (b) how the world operates, the latter constituting confirmations (or the lack thereof) of the theories and hypotheses. That is, truth is evaluated in terms of the correspondence between (a) how scientists (the knowers) describe nature through their theories and hypotheses and (b) how nature (the known) works.

Logical positivism and conventional operationism. In the philosophy of science that psychology has largely embraced, this resolves into logical positivism and conventional operationism, as well as the hypothetical-deductive model of theory building, theory testing, and explanation (Carnap, 1935; Stevens, 1939). In this view, the truth about independently discoverable laws, for instance, about the structure and function of intelligence or personality, is evaluated in terms of predictive correspondences between (a) hypotheses about constructs deduced from theories of intelligence and personality and (b) the way behavior indicative of those constructs (and thus those theories) is observed to operate (see Hackenberg, 1988; Hayes, Hayes, & Reese, 1988).

Behavior Analysis

Descriptive positivism. Little of this seems like behavior analysis to me (see Hackenberg, 1988). Behavior analysis long ago rejected logical positivism, conventional operationism, and for the most

part (but not always, see Ferster, 1978), the formal hypothetical-deductive approach to theory construction (Smith, 1986, pp. 257–297).

Skinner's positivism is a descriptive positivism, not a logical positivism (Skinner, 1945; see Moore, 1985). From this perspective, for instance, genius and sociopathy are not hypothetical, unobservable orders and disorders that attain scientific credibility and meaning through a network of logically or empirically verifiable statements about observable behavior indicative of them. Rather, genius and sociopathy are words spoken on certain occasions with respect to certain behaviors in context—said occasions, behavior, and context constituting the meaning of genius and sociopathy. This is an open and descriptive positivism, not a narrow and symbolically logical one.

Relatedly, Skinner's operationism is concerned with the workability of terms and concepts, not solely with agreement among scientists about what terms and concepts mean (Skinner, 1963; see Moore, 1975), although agreement is always a useful and important first step. In other words, the worth of terms and concepts such as genius and sociopathy lies not in whether we agree on logical and empirical operations that permit truth-by-agreement about the nature of genius and sociopathy. In other words, their worth lies not in operations such as the giving and scoring of IQ tests and personality inventories. Rather, their worth lies in their utility in describing, predicting, and experimentally (not statistically or arbitrarily) controlling behavior as our subject matter.

Finally, Skinner's theory building is empirical and inductive, not hypothetical and deductive (Skinner, 1947, 1950, 1956; see Day, 1980; Ferster, 1978). Psychology was perhaps not necessarily wrong in adopting the formal hypothetical-deductive style of the physics of the 1920s and 1930s, as physics attempted to discern the nature of as-yet unobserved events at the galactic and subatomic levels. Rather, psychology was premature in its emulation. In its physics envy, psychology's hypothetical-deductive theories about the supposedly inner stuff of mind (e.g., of hypothetical constructs involving cognition, personality, and the like) ran too quickly and too far ahead of its basic, empirically derived terms, concepts, and laws—terms, concepts, and laws of the sort known to physics when it took up that style of scientific inquiry. As a consequence, theories of mind, cognition, and personality have been largely unconstrained by knowledge about basic terms, concepts, and laws of behavior. That is, psychology has been largely unconstrained by knowledge that has been empirically and inductively derived from the analysis of behavior as a subject matter in its own right (Lee, 1988).

Pragmatic theory of truth. In rejecting logical positivism and conventional operationism, behavior analysis also rejects truth-by-agreement as the truth criterion for knowledge (Skinner, 1974, p. 16). As may be gleaned from my preceding remarks, the truth criterion in behavior analysis is successful working—pragmatic, effective action (Skinner, 1956, 1966; see Hayes et al., 1988; Morris, 1992a). It is not just the description and prediction of behavior, but also its experimental analysis achieved through experimental control (Hayes & Brownstein, 1986; Morris, 1992a).

This philosophical pragmatism takes knowing to be a behavioral relation between the knower and the known (Dewey & Bentley, 1949; see Pronko & Herman, 1982). As a behavioral relation, knowing (e.g., knowing about behavior) is a function of past and present contingencies, and hence is relative, not absolute. Even knowing the truth about behavior cannot be independently evaluated because "evaluating" is behavior, too—behavior subject to its own past and present contingencies and contexts.

In this view, the laws of science, including the laws of behavior, are not independently discoverable things and relations. They are the products of the interactions among scientists and their subject matter. Although prediction is a properly valued goal in science, Skinner's philosophical pragmatism judges theories, laws, and principles true not merely

(a) by their correspondence with behavior indicative of them or (b) across other theories, laws, and principles, but also by their usefulness in understanding behavior through its experimental analysis (see Smith, 1986, pp. 257–297).

As Skinner (1938) wrote in *The Behavior of Organisms*, "So far as I am concerned, science does not establish truth or falsity; it seeks the most effective ways of dealing with subject matters" (Skinner, 1938, p. 241). Later, in *About Behaviorism*, he commented that scientific knowledge is "a corpus of rules for effective action and there is a special sense in which it could be 'true' if it yields the most effective action possible" (Skinner, 1974, p. 259). Jack Marr (1985) put it this way:

> 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)

This is what it would be like to be a behavior analyst, not a mechanist, with respect to the behavior of "the one"—our behavior as scientists (Morris, 1992a).

CLOSING COMMENTS

Meanings and Mechanism

In my introduction, I offered two reasons for why I prepared this paper. One was the intellectually lazy ploy of defining what radical behaviorism is not. The second was to point out that when behavior analysts call themselves or let themselves (and their science) be called mechanists (and mechanistic), this may lead to inapt (and inept) characterizations of our basic and applied research and conceptual programs.

I built my argument around a representative sample of definitions and meanings of mechanism and its cognates occurrent in use and function. These I drew from ordinary-language dictionaries and from dictionaries and glossaries in behavior analysis, psychology, the behavioral sciences, and philosophy. I presented another, more philosophical meaning of mechanistic, an authoritative one first adopted in developmental psychology and now more widespread. I described its implications for the behavior of "the other one" and for the behavior of "the one." From what I found out about mechanism through the process—through discovering what mechanism means in behavior analysis, society, science, psychology, and philosophy—behavior analysis is not mechanistic by definition or in worldview.

I am not saying that I have taken up all and the only meanings of mechanism and its cognates. For instance, I passed over positive denotations and connotations, such as those found in the success that the mechanical worldview earlier brought to the physical and biological sciences or, closer to home, those found in talk of "behavioral mechanisms." As for the latter, though, mechanism means something like a process (e.g., reinforcement); nothing mechanistic is necessarily implied. I also passed over negative denotations and connotations of mechanism, as in "cold" and "unhuman" (cf. Todd & Morris, 1992). And, of course, there are the sciences of classical, celestial, and quantum *mechanics*, but these need to be distinguished from mechanism as philosophy. Certainly, there are other meanings too, at some point probably as many as there are people who use the terms *mechanism*, *mechanist*, and *mechanistic*.

I am also not saying that mechanistic thinking never did nor does not now occur in behavior analysis. Surely it did, even in Skinner's early work (see Coleman, 1984; Schraff, 1982), and it does now, for instance, when we fail to define reinforcers functionally (see Hayes & Brownstein, 1986; Hayes et al., 1988). But this does not mean that behavior analysis is essentially mechanistic in its science, practice, or philosophy (Moxley, 1992).

What this does mean, though, is that the term *mechanism* has mixed meanings—it conveys mixed messages. As such, when we speak of mechanism, we should speak with care, caution, and qualification, if, indeed, we should speak of mechanism at all. To paraphrase Hineline's (1980, 1983, 1984) argument

about the limits of cognitive talk in behavior analysis, perhaps we should refrain from "mechanistic" talk, too. It tends to be imprecise in both scientific and vernacular use, it tends to obscure important properties of behavior, and it tends to trivialize behavior analysis (see Hineline, 1983, p. 183).

The Evolution of Terms and Definitions

One of the unique characteristics of behavior analysis is its treatment of the behavior of scientists, one important concern being the variables that control scientific practice (Day, 1969a). As Catania (1968) has noted, and Moore (1984) has observed,

> [T]he progressive refinement of terminology [e.g., *mechanistic*] is often part of a science itself. Precision of usage allows accurate and unambiguous communication of experimental design, apparatus technology, and results, so that readers may benefit from the work of others. Accordingly, it is healthy to be concerned with definitions, meaning, and usages, because the terms we use accommodate effective action with respect to the world. (Moore, 1984, p. 389)

An important implication of this concern is that as scientific practice evolves empirically and conceptually, so too should its terms and definitions (Baum, 1974). When terms and definitions fail to evolve to accommodate changes in scientific practice, they become misleading or meaningless.

Once upon a time, for example, the term *mechanistic* conveyed the honorific of "natural science" in all its power and glory. Think of Sinclair Lewis's (1925) *Arrowsmith* and the revered figure of Max Gottlieb, the bacteriologist based on the real-life behavioral-biologist and mechanist, Jacques Loeb (1859–1924) (Logue, 1988; see Pauly, 1987). Mechanism was especially an honorific when psychology was first struggling to become scientific, and to be accepted as a science. In its struggle, psychology became behavioristic, and called itself mechanistic because it was mechanistic, because it was scientific (see Boring, 1964). At that time, *mechanistic* also conferred a seriousness of style, and a hope and an optimism that appealed to the pragmatic, progressive culture of the United States (Buckley, 1989; O'Donnell, 1985). *Mechanistic* was both a scientifically and a socially valid term.

Contemporary behaviorism (i.e., behavior analysis) evolved, in part, out of that behaviorism, but it has evolved so considerably since then (see Day, 1980; Moore, 1987) that the social, scientific, and psychological meanings of mechanism are no longer applicable to it—they are no longer scientifically or socially valid. First, these meanings are no longer applicable to the behavior-analytic subject matter. Our unit of analysis is not a two-term S–R unit, but a three-term contingency (Skinner, 1938; see Ringen, 1976).

Second, these meanings are no longer applicable to behavior-analytic science. Behavior analysis is not a logical positivism, but a descriptive, pragmatic positivism (Skinner, 1945; see Smith, 1986, pp. 260–297). Philosophy of science evolves. Indeed, as Skinner (1969) observed,

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

Third, these meanings of mechanism, mechanist, and mechanistic are also no longer applicable to congenial relations between behavior analysis and much the rest of psychology (Skinner, 1987; see Coleman & Mehlman, 1992), not to speak of society and the rest of science. Society, science, and psychology are not inclined toward behavior analysis, in part, because they take it to be mechanistic in its treatment of its subject matter and in worldview, a misunderstanding that behavior analysts themselves have sometimes perpetuated (Morris, 1992a). This mistaken inclination (or disinclination) would be improved, I think, if behavior analysts explained themselves and their science differently (see Hayes et al., 1988; Morris, 1988a) and if we spoke in a more measured fashion (see Hineline, 1991; Neuringer, 1991). As Catania (1989) has cogently quipped, "Those who weigh their words may find that the scales then begin to tip in their favor" (p. 194).

In conclusion, what I have offered is an argument, but only an argument—an argument that behavior analysis is not mechanistic. It is an argument that would not necessarily be true even if we were all to agree with it. The truth of my argument lies in its usefulness, in its consequences. Externally, its truth should be judged by its usefulness in understanding the behavior of "the other one"—the behavior of our subjects and the individuals with whom we work, and of society, science, and psychology. At least Marr (1982) is hopeful in these regards: "The abandonment of mechanistic determinism should not be viewed by behaviorists with despair, but rather be looked upon as liberating (as it has been for physics)" (p. 207).

Internally, the truth of my argument should be judged by its usefulness in understanding the behavior of "the one"—our own behavior in discovering some uniformities, ordering some confusing data, and resolving some puzzlement about what behavior analysis is and is not (cf. Skinner, 1979, p. 282). One thing it is not is mechanistic.[2]

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[2] One thing behavior analysis *is*, however, is a variety of the worldview of contextualism (see Pepper, 1942, pp. 232–279). This seems to be true at least for its unit of analysis (the historic event, the act-in-its-context) and for its truth criterion (successful working, effective action) (see Hayes, 1988; Hayes et al., 1988; Hayes, Hayes, Reese, & Sarbin, in press; Morris 1988a, 1988b, 1989, 1991, 1992a, in press). But this is another matter, for another time, for another place. And even if arguable, before we can decide what *ism* behavior analysis might be a variety of—mechanism, contextualism, selectionism (see Palmer & Donohoe, 1992), or selectivism (Pepper, 1966), if any *ism* at all—we first need to understand what the different *isms* mean. And that is part of the purpose of this paper—to begin to understand what is meant by one *ism*—*mechanism.*

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