Radical Behaviorism and Scientific Frameworks

From Mechanistic to Relational Accounts

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A substantial portion of B. F. Skinner's scholarship was devoted to developing methods and terms for a scientific study of behavior. Three concepts central to scientific accounts—cause, explanation, and theory—are examined to illustrate the distinction between mechanistic and relational frameworks and radical behaviorism's relationship to those frameworks. Informed by a scientific tradition that explicitly rejects mechanistic interpretations, radical behaviorism provides a distinctive stance in contemporary psychology. The present analysis suggests that radical behaviorism makes closer contact with the "new world view" advocated by physicists and philosophers of science than does much of contemporary psychology.

Approaching the end of the 20th century, there remains no unified framework or set of principles to define the discipline of psychology and to guide research. Lack of unity is more extensive than a simple incompatibility between interpretations of particular experiments, or even particular theorists:

> it has to do with what counts as the subject matter of psychology, with what questions we should ask about this subject matter, with how we should go about finding answers to these questions, with the status of existing psychological knowledge, and with whether psychology can be a science. Psychologists do not agree upon these basic matters. (Lee, 1988, p. 2–3)

A diversity of philosophical approaches gives rise to fundamental disagreements over the nature of psychology's subject matter, leading to variations in methodology, data, and interpretation, and to little generalization of findings from one approach to another.

One principle that continues to distinguish psychology from its parent discipline, philosophy (even though it fails to unite its various approaches), is a commitment to the application of scientific methods to epistemological questions. As the physical sciences have demonstrated their power to understand, explain, predict, and control the world around us, psychologists have hoped that scientific methods will prove equally powerful when applied to questions of human psychology. Bertrand Russell (1946), among others, expressed this faith in the superior explanatory power of science over other methods of gathering knowledge: "I have no doubt that, insofar as philosophical knowledge is possible, it is by these methods (the methods of science) that it must be sought. I have also no doubt that, by these methods, many ancient problems are completely soluble" (p. 788).

Major shifts have taken place in the philosophy of science and in our thinking about science in the course of the present century. At the turn of the century physics was thrown into turmoil by evidence that the principles of Newtonian science previously thought to be universal truths could not be applied to certain types of physical phenomena. The philosophers of the Vienna Circle (logical positivists) attempted a program of defining the limits of scientific explanation and the elimination of metaphysics from such explanations. More latterly, theorists such as Kuhn (1962) and Lakatos (see Lakatos & Musgrave, 1970) have swept away popular notions of "absolutes" and "ultimate truths" in science by pointing out that even in the process of validating scientific knowledge there is a measure of arbitrary decision making. Since Kuhn and Lakatos, it is no longer feasible to argue that the world is "just so" because science claims it to be, and the assumption of "pure" observation (observation independent of theory, uninformed by background assumptions) has similarly dropped out of the mainstream of acceptable opinion (see, e.g., Hanson, 1958). Philosophers and scientists alike continue to debate the implications of new views of science and the implications of the new views of nature given by contemporary science. The impact of such debates was neatly captured by Woolgar when he stated that "One of the most remarkable features of modern thought is the extent to which ideas about science have changed" (Woolgar, 1988, p. 9).

Implications of the loss of certainty in science and in the philosophy of science have been especially considered over the past two decades in works such as *The Tao of Physics* (Capra, 1975); *Mathematics: The Loss of Certainty* (Kline, 1980); *The Death of Nature: Women, Ecology, and the Scientific Revolution* (Merchant, 1982); *The Turning Point: Science, Society and the Rising Culture* (Capra, 1983); *Order out of Chaos: Man's New Dialogue With Nature* (Prigogine & Stengers, 1985); *Mathematics and the Search for Knowledge* (Kline, 1985); *The Cosmic Blueprint* (Davies, 1987); *Chaos: Making a New Science*

My sincere thanks go to Derek Blackman and Mike Harries for the time they gave to make substantive and editorial comments.

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Vol. 47, No. 11, 1287–1299

(Gleick, 1988); *A Brief History of Time* (Hawking, 1988); and *Does God Play Dice?* (Stewart, 1989). With authors ranging from Nobel-prizewinning chemists to science journalists, this debate clearly has multidisciplinary participation as well as popular interest. The "scientific revolution" of the early 20th century has generated a "philosophical revolution" at the end of the century. As a scientific discipline, psychology must be concerned with issues currently abroad in the philosophy of science while it continues its internal debates over the nature of its subject matter and appropriate methods of investigation.

Scientific Frameworks

A central aspect of contemporary philosophy-of-science debates that has particular implications for psychology, both in terms of internal scientific considerations and its place among the sciences in general, is the assertion that the mechanistic worldview of Newtonian–Cartesian science has passed its zenith. The position was most thoroughly articulated by Merchant (1982) and Capra (1983) who argued that a mechanistic conception of reality dominated the 17th, 18th, and 19th centuries, when "Matter was thought to be the basis of all existence, and the material world seen as a multitude of separate objects assembled into a huge machine. Like human-made machines, the cosmic machine was thought to consist of elementary parts" (Capra, 1983, pp. 31–32). This mechanistic vision of natural phenomena, Capra argued, provided a framework that informed methodology and theoretical interpretation in other sciences: "Whenever psychologists, sociologists, or economists wanted to be scientific, they naturally turned toward the basic concepts of Newtonian physics" (p. 32).

Merchant (1982), referring to machines as "structural models for western ontology and epistemology" (p. 227), cogently elaborated this view of the rise of machine-as-metaphor:

> The imagery, iconography, and literary metaphor associated with machines extended the experiences of everyday life to the realm of the imagination, where machines became symbols for the ordering of life itself. Out of such symbolic universes evolve conceptual universes as new definitions of reality replace the old. As the machine and clock increased their symbolic power as root metaphors, in response to society's changing needs, wants, and purposes, the symbolic force of the organism declined in plausibility and the organic conceptual framework underwent a fundamental transformation. The images and symbols associated with the machines of everyday life helped to mediate the transition between frameworks. (p. 227)

Natural phenomena in contemporary physics are no longer seen to consist of separable and discrete "elementary parts" (Capra, 1983, p. 32). Modern physics is no longer underpinned by machine-as-metaphor, but has moved to a *relational* framework for understanding its subject matter:

> In the twentieth century . . . physics has gone through several conceptual revolutions that clearly reveal the limitations of the mechanistic world view and lead to an organic, ecological view of the world. . . . The universe is no longer seen as a machine, made up of a multitude of separate objects, but appears as a harmonious indivisible whole; a network of dynamic relationships. (Capra, 1983, p. 32)

Psychology, medicine, biology, economics, and other sciences that have traditionally modeled themselves after physics are called upon to reexamine the usefulness of the mechanistic, Newtonian–Cartesian framework in understanding their own subject matter. In the climate of widespread calls for a new framework—a nonmechanistic, relational framework in sciences other than physics—psychologists are faced with the conceptual task of examining the basic principles, assumptions, and methods that constitute their scientific framework.

Radical Behaviorism and Scientific Frameworks

In the early part of this century, behaviorism generated a major scientific shift within psychology. Many historical and contemporary figures are listed under this heading in the psychological literature, including of course B. F. Skinner. What is not often noted, however, is that the behaviorism of B. F. Skinner—radical behaviorism—differs distinctively from other behavioral systems both at the philosophical level and at the level of its scientific practice. Much of contemporary psychology belongs to a behavioral tradition, but radical behaviorism is distinct in two major respects. First, it is characterised by a high degree of internal coherence; its subject matter is particularly carefully defined, and its methods of data collection, analysis, and interpretation generally agreed upon by researchers within the field. This level of coherence marks it as "the closest thing to a school or paradigm among all modern positions" (Hillix & Marx, 1974, p. 264). Second, it is distinct in that its historical influences have led to the development of a descriptive, observational, and integrative system of inductively derived principles, in contrast to the theory-driven, hypothetico-deductively derived, statistically based principles of most other branches of experimental psychology.

Defining behaviorism as "a philosophy of science concerned with the subject matter and methods of psychology" (Skinner, 1969, p. 221), Skinner devoted a substantial portion of his scholarship to developing methods and terms for a scientific study of behavior. Throughout his lifetime, he continually confronted the task of developing empirically valid methods and terms to account for behavior, and from his earliest efforts (e.g., Skinner, 1931/1972) onward emphasized the power and utility of an explanatory system that is explicitly relational.

The relational nature of radical behaviorism and the explanatory system it generates can best be illustrated by examining three concepts that together compose scientific accounts: *cause, explanation,* and *theory.* These concepts are central to scientific practice in the sense that causal relations form the bedrock of scientific explanation (although a succinct definition of science is elusive, it would be hard to dispute that causal relations are fundamental to explanation in this enterprise). Explanations, however, do not simply emerge independently. Rather, they are framed, organized, and expressed within a linguistic network of related concepts (i.e., theories).

In the process of unravelling these intimately bonded concepts, it becomes clear that each carries more than one definition, and that they have different underlying meanings related to different scientific frameworks. Also in the unravelling, the distinction between radical behaviorism and mechanistic explanatory systems becomes clear.

Causation in Science

The concept of causation, like many other concepts in science and philosophy, has changed from one historical era to another as a consequence of shifts in empirical knowledge and careful philosophical analyses. Hume's analysis and positivist developments in science and the philosophy of science accomplished a relatively recent major shift.

The historical significance of Hume's analysis lies in dispensing with notions of "force" or "agency" that continued to haunt the concept of causation. Russell (1946), tracing the rise of science and the development of scientific ideas, noted that

> "Force" in Newton, is the cause of changes of motion, whether in magnitude or direction. The notion of cause is regarded as important, and force is conceived imaginatively as the sort of thing that we experience when we push or pull. (p. 524)

However, it gradually became clear that gravitational equations could be worked out without alluding to forces:

> What was observable was a certain relation between acceleration and configuration; to say that this relation was brought about by the intermediacy of "force" was to add nothing to our knowledge. (p. 524)

Russell described this conception of causal force as "the faint ghost of the vitalist view" of the causes of motion, and with the increasing sophistication of scientific equations "gradually the ghost has been exorcized" (p. 524).

Hume (1777/1975) pointed out that our interpretation of causation involves more than simple observations of constant relations. What we observe and learn by experience, he argued, are no more and no less than relationships—"the frequent *Conjunction* of objects" (p. 70). What we add to observation is a sense of power or agency on the part of a causal event: "We suppose that there is some connexion between them (cause and effect); some power in the one, by which it infallibly produces the other, and operates with the greatest certainty and the strongest necessity" (p. 75). This added supposition derives from the way we feel when we move about changing the world by pushing, pulling, lifting, dropping, and otherwise manipulating objects, but it is merely a thing of thought. Cohen and Nagel (1955) described this as "common-sense" causation:

> The "common-sense" notion of cause is an interpretation of nonhuman behavior in terms peculiarly adopted to human behavior. Thus, "John broke the window," is supposed to express a causal relation, because there is an agent "John" who *produced* the breaking of the window. (p. 246)

Similarly, in a statement such as "moist air rusts iron," moist air is said to be the cause and rusted iron the effect, in that moist air is believed to produce the rusting: "In the popular mind, all *changes* require *causes* to explain them, and when found are interpreted as agents producing the change" (p. 246). Hume's analysis concluded that notions of agency, force, or necessity of connection are superfluous. Today the scientific conception of causation refers to events occurring "as a function of" other events rather than in terms of "A exerts a force on B." But as Cohen and Nagel pointed out, the popular conception retains the notion of agents producing change.

Causal Chains and Causal Networks

A feature of causal thinking that has been more recently challenged (Hanson, 1955) is the chain metaphor. In setting events in a relation of succession, this metaphor embraces the relational nature of causation without necessarily invoking force or agency on the part of any link in the chain. Thus, it describes a sequence of events between, for example, administration of a drug and cessation of pain: Administration of a drug is followed by a change in the constitution of blood, which is followed by a change in the activity of nerve endings, which is followed by a reduction in pain. Or, as in a much used example, between the cue ball striking the red ball, which bounces off the cushion and strikes the black ball, which then falls into the pocket. The chain in these cases has an identifiable beginning and end, with each event contiguous in space and time. Like a chain, the metaphor illustrates causation as a contiguous sequential process. In these examples, the chain is sequentially identifiable from beginning to end, and in these kinds of examples the metaphor may be useful for practical purposes.

Hanson (1955) pointed out, however, that this conception of causation does not take into account the voluminous background knowledge tacitly involved in causal accounts. It ignores whole systems of constructs and properties involved in causal accounts that are not simply given by immediate observation and experience. It ignores the fact that causal accounts are only meaningful as such within conceptual networks. Drugs, blood, nerve endings, and so on are conceptual units related within a network of conceptual units. Without the network, the words themselves have no causal significance. It is the background knowledge—the network—that gives them this significance. The cue ball example, striking in its simplicity, is similarly only meaningful against a conceptual background involving knowledge of the movement of spheres, of angles of deflection, and of the properties of billiard tables. Especially in such simple cases, familiarity obscures background knowledge, leaving a superficial impression of one-to-one causal sequences. The chain metaphor ignores the fact that causal terms are more than isolated observations and ignores the conceptual systems lying behind those terms, giving them meaning as causal explanations:

Genuine causal connexions can be expressed (explicitly or implicitly) only in language that is many-levelled in its generality and its explanatory power. This is why the language of causality is diagnostic and prognostic, and why the simple tick-tock, click-click, links-in-a-chain view of cause and effect is so artificial and inapplicable. (Hanson, 1955, p. 300)

Causal terms are related, not as links in a chain but as webs or networks, by theories. Causal relations are expressed within conceptual systems, and it is those systems of background knowledge that distinguish causal relation from mere contiguity in space and time. The simple one-to-one implication of the chain metaphor does not properly illustrate the weblike character of causal terms.

Links-in-the-Chain and Mechanistic Thinking

Hanson (1955) traced the illusory power of the chain metaphor to the same source as did Merchant (1982) and Capra (1983). The simplicity and power of the designed machine, both inside and outside of the laboratory, its endurance and stability under changing external conditions, give rise to links-in-the-chain causal thinking:

Such machines work with considerable indifference to alterations in environment. Clocks, anemometers, windmills, water-wheels, etc. are made *not* to stop for thunderstorms, swarms of bees, the barking of dogs or the crowing of roosters . . . from this the temptation grew to construe causal explanation as mechanical explanation; that is, explanation of the perseverance of manufactured machines. (Hanson, 1955, p. 309)

From this construal, it is only a small step to think about natural phenomena in a similar manner, functioning as mechanical systems.

Hanson (1955) argued that causal explanation is better described with terms such as *causal theories* or *causal laws,* because these terms recognize more than the one-to-one relationships implied by *causal chain* and are more indicative of the weblike complexity of causal accounts. He noted, however, that the chain metaphor remains popular.

Causation in Radical Behaviourism

Skinner adopted his conception of causation directly from Ernst Mach, whose *Science of Mechanics* (1893/1960) in particular and whose positivist orientation toward science in general influenced Skinner in his early years as a graduate student at Harvard University (Skinner 1931/1972, 1938, 1978; Smith, 1986).

Mach referred to Hume's analysis in opposing what he considered to be the metaphysical concept of *force* (to which Newton remained committed) and replaced *cause and effect* with *functional relation.* In the Preface to his *Science of Mechanics,* Mach (1893/1960) stated that his aim was to "clear up ideas, expose the real significance of the matter, and get rid of metaphysical obscurities" (p. xxii). One of those metaphysical obscurities was precisely the notion of force lingering on in Newtonian causation.

Influenced by Mach's analysis, Skinner replaced agency, push–pull causality, with Mach's functional relations:

We may now take that more humble view of explanation and causation which seems to have been first suggested by Mach and is now a common characteristic of scientific thought, wherein, . . . the notion of function [is] substituted for that of causation. (Skinner, 1931/1972, pp. 448–449)

In the Skinnerian system, a cause is replaced with a "change in the independent variable" and an effect is replaced with a "change in the dependent variable," transforming the cause–effect connection into a "functional relation" (Skinner, 1953, p. 23). Especially important in relation to causation and human behavior is the elimination of the concept of agency, because although it is no longer customary to describe relations in other sciences in terms of internal force or agency, these pre-Humean notions persist in descriptions of human behavior, even in contemporary social learning theory (e.g., Bandura, 1977, 1989). As in other contemporary natural sciences, cause in radical behaviorism is stripped of its older connotation of force or agency.

Selection as a Causal Mode

In addition to departing from the popular conception of cause as a force or agent producing change, radical behaviorism also departs from the traditional chain metaphor, which requires that (like a machine) causal relations be contiguous in space and time, that gaps between cause and effect be filled by a sequence of events standing in a relation of succession. In the mechanistic framework, if aspirin alleviates headaches, it is because from aspirin to headache there is a contiguous, sequential chain of events culminating in the cessation of pain. If a mechanical system breaks down, it is because one or more of the components of the chain has failed. When the component is removed, repaired, and restored, the mechanical system proceeds as normal. Components in mechanical systems are prepared separately, and the system assembled into a perfect whole that is the sum of its parts.

The chain metaphor, with its requirement of contiguity, dominates many areas of contemporary psychology. Cognitive psychology is a particularly good example, in which contiguous causality is satisfied by internal systems, machinelike in their organization and predictability. Cognitive psychology considers successive mediating events between the outer world of the organism (environmental input) and the behavior of the organism (response output). The gap between these two endpoints is reputed to be filled by various components (e.g., a complex memory system comprising several constituent parts, an information processing system also comprising several constituent parts, a "cognitive map," or a symbol manipulation system). These components may be relatively simple in design, or they may involve complex hierarchically organized structures having multiple functions. Nevertheless, their overall role in the explanatory system is that they satisfy the chain metaphor's requirement of contiguity. Less machinelike and more abstract links in the chain are provided by other psychological accounts, with components such as expectations, intentions, desires, thoughts, attitudes, and other "mental" states. The common feature of all of these is that they act as links, they fill spatial and temporal gaps between one event and another, and in doing so they give a machinelike account of causal relationships. Like a machine, a breakdown is attributed to one or more of the events in the causal sequence, to a malfunction in one of the components mediating between two end events.

The earlier statement that much of contemporary psychology belongs to a behavioral tradition refers to the influence of the originator of stimulus–organism–response (S–O–R) psychology—Edward C. Tolman. Unhappy with Watson's stimulus–response connections as explanations of behavior, Tolman argued for the inclusion of such theoretical terms as *expectations* and *anticipation* between input (stimulus) and output (response):

> This new behaviorism will be found capable of covering not merely results of mental tests, objective measurements of memory, and animal psychology as such, but also all that was valid in the results of the older introspective psychology. And this new formula for behaviorism which we would propose is intended as a formula for *all* psychology—a formula to bring peace, not merely to the animal worker, but also to the addict of imagery and feeling tone. (Tolman, 1922; in Hillix & Marx, 1974; p. 221–222)

Although by 1959 Tolman himself had to some extent lost faith in his system, or "so-called" system, as he put it (Tolman, 1959, p. 152), Bower and Hilgard (1981) have suggested that his impact has been greater than he anticipated: "The sort of program Tolman envisioned seems now to be coming to fruition in modern cognitive psychology" (p. 351). The cognitive approach is merely one example of a standard framework in contemporary psychology that concerns itself for the most part with contiguous, mediating events between environment (input) and behavior (output). For all its admitted vagueries, Tolman's behavioral S–O–R formulation continues to provide the framework for mainstream scientific psychology, even though contemporary scientific thinking in other fields has moved well beyond the interpretive metaphor that this framework takes as fundamental.

In contrast, radical behaviorism adopts a causal mode that carries no requirement to provide mediating links between one event and another, is not sequential, and does not presuppose contiguity in space and time. It is a mode that encompasses causation over time (life history, experience) and has been compared with the Darwinian mode of selection on variation. The Darwinian template appeals to the selection of particular biological characteristics of a species over time (selection on variation: *phylogenetic*). Radical behaviorism appeals to the selection over time of characteristics of behavior out of the wide range of possibilities available to the individual (selection on variation: *ontogenetic*). Skinner (1972) noted that

> Selection is a special kind of causality, much less conspicuous than the push–pull causality of nineteenth-century physics, and Darwin's discovery may have appeared so late in the history of human thought for that reason. The selective action of the consequences of behavior was also overlooked for a long time. It was not until the seventeenth century that any important initiating action by the environment was recognized. (p. 353)

Agency in biological creation was once ascribed to God, as agency in human behavior largely continues to be ascribed to an internal "self" separate from and overriding environmental events. Selection on variation, or environmental selection, is a causal mode that does not require gaps within functional relations to be filled by mediating sequences of contiguous events. Selection occurs over time, not necessarily in an immediate temporal or spatial relationship to the repertoire of interest.

Selection, even of very complex behavior, is demonstrated in the shaping procedures of operant conditioning experiments in which patterns are developed, strengthened, maintained, and extinguished by controlling both setting conditions and consequences (contingencies of reinforcement). Thus, Skinner (1984) noted, "Selection is not a metaphor, model, or concept; it is a fact. Arrange a particular kind of consequence, and behavior changes. Introduce new consequences, and new behavior will appear and survive or disappear" (p. 503). Selection as a causal mode is not an assumption; it is empirically validated in operant conditioning experiments that demonstrate shaping and maintenance of even complex behavior by complex contingencies.

Action Over Time

If causation (selection) occurs over time, then a logical progression is to study its effects over time. Action over time is an unusual concept for most of psychology, which tends to treat its subject matter episodically. Many types of psychological research examine episodes in the lives of organisms, slices of an ongoing process, and attribute causation to immediate features of the episode. In contrast, research informed by radical behaviorism allows for the examination of behavioral processes as extended over time and allows for the identification of relations between behavior and other events that also occur over time. Patterns of behavior can be established over a long period of time by patterns of consequences, and without the necessity of contiguity radical behaviorism's causal mode allows for multiple scales of analysis. That is to say that when behavioral and environmental events do not reveal contiguous relations, the level of analysis may be shifted to the abstraction of patterns: "Just as the power of a microscope must be adjusted as a function of the phenomenon under study, so too does the level of behavior analysis need to be adjusted to the functional unit of behavior–environment interaction" (Morris, Higgins, & Bickel, 1982, p. 119).

When causation is not necessarily contiguous, causal accounts can refer to properties not ordinarily included in episodic research. Patterns can be abstracted and accounted for with reference to events occurring over time in the organism's environment. The present organism (i.e., as of now) consists of an accumulation of past effects, whereas in episodic research the present organism is divided into its behavior and an internal, independent system that is said to account for the behavior. Episodic research, as Lee (1988) has noted, examines

> selected parts of conduct over limited periods of time, often a few minutes. This research seldom approaches psychological hypotheses by building a history and by studying the effects of this history on subsequent performance. Even when a personal history is built, psychologists seldom attribute the results to the history. Instead they attribute performance to psychological categories such as knowledge, intention, and so forth, with these categories formulated as intervening variables. (p. 162)

Personal history is neglected in the episodic account by a commitment to contiguous causation, whereas the causal mode of variation and selection draws attention to the effects of past experience on present behavior. Personal history (experience) is a necessary part of explanations of present behavior in the variation and selection causal mode.

Explanation and Theory

Description and Explanation: Mach

Another feature of Mach's philosophy of science directly adopted by Skinner is the proposal to limit or reduce the concept of explanation to description. To the modern reader, accustomed to thinking of science as an enterprise going beyond descriptions to explanation, this proposal seems contradictory to the aims of science. Indeed, Hempel and Oppenheim began their classic "Studies in the Logic of Explanation" with precisely this assertion: "scientific research in its various branches strives to go beyond a mere description of its subject matter by providing an explanation of the phenomena it investigates" (Hempel & Oppenheim, 1960, p. 135). Mach's distinction arose from two features of his argument: (a) the definition of *description,* which is related to Mach's views on causation; and (b) Mach's opposition to certain kinds of theories, especially to those grounded in a mechanistic view of nature that consequently appeal to hypothetical entities to bridge temporal and spatial gaps between causes and effects. This somewhat misleading distinction is derived from a major debate of the 19th century concerning appropriate interpretive techniques (theories) in physics and a dispute over attempts to describe natural phenomena in terms analogous to the workings of a "great machine."

Mach asserted that complete descriptions of phenomena suffice as explanations. He wrote that

> [it] is only possible of events that constantly recur, or of events that are made up of component parts that constantly recur. That only can be described, and conceptually represented, which is uniform and conformable to law; for description presupposes the employment of names by which to designate its elements; and names can acquire meanings only when applied to elements that constantly reappear. (Mach, 1893/1960, p. 6)

In this passage, Mach made the point later made by Hanson (1955)—that the words used to describe phenomena are "many-leveled" in their generality and explanatory power. When refraction of light in water is described, the words *light* and *water* already compact several conceptual properties that, if need be, can be further described by other words compacting conceptual properties. Mach and Hanson agreed that descriptive terms in science imply properties and relations (Hanson's "background knowledge"). Unexplained phenomena are those in which the scientist has not yet discovered simple recurring elements, elements "that amid all multiplicity are ever present" (Mach, 1893/1960, p. 6):

> When once we have reached the point where we are everywhere able to detect the *same* few simple elements, combining in the ordinary manner, then they appear to us as things that are familiar; we are no longer surprised, there is nothing new or strange to us in the phenomena, we feel at home with them, they no longer perplex us, they are *explained.* (p. 7)

Descriptive terms in science are embedded within theoretical frameworks, webs of related concepts that give them meaning. An explanation of why light refracts in water is contained in another description, a description of the behavior of a type of wave phenomena (light being an instance of that type) traveling through optical media that are denser than air (water being an instance of those media). In describing relations between conceptual properties (e.g., light and water) in the form of a general law (e.g., refraction), the phenomenon is explained.

"Light refracts in water" as a description of a relation between conceptual properties does not, of course, satisfy the question "Why does light refract in water?" That question is satisfied (the phenomenon accounted for) by a further description of the relation between the properties of light and water and the law of refraction. In another context, "Light refracts in water" does function as an explanation, as the answer to the question "Why does this straight rod seem to bend when I put it in the lake?" In this context, "Because light refracts in water" (the same description of relations) is an explanation of the phenomenon observed. At each level of questioning, explanations are given by describing relations between conceptual properties—Mach's "functional relations." Often the conceptual properties are so compacted, so familiar, that they fade from prominence. Nevertheless, every account of natural phenomena amounts to a description of relations. Explanations *are* descriptions.

If explanations are descriptions, what then necessitated a distinction? This question raises the second feature of Mach's argument and requires a brief excursion into one of the major debates of 19th-century physics. This debate's relevance to explanation and theory in contemporary psychology become clear in following sections.

Explanation and Theory: Mach

Superficial accounts of the atomic debate represent it as a dispute over the reality of atoms and the explanatory power of atomic theory, with Mach's opposition to certain types and certain treatments of atomic theory widely documented (e.g., Blackmore, 1972; Bradley, 1971; Cohen & Seeger, 1970; Feyerabend, 1970; Bradley, 1971). Laudan (1981) and Brush (1968), however, argued that such accounts ignore both the context of that debate and its wider implications for the philosophy and methodology of science. Laudan stated that: "though the fact of Mach's opposition to atomic/molecular theories is well known and widely cited, Mach's specific argumentative strategies against such theories have been less fully explored and understood" (Laudan, 1981, p. 202). Similarly, Brush (1968) noted that

> When Mach's statements on atomic theory are put in their historical context, it turns out that Mach's position is much more complex than is generally reported. Moreover, some of the *scientific* questions discussed by Mach are by no means settled even today, to say nothing of the philosophical or methodological ones. (p. 193)

Mach was troubled by his era's emphasis on atomic explanations for two reasons. First, he was disturbed by the possibility that theories of this sort could distract attention from the phenomena they were invented to account for—that hypothetical constructs within the theory, rather than functional relations, might become the focus of attention. Second, he saw such theorizing as an attempt to reduce natural phenomena to mechanical systems and to describe the world as though it functioned as a great machine whose whole could be understood by identifying each of its parts. In other words, this type of theorizing belonged to a mechanistic worldview that Mach did not share with some of his contemporaries.

Although Mach was hostile to interpretations that implanted links between functional dependencies, he allowed for a heuristic function of unobservable, hypothetical properties or entities, such as atoms. As provisional helps rather than ontological realities, they may be useful for generating new questions and establishing new relations and laws. Temporarily useful props on which to build experimentation and suggest new problems, they were to be abandoned once new relations had been established:

> For Mach, theoretical entities may play an important but intrinsically transitional role in natural science. Once they have suggested those empirical connections that are the warp and woof of scientific understanding, they can be discarded as so much unnecessary scaffolding. . . . Above all, Mach stresses that we must not confuse the tool with the job by pretending that the model does anything more than establish functional relationships between data. (Laudan, 1981 p. 212)

For Mach, theoretical models based on hypothetical entities did not describe anything in the world; they were provisionally useful tools of science to be discarded when they no longer lead to the discovery of functional dependencies. They were to remain in the realms of the hypothetical and not to be given the status of explanations (descriptions). Such theorizing becomes problematic when it is elevated to the status of explanation. Mach's distinction between description and explanation, then, is a distinction between explanatory systems that integrate and describe observed functional dependencies, and explanatory systems invoking hypothetical entities between those dependencies as links in a causal chain. The atomic debate was less about the question of whether atoms existed than interpretation and causal modes in science.

In his introduction to *The Science of Mechanics* (Mach, 1893/1960), Karl Menger noted that 18th- and 19th-century physics was suffused with attempts to explain gravitation by appealing to mediating entities:

> Physicists hypostasized vortices, or tensions in media, or bombardments of the bodies by particles traversing space at random and driving, for instance, a stone towards the earth because the latter shields the stone against the particles coming from below. (p. vii–viii)

Gravitational attraction or repulsion was attributed in these accounts to action taking place through a particle medium or ether. This medium, hypothetical and unobserved, allowed for contact action, a links-in-the-chain or mechanistic causality. If events at a distance show functional relatedness (so the mechanist's thinking goes), then there must be between those events a sequence of other events, some medium, some structure, some mechanism, that connects them. For the mechanist, it is the thing in between that explains the relation, even though the relation was all that was ever viewed.

Of mechanistic thinking, Mach (1893/1960) wrote, "The view that makes mechanics the basis of the remaining branches of physics, and explains all physical phenomena by mechanical ideas, is in our judgement a prejudice" (p. 596). He was critical of the way in which hypothetical entities often shifted from the status of scientific tools to the status of explanations, becoming realities in themselves behind the phenomena. He was critical also of scientists who, having created theoretical models involving hypothetical constructs (such as atoms), proceeded to make the constructs themselves into objects of inquiry, relegating to the background the phenomena that the models or constructs were originally developed to account for. He referred to these models as "intellectual machinery" and cautioned that the machinery of thought should not be mistaken for descriptions of the real world:

> A person who knew the world only through the theater, if brought behind the scenes and permitted to view the mechanism of the stage's action, might possibly believe that the real world also was in need of a machine-room, and that if this were once thoroughly explored, we should know all. Similarly, we, too, should beware lest the *intellectual* machinery, employed in the representation of the world on *the stage of thought,* be regarded as the basis of the real world. (Mach, 1893/1960, p. 610)

Mach opposed the practice of elevating hypothetical (invented) constructs to the status of explanation because they describe nothing. He also cautioned against the practice of turning such constructs into ontological realities and into the focus of inquiry. Furthermore, he consistently opposed a causal mode requiring things in between to connect dependent phenomena, a world-as-machine view.

Description and Explanation: Skinner

Skinner's descriptions take the same form as Mach's, they are statements of functional dependencies, of regularities in the relation between dependent and independent variables. Description in this sense differs from narration, in which "The story is simply told of something that has once happened" (Skinner, 1938, p. 9). A narrative statement simply states the occurrence of a single event:

> In the narrative form, for example, it may be said that "at such and such a moment the ape picked up a stick." Here there is no reference to other instances of the same behavior either past or future. It is not asserted that all apes pick up sticks. (p. 9)

In the refraction example, a narrative statement would be "At time X, this beam refracted through this medium," which describes an instance without reference to regularity. "Light refracts in water," however, expresses a uniformity, a regularity in the behavior of light in certain media. "Apes eat bamboo" similarly expresses a regularity, summarizes a uniformity—a relation between conceptual properties.

To be explanatory, a description must relate uniformities between classes or properties. In the Skinnerian system, descriptions integrate and summarize relations. They do go beyond single instances to uniformities, but do not go beyond the relations observed. *Reflex,* for example, describes a correlation between one event and another. When Skinner stated that reflex is "not given local or physiological properties" in his system (Skinner, 1938, p. 44), he referred to the traditional practice of locating reflex within the organism and ascribing to it physiological properties such as a "neurological arc" bridging a gap between its two end-terms, stimulus and response. For Skinner, *reflex* describes no more or less than a relation. The term is an abstraction of a reliable uniformity. If a reflex is located at all it is located in the relation between particular kinds of stimuli and responses, and not within the organism. In the expression of these relations, which "amid all multiplicity are ever present" (Mach, 1893/1960, p. 6), lies explanation. Behavior is explained in the description of uniform relations between dependent variables (units of behavior) and independent variables in the context in they occur. Explanation, for Skinner as for Mach, is description. As with Mach, therefore, the question of why Skinner sought to confine his system to description is raised.

In the case of Mach and the atomic debate in 19th-century physics, the distinction between explanation and description grew out of disputes over interpretive techniques (theories) and causal thinking. Similarly, in 20th-century psychology, Skinner's distinction grew out of his opposition to particular kinds of interpretations of human behavior (theories) and to a causal thinking that requires temporal gaps to be filled by contiguous, mediating events or structures.

Explanation and Theory: Skinner

Several of Skinner's papers were given over wholly or mainly to outlining his views on explanation, description, and theory in a science of behavior, notably "Current Trends in Experimental Psychology" (1947/1972), "Are Theories of Learning Necessary?" (1950/1972), "Critique of Psychoanalytic Concepts and Theories" (1956), and "The Flight from the Laboratory" (1961/1972). Although he has sometimes been interpreted as antitheoretical (e.g., Westby, 1966), and even atheoretical (e.g., Scriven, 1956), in "Current Trends in Experimental Psychology" Skinner (1947/1972) clearly expressed an opposite view. He argued in this paper that "Behavior can only be satisfactorily understood by going beyond the facts themselves. What is needed is a theory of behavior" (p. 301), and that

> Whether particular experimental psychologists like it or not, experimental psychology is properly and inevitably committed to the construction of a theory of behavior. A theory is essential to the scientific understanding of behavior as a subject matter. (p. 302)

In the late 1940s and early 1950s, psychology was beginning to doubt the claims of its major theorists that a comprehensive theory of behavior would be forthcoming. Hull's theoretical system had been dominant for some time but was beginning to flounder, and what has been described as psychology's "Age of Theory" (Smith, 1986) had begun its decline. Taking his cue from Mach, Skinner addressed fundamental scientific issues in his contribution to the theory debate. In this sense, his analysis continues to illuminate issues surrounding mechanistic and relational explanatory systems in psychology.

Description, Observation, Integration

Despite some contrary interpretations, it is clear from the excerpts cited above that Skinner was committed to the development of a theory of behavior. But the term *theory* is somewhat ambiguous, because it can carry at least three meanings, two of which involve speculation, with the third involving integration. A theory may be simply a guess—a predictive or explanatory guess such as "I have a theory that such and such will happen," or "I have a theory that this is caused by that," in which the speaker is guessing an outcome or suggesting a causal relation. *Theory* may refer to a model involving one or more hypothetical entities constructed in an attempt to account for mysterious, unexplained phenomena. In this sense also, the term involves the notion of speculation or guessing. This type of theory amounts to a tentative explanation that, in science at least, is usually required to be submitted to experimental test in order to establish to what degree the model fits empirical data. *Theory* also may be used to refer to an explanatory system, such as Skinner's, that describes regularities, states general principles, and integrates uniformities in a given subject matter. These latter kinds of theories do not carry the same requirement to be submitted to experimental check, because they are "data-driven" (derived from observation) and are not constructed prior to experimentation. In this sense, integrative theories are not speculative: They describe without guessing. Theoretical terms in this type of explanatory system do not preempt experimentation, but are derived from it. In such a system, speculation does not take place at the level of explanation but at the level of experimentation, when an attempt is made to discover which out of the multiplicity of variables present in a given context may be functionally related. Explanations in this system do not refer to processes or entities beyond observation. Rather, the descriptions entailed in explanation are statements about observed regularities.

Skinner identified three stages of theory construction. The first and perhaps most important stage is to define the subject matter. The next step involves developing theoretical terms to express relations within the subject matter—integrative terms. "Observed relations of this sort are the facts of a science—or, when a sufficient degree of generality has been reached, its laws" (Skinner, 1947/1972, p. 307). As further regularities appear, theory construction proceeds to a third stage, which involves adding new theoretical terms to describe these new regularities. "Third stage concepts" (Skinner, 1947/1972, p. 307) are additions to the regularities expressed at the second stage without being additions to the basic data (subject matter). They emerge from regularities without invoking unobserved or hypothetical properties.

Skinner carefully defined his basic data, his subject matter, summarized in the term *behavior:*

> behavior is that part of the functioning of an organism which is engaged in acting upon or having commerce with the outside world . . . by behavior, then, I mean simply the movement of an organism or of its parts in a frame of reference provided by the organism itself or by various external objects or fields of force. It is convenient to speak of this as the action of the organism upon the outside world. (Skinner, 1938, p. 6)

The most significant part of this definition is "in a frame of reference"; the subject matter of Skinner's science of behavior (the data to be explained) is not muscle twitches or lever presses, it is relations between behavior and the world that the organism is acting upon—behavior in the context in which it occurs.

Having defined his subject matter, Skinner developed theoretical terms to describe those relations. *Conditioning* and *extinction,* for example, describe the shaping of behavior as a function of events in the context in which it occurs. *Operant behavior* refers to any behavior emitted by an organism that produces an effect, and an *operant* refers to a class of responses having a particular effect. In experimental situations with rats, for example, lever pressing is a class of responses having the effect of producing food. Topography is not important for experimental or explanatory purposes. A rat may press a lever with its paw, its foot, its nose, or its tail, but the topography of the operant is of less import than its relation to the context in which it occurs. In the case of humans, operants may be topographically more diffuse. Depending on the experimental question in hand, an operant can be anything from washing dishes to a violent verbal outburst, the common feature being that each is identified as a functional unit of behavior in relation to its context.

Skinner also developed descriptive terms for dependencies between setting conditions and consequences of behavior. *Discriminative stimulus* refers to a discrete aspect of the setting condition in which an operant occurs that is functionally related to that operant. Similarly, *reinforcer* refers to the effect of a discrete consequence on behavior. Several consequences may follow an operant, but not all may be functionally related to it. A reinforcing consequence is one that shapes or maintains an operant. In the case of *discriminative stimulus* and *reinforcer,* as in the case of *operant,* intrinsic properties of objects or events are of less import than the functional relations that those terms describe. A red light is not a discriminative stimulus because it is red, but because it is functionally related to an operant. Candy is described as a reinforcer if and when it shapes and maintains behavior; not because it is sweet, but because it is functionally related to an operant.

At the second stage in the construction of his system, Skinner developed integrative terms to express observed relations in the subject matter. Those terms are derived from the subject matter itself and do not appeal to assumed properties or entities beyond those that are empirically given. Relations between discriminative stimuli, operants, and reinforcers may be expressed as a function of time, of rate of response, of magnitude of reinforcement, of rate of reinforcement, of availability of alternatives, of the presence of verbal behavior, and so on, and it is with the identification of new relations that third-stage concepts emerge. For example, early in his experimental career, Skinner observed that response rate and rate of reinforcement were related across a broad range of settings. New terms were added to integrate these observations: *Variable interval, variable ratio, fixed interval, fixed ratio, concurrent,* and *differential reinforcement of low rate* all describe patterns in rates of reinforcement that are reliably related to patterns and rates of behavior. Third-stage concepts emerge out of second-stage relations without changing anything in the first stage, the basic data. Basic data are maintained even as theoretical terms are broadened, and those terms always describe observed regularities.

Nothing in the Skinnerian system is conveniently invented or modified to account for data. Behavior is explained by describing functional dependencies within the data rather than by invoking unobserved properties or entities. When he set out his views on appropriate interpretive techniques Skinner stated that

> [a theory] has nothing to do with the presence or absence of experimental confirmation. Facts and theories do not stand in opposition to each other. The relation, rather, is this: theories are based upon facts; they are statements about organizations of facts. (Skinner, 1947/1972, p. 302)

The theoretical terms of his explanatory system are empirically derived (i.e., derived by observation) and do not stand or fall on experimental confirmation. Neither does the theoretical system as a whole stand or fall on experimental confirmation, because only *observed* regularities form the linguistic basis of the system. Like Mach, Skinner was concerned with the way in which particular interpretive techniques (theories) divert attention toward the structure, function, or activity of the hypothetical entities offered as explanatory accounts. In such theories, the person fades from prominence as interest focuses instead on aspects of complex, hypothetical structures and mechanisms. Skinner's opposition to the mechanistic thinking underlying these kinds of theories (which I refer to here as *models* or *theoretical models* for the purpose of greater clarity) was based on the Machian view of cause and effect as functional dependency. Like Mach, Skinner found no discomfort in integrating cause and effect (functional relations) without a mediating structure or mechanism through which action takes place. According to Skinner's view, behaving organisms, human or otherwise, are not mechanical structures to be likened to telephone exchanges and networks, information processing systems, computer storage banks, and so on. They are biological organisms that operate within a context that affects their behavior and which they in turn have an effect upon and that are changed by their experience in that context. As Mach before him rejected a view of the world as a great machine, so Skinner rejected machine analogies in his scientific system and eschewed mechanical principles for describing organisms in their worlds.

Another feature of Skinner's opposition to models including terms not derived from data was a practical argument. Theoretical models require experimental testing to establish the best fit between competing models and data, and as such, they are wasteful of valuable energy and resources. Data obtained from testing models are only considered valuable if they conform to predictions deduced from the model, if they are positively in favor of the predictions. If results do not conform to predictions, the model is either rejected or undergoes extensive modification leading to further experimentation, but the data themselves are useful only insofar as they demonstrate problems in the model.

Because the process of constant experimental check usually leads to the decline of one model and the rise of another (either a modified model or a competing one), a large part of the research associated with the testing of models is discarded:

> Research designed with respect to theory is also likely to be wasteful. That a theory generates research does not prove its value unless the research is valuable. Much useless experimentation results from theories, and much energy and skill are absorbed by them. Most theories are eventually overthrown, and the greater part of the associated research is discarded. (Skinner, 1950/1972, p. 71)

Mechanistic Thinking in Psychology

As noted, much of contemporary psychology continues Tolman's tradition of providing mediating events between environmental input and behavioural output, the S–O–R template. Links between functional dependencies are often characterized as systems or mechanisms, and otherwise as less tangible components such as *mind, intentions, beliefs, attitudes, attributions, stress, knowledge, theory of mind,* and so on. Mechanistic thinking pervades scientific psychology's explanatory systems.

The input–system–output view of environment, persons, and their behavior, and the consequent division of persons into multiple internal faculties or components, has been opposed from a number of directions and for a number of reasons. Costall (1984), for example, has drawn attention to the compatibility between J. J. Gibson's (1966, 1979) argumentative strategies in favor of an active–interactive perspective on visual perception and those of B. F. Skinner:

> Both insist that behavior presents a primary datum for psychology which is not to be treated as a mere symptom of underlying structures of either the cognitive or physiological kind. They recognize that the description of behavior is nevertheless difficult, and they promote a molar and functional classification of behavior rather than muscle-twitch psychology or classical reflexology. (Costall, 1984, p. 114)

He noted also that Gibson's ecological perspective and Skinnerian accounts are committed to the view that relations between organisms and their worlds can only be linguistically expressed interdependently, that causal events cannot be identified independently of the relation itself (p. 113).

From another perspective, Neisser (1982) opposed the reification of a hypothetical construct, *memory,* on the grounds that it has diverted researcher's attention from such variables as the conditions under which people remember, the kinds of details they remember, and the ways in which they use aspects of the past (memories) in present settings. Theories of memory have become self-sustaining, according to Neisser. Detailed and elegant memory theories and their accompanying experiments allow researchers to demonstrate their substantial methodological skills. Nevertheless, memory research has contributed little that is new to our overall understanding of remembering: "The opposite is more often the case. When a particular experimental result seems to contradict an established principle, a dozen psychologists leap into the breach to restore it" (Neisser, 1982, p. 7). In addition to advocating the study of natural contexts and everyday remembering in preference to the static, structural, and traditional memory experiment, Neisser suggested that the concept of memory itself belongs to a traditional view of the subject matter of psychology that may be less useful now than it had once seemed.

> [Memory] is a concept left over from a medieval psychology that partitioned the mind into independent faculties: 'thought' and 'will' and 'emotion' and many others, with 'memory' among them. Let's give it up and begin to ask our questions in different ways. (p. 12)

Watkins (1990) concurred with Neisser in the view that, although memory research has generated a vast literature of elegantly designed and executed experimentation, memory theorizing has done little to improve our understanding of remembering. Watkins like Skinner, approached the issue of theorizing from a philosophy of science perspective, arguing that the crucial flaw in the field of memory research, the flaw that generates a plethora of theories, is *mediationism:*

> I believe that the sorry state of memory theorizing is a direct result of adopting the mediationist doctrine. Were we to disregard the doctrine, we would be less prone to indulge in personal theorizing, we would rid ourselves of the essential cause of our communication difficulties, and we would clear the clutter that more than anything else stands in the way of a better understanding of the nature of memory. (p. 329)

Watkins further noted that the appeal of mediationism is closely allied to the appeal of mechanism as a mode of explanation: "The memory trace bridges a temporal gap between an event and its recall and thereby provides an accounting of memory in mechanistic terms, without recourse to the concept of action at a distance" (p. 334). Memory theorizing thus continues to provide a clear focus for debate on issues of scientific frameworks and the usefulness of mechanistic accounts. Indeed, memory theorizing—its process, form, and function—provides a clear example of the scientific concerns of both Mach and Skinner.

Whereas Mach cautiously allowed for a heuristic function of theories containing hypothetical entities, he consistently argued against elevating such theories to the status of explanation because nothing is described by the linguistic terms referring to hypothetical entities. Once such theories have assisted in establishing previously unknown functional relations, the additional linguistic terms should be abandoned in favor of terms that describe those relations. Skinner was less sympathetic to theoretical models on the grounds that they divert attention from the behavior they are attempting to explain and that they are wasteful of energy and resources, as theoretical models normally decline with the production of new evidence and the rise of competing models.

The model of "working memory" (Baddeley & Hitch, 1974), for example, may well have functioned as one of Mach's "tools of thought," as a useful heuristic for establishing previously unknown functional relations. Indeed, the model has experimentally established several important functional relations. The *phonological similarity effect* is a relation between phonological properties of letter sequences and people's ability to reproduce letter sequences—"the more phonologically similar the sequence is, the harder the subject will find it to reproduce the sequence" (Baddeley, 1982, p. 415). The *word length effect* is a relation between the length of words in a sequence and people's ability to reproduce the sequence—"Memory span for words is a simple function of the spoken duration of the constituent words" (p. 415). The *unattended speech effect* is a relation between the simultaneous presentation of visual and aural material and people's ability to recall the visually presented material—"If a subject is required to remember a sequence of visually presented items, then his performance will be markedly impaired if irrelevant material is spoken at the same time" (p. 415). *Articulatory suppression* (dispensing with references to subvocal rehearsal and memory span) is a relation between material presented to a subject at the same time he or she is required to speak and the subject's ability to report the material presented—"If subjects are prevented from subvocally rehearsing material by requiring the subject to utter some irrelevant speech sounds such as the word 'the' then their immediate memory span is impaired" (p. 415).

Four demonstrated functional relations can be described in the following terms: Remembering is a function of phonological similarity; remembering is a function of word length; remembering is a function of simultaneous presentation of different kinds of material; and remembering is a function of simultaneous presentation of material and speaking. Terms such as *central executive, visuo-spatial scratchpad,* and *articulatory loop* (Baddeley & Hitch, 1974) are unnecessary in describing these relations, because what or how much a person remembers is shown to be a direct function of aspects of the stimulating environment (word lengths, list lengths, phonemic similarities, acoustic similarities, simultaneous listening and speaking, etc.), and no amount of additional, internal, theoretical references alters those relations. If the terms of the model are allowed to fall away in the expression of functional relations, then Mach would not have disputed its usefulness and would have applauded its ability to lead to the establishment of new relations. Furthermore, as the concept of explanation was understood by Mach and Skinner, the maintenance of these additional references contributes nothing to a scientific explanation of remembering. Their maintenance, as Watkins (1990) has noted, satisfies a mechanistic conception of explanation by providing mediating links between functional relations and by bridging temporal gaps between independent and dependent variables.

Additional theoretical terms become particularly irrelevant in applied settings (i.e., in contexts in which behavior has somehow "gone wrong" or in which efforts are made to strengthen or weaken desirable or undesirable behavior, respectively) for, as a simple matter of logic, how does a clinician, educational psychologist, or teacher manipulate hypothetical components? For example, examining the processes of normal and dyslexic reading, Baddeley (1982) found that poor readers "show much less evidence of the influence of phonemic similarity than do good readers, *suggesting that they are not fully utilizing the articulatory loop*" (p. 416, italics added). When a group of dyslexic boys demonstrated the functional relations described above as the *phonological similarity effect,* the *word length effect,* and *articulatory suppression,* Baddeley concluded that "this indicates they were indeed *using the articulatory loop,* but does not necessarily mean that the system was functioning as efficiently as in normals" (p. 416, italics added). In this case, the theoretical component does not fall away in the expression of functional relations, but is referred to as a defective ontological system. Aside from the philosophical difficulties involved in granting ontological status to hypothetical components, how is reading capability to be strengthened if the source of the problem is said to lie in a malfunctioning articulatory loop? How would a clinician, educational psychologist, or teacher restore to full and proper functioning a hypothetical component?

This complex, multicomponent model was developed because an older and simpler model—Atkinson and Shiffrin's (1968) two-component model—could not account for "the plethora of information processing capabilities of complex organisms, especially humans" (Morris, 1986, p. 281); since its original formulation, the model has been updated on a number of occasions (Morris, 1986). The components of the model, like the human behavior they are said to account for, are extremely complex in both their form and function, and the model is summarized in the following way:

> The central executive which formed the control centre of the system was assumed to select and operate various control processes. It was assumed to have a limited amount of processing capacity, some of which could be devoted to the short-term storage of information. It was able to offload some of the storage demands onto subsidiary slave systems of which two were initially specified, namely the Articulatory Loop, which was able to maintain verbal material by sub-vocal rehearsal, and the Visuo-Spatial Scratch Pad, which performed a similar function through visualization of spatial material. (Baddeley, 1981; cited in Morris, 1986, p. 281)

What is of interest here is not so much the form or function of the components of the model, as it is the development and function of the model itself. A two-component model has developed into a more complex model comprising at least three components as the data on human remembering have become more complex, and a review of research generated by the model (Morris, 1986) is concerned primarily with implications for structural aspects of the model's components. For example, statements such as "The slave systems have proved to be more complex entities than was at first imagined, and their number is proliferating" (p. 293), and "The future of working memory seems to be heading towards further fractionation of the system" (p. 293) refer to unimagined complexity of aspects of the model rather than to the complexity of behavior (in this case, remembering). The model has taken on characteristics of the behavior it was invented to explain, and the behaving person has been relegated to secondary status relative to the model. The hypothetical nature of the model's components allows for them to be modified and multiplied at will, taking on whatever features or properties the scientist decides. As Watkins (1990) has noted, "Mechanistic theories are neither compelled nor constrained by the data" (p. 334).

Linguistic terms referring to hypothetical constructs continue to be retained even after the establishment of functional relations. The model described provides a useful illustration of Mach's and Skinner's concerns regarding the concepts of cause, explanation, and theory in scientific practice. This model, however, is only one example of how mechanistic theorizing in psychology can result in a proliferation of personal theorizing, can lead to a focus on the structure and function of hypothetical components, and thus to a relegation of behavior and the behaving person, and can hamper the development of practical techniques for changing problematic behavior and strengthening weak behavior.

Conclusion

Mach's participation in the 19th-century dispute over interpretation and causal modes in physics was echoed by Skinner's natural science concerns in 20th-century psychology. It has been pointed out that Mach's concerns relating to mechanistic explanatory systems were not concerns over the ontological status of hypothetical constructs (e.g., atoms, vortices, particles, and other mediating entities postulated by physicists of his time) but were broader scientific, philosophical, and methodological concerns (Brush, 1968). Similarly, debates over psychological structures or events are often mistaken today for ontological disputes—such as whether memory, mind, and mental states in general exist. However, when viewed from a philosophy-of-science perspective, these debates take on a new form. They become debates about the meaning of explanation, about the conception of causation employed, and about the pragmatic value of theories and theoretical models.

Radical behaviorism's explanatory system focuses on relations between behaving persons (or other organisms), the setting conditions of behavior, and its consequences—behavior in its context. Persons are illustrated in this system as indivisible wholes, active in and interactive with their environments, changing and changed by the context and consequences of their behavior—a concept identical to Capra's (1983) "network of dynamic relationships" (p. 32) in the world of contemporary physics. Relations between organisms and their world are the focus of causal accounts, expressed in integrative theoretical terms that explain behavior over time without the need for mechanistic links between functionally dependent events.

A commitment to scientific method continues to distinguish psychology from philosophy, a view from which few psychologists would dissent. Clearly, however, psychologists are not all committed to the same kind of science. Skinner's commitment was to a descriptive, observational, and integrative science that did not require mediating structures or mechanisms to account for cause–effect relationships, that was informed by a relational view of its subject matter (contained in its definition) and a philosophy that did not separate persons into behavior and internal systems, and that sought to describe (explain) how persons and environments interact, the effect that persons have in producing consequences in their environment, and the effect the environment has in shaping and maintaining behavioral repertoires. Few psychologists are committed to this kind of science, for the majority conceive of causation as contiguous and sequential, demanding of links in the chain to account for behavior.

If psychologists are to heed the call for a new scientific framework, and relational accounts are to be a goal for modern psychology, then a careful examination of Skinner's philosophy of science can assist in the conceptual analysis required. Throughout his lifetime, B. F. Skinner continually confronted the task of developing empirically valid methods and terms to explain behavior. He consistently advocated a relational framework for the subject matter of psychology, thoroughly and patiently working out how this might be achieved within a scientific tradition. Radical behaviorism today provides a stable and coherent philosophical position within psychology. Its technological applications continue the relational, integrative, contextual scientific program advocated by Skinner. The success of his methods across a broad range of applied settings is the harvest of his careful labor.

REFERENCES

Atkinson, R. C., & Shiffrin, R. M. (1968). Human memory: A proposed system and its control processes. In K. W. Spence & J. T. Spence (Eds.), *The psychology of learning and motivation: Advances in research and theory.* (Vol. 2, pp. 89–195). San Diego, CA: Academic Press.

Baddeley, A. D. (1981). The concept of working memory: A view of its current state and probable future development. *Cognition, 10,* 17–23.

Baddeley, A. D. (1982). Reading and working memory. *Bulletin of the British Psychological Society, 35,* 414–417.

Baddeley, A. D., & Hitch, G. J. (1974). Working memory. In G. H. Bower (Ed.), *Recent advances in learning and motivation* (pp. 47–89). San Diego, CA: Academic Press.

Bandura, A. (1977). Self-efficacy: Toward a unifying theory of behavioral change. *Psychological Review, 84,* 191–215.

Bandura, A. (1989). Human agency in social cognitive theory. *American Psychologist, 44,* 1175–1184.

Blackmore, J. T. (1972). *Ernst Mach: His work, life, and influence.* Berkeley: University of California Press.

Bower, G. H., & Hilgard, E. R. (1981). *Theories of learning* (5th ed.) Englewood Cliffs, NJ: Prentice-Hall.

Bradley, J. (1971). *Mach's philosophy of science.* London: Athlone Press.

Brush, S. G. (1968). Mach and atomism. *Synthese, 18,* 192–215.

Capra, F. (1975). *The Tao of physics: An exploration of the parallels between modern physics and Eastern mysticism.* London: Flamingo.

Capra, F. (1983). *The turning point: Science, society and the rising culture.* London: Fontana.

Cohen, M. R., & Nagel, E. (1955). *An introduction to logic and scientific method.* London: Routledge & Kegan Paul.

Cohen, R. S., & Seeger, R. J. (Eds.). (1970). *Ernst Mach physicist and philosopher* (Boston Studies in the Philosophy of Science, Volume VI). Dordrecht, The Netherlands: Reidel.

Costall, A. P. (1984). Are theories of perception necessary? A review of Gibson's *The ecological approach to visual perception. Journal of the Experimental Analysis of Behavior, 41,* pp. 109–115.

Davies, P. (1987). The cosmic blueprint. London: Heinemann.

Feyerabend, P. K. (1970). Philosophy of science: A subject with a great past. In R. H. Stuewer (Ed.), *Historical and philosophical perspectives of science* (Minnesota Studies in the Philosophy of Science, Vol. 5, pp. 172–183). Minneapolis: University of Minnesota Press.

Gibson, J. J. (1966). *The senses considered as perceptual systems.* Boston: Houghton Mifflin.

Gibson, J. J. (1979). *The ecological approach to visual perception.* Boston: Houghton Mifflin.

Gleick, J. (1988). *Chaos: Making a new science.* London: Heinemann.

Hanson, N. R. (1955). Causal chains. *Mind, 64,* 289–311.

Hanson, N. R. (1958). *Patterns of discovery: An inquiry into the conceptual foundations of science.* Cambridge, England: Cambridge University Press.

Hawking, S. W. (1988). *A brief history of time: From the big bang to black holes.* London: Bantam Press.

Hempel, G., & Oppenheim, P. (1960). Studies in the logic of explanation. In E. H. Madden (Ed.), *The structure of scientific thought* (pp. 19–29). Boston: Houghton Mifflin.

Hillix, W. A., & Marx, M. H. (Eds.). (1974). *Systems and theories in psychology: A reader.* St. Paul, MN: West.

Hume, D. (1975). *Enquiries concerning human understanding and concerning the principles of morals* (3rd ed. rev., notes by P. H. Nidditch). Oxford England: Clarendon Press. (Original work published 1777)

Kline, M. (1980). *Mathematics: The loss of certainty.* New York: Oxford University Press.

Kline, M. (1985). *Mathematics and the search for knowledge.* New York: Oxford University Press.

Kuhn, T. S. (1962). *The structure of scientific revolutions* (2nd ed.). Chicago: University of Chicago Press.

Lakatos, I., & Musgrave, A. (Eds.). (1970). *Criticism and the growth of knowledge.* Cambridge, England: Cambridge University Press.

Laudan, L. (1981). Ernst Mach's opposition to atomism. In L. Laudan, *Science and hypothesis* (pp. 202–225). Dordrecht, The Netherlands: Reidel.

Lee, V. (1988). *Beyond behaviorism.* Hillsdale, NJ: Lawrence Erlbaum.

Mach, E. (1960). *The science of mechanics: A critical and historical account of its development.* Peru, IL: Open Court. (Original work published 1893)

Merchant, C. (1982). *The Death of nature: Women, ecology and the scientific revolution.* London: Wildwood House.

Morris, E. K., Higgins, S. T., & Bickel, W. K. (1982). Comments on cognitive science in the experimental analysis of behavior. *The Behavior Analyst, 5,* 109–125.

Morris, N. (1986). Working memory, 1974–1984: A review of a decade of research. *Current Psychological Research & Reviews, 5,* 281–295.

Neisser, U. (1982). *Memory observed: Remembering in natural contexts.* San Francisco: Freeman.

Prigogine, I., & Stengers, I. (1985). *Order out of chaos: Man's new dialogue with nature.* London: Fontana.

Russell, B. (1946). *A history of Western philosophy.* London: Allen & Unwin.

Scriven, M. (1956). A study of radical behaviorism. In H. Feigl & M. Scriven (Eds.), *The foundations of science and the concepts of psychology and psychoanalysis,* (Minnesota Studies in the Philosophy of Science, Vol. 1, pp. 88–130). Minneapolis: University of Minnesota Press.

Skinner, B. F. (1972). The concept of the reflex in the description of behavior. In B. F. Skinner, *Cumulative record: A selection of papers,* (3rd ed., pp. 429–457). Des Moines, IA: Meredith. (Original work published 1931)

Skinner, B. F. (1938). *The behavior of organisms.* New York: Appleton-Century-Crofts.

Skinner, B. F. (1972). Current trends in experimental psychology. In B. F. Skinner, *Cumulative record: A selection of papers* (3rd ed., pp. 295–313). Des Moines, IA: Meredith. (Original work published 1947)

Skinner, B. F. (1972). Are theories of learning necessary? In B. F. Skinner, *Cumulative record: A selection of papers* (3rd ed., pp. 69–100). Des Moines, IA: Meredith. (Original work published 1950)

Skinner, B. F. (1953). *Science and human behavior.* New York: Macmillan.

Skinner, B. F. (1956). Critique of psychoanalytic concepts and theories. In H. Feigl & M. Scriven (Eds.), *The foundations of science and the concepts of psychology and psychoanalysis* (Minnesota Studies in the Philosophy of Science, Vol. 1, pp. 77–87). Minneapolis: University of Minnesota Press.

Skinner, B. F. (1972). The flight from the laboratory. In B. F. Skinner, *Cumulative record: A selection of papers* (3rd ed., pp. 314–330). Des Moines, IA: Meredith. (Original work published 1961)

Skinner, B. F. (1969). *Contingencies of reinforcement: A theoretical analysis.* Englewood Cliffs, NJ: Prentice Hall.

Skinner, B. F. (1972). A lecture on "Having" a poem. In B. F. Skinner, *Cumulative record: A selection of papers* (3rd ed., pp. 345–355). Des Moines, IA: Meredith.

Skinner, B. F. (1978). *Reflections on behaviorism and society.* Englewood Cliffs, NJ: Prentice-Hall.

Skinner, B. F. (1984). Selection by consequences. *The Behavioral and Brain Sciences, 7,* 477–481, 502–510.

Smith, L. D. (1986). *Behaviorism and logical positivism: A reassessment of the alliance.* Stanford, CA: Stanford University Press.

Stewart, I. (1989). *Does God play dice?* Oxford, England: Basil Blackwell.

Tolman, E. C. (1922). A new formula for behaviorism. *Psychological Review, 29,* 44–53.

Tolman, E. C. (1959). Principles of purposive behavior. In Sigmund Koch (Ed.), *Psychology: A study of a science* (Vol. 2, pp. 92–157). New York: McGraw-Hill.

Watkins M. J. (1990). Mediationism and the obfuscation of memory. *American Psychologist, 45,* pp. 328–335.

Westby, G. (1966). Psychology today: Problems and directions. *Bulletin of the British Psychological Society, 19,* 1–19.

Woolgar, S. (1988). *Science: The very idea.* Chichester, England: Horwood.