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Précis of *Behaviorism: A conceptual reconstruction*

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Abstract: The conceptual framework of behaviorism is reconstructed in a logical scheme rather than along chronological lines. The resulting reconstruction is faithful to the history of behaviorism and yet meets the contemporary challenges arising from cognitive science, psycholinguistics, and philosophy. In this reconstruction, the fundamental premise is that psychology is to be a natural science, and the major corollaries are that psychology is to be objective and empirical. To a great extent, the reconstruction of behaviorism is an elaboration of behaviorist views of what it is for a science to be objective and empirical. The reconstruction examines and evaluates behaviorist positions on observation and the rejection of introspection, the behavioral data language, theory construction, stimulus–response psychology, the organization of behavior, complex processes, agency, and the interpretation of mentalistic language. The resulting reconstruction shows behaviorism to be a pragmatic psychological version of positivism based on a behavioral epistemology.

Keywords: behaviorism; cognitivism; conditioning; epistemology; history of science; introspection; mind; philosophy of science; theory-construction

General overview. After dominating American psychology for nearly a half century, behaviorism today finds itself on the defensive. New and competing approaches to psychology have arisen in psycholinguistics, cognitive science, and philosophy. A reformulation of behaviorism is called for, one that takes into account these challenges.

Behaviorism is not the science of behavior developed by behaviorists. It is, rather, the conceptual framework underlying that science. First, it is a philosophy of science dictating standards for posing psychological questions and for the methodology, explanations, and psychological theory involved in answering them. Second, behaviorism is a philosophy of mind that makes certain assumptions about human nature and the working of the mind. Third, there are several very general empirical hypotheses that constitute a background theory for all behavioral theories. Fourth, behaviorism is an ideology, recommending goals for behavioral science and its application.

In this conceptual reconstruction, the entire scope of behaviorism, from roughly 1910 to the present, is considered. The reconstruction is organized around conceptual issues rather than historical periods. The conceptual framework of behaviorism is elaborated as a logical rather than a chronological development. The reconstruction begins with a few fundamental premises, but because their implications can be developed in more than one way, the framework is organized like a branching tree diagram. Each node of the tree represents a conceptual choice point, often generated by a criticism of behaviorism or by the application of behaviorist analysis to a new question. Each branch growing from a node symbolizes a different conceptual decision. A major purpose of this reconstruction is to prune the behaviorist tree diagram of weak positions and to note or, when necessary, create critically sound paths through it.

Because there is no set of necessary and sufficient conditions that identify behaviorist ideas, a more fruitful approach is to view the various behaviorist positions as sharing a family resemblance: A set of overlapping features, some related by ties of similarity and some by historical association.

The first premise in the conceptual reconstruction of behaviorism is that psychology is a natural science. Two important corollaries are: (1) Science, and psychology in particular, must be empirically based; (2) science, and psychology in particular, must be objective. The meaning of "objective," and "empirical," for behaviorism, as well as the meaning of "science," is apparent only in the context of the full reconstruction of behaviorism.

Observation: The case against introspection. The first node in the behaviorist tree diagram is the rejection of introspection, the internal observation of one's own consciousness, as a method of observation for psychology. The branches leading from this node represent the reasons for this rejection. One behaviorist objection to introspection is that it is especially prone to error. However, proponents of introspection reply that it can be as objective as any other kind of observation when carried out properly. A second objection is that introspection's subject matter, consciousness, is not objective. This criticism, however, either begs the question or is based on unsupported metaphysical notions of objectivity.

A third objection is that introspective evidence and observations are private, that is, available to only one observer. For evidence and observations to be objective, they should be available to, and verifiable by, any number of observers. Thus, "private" is equated with "subjective," and "public" is identified with "objective."

Proponents of introspection offer two replies. First,

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they argue that although introspective reports cannot be verified by shared observation, they can be verified indirectly. For example, although Smith might have been the only person to observe a meteor strike earth, his report can be indirectly verified by, for example, finding ash at the site. By analogy, Smith's introspective observations can be verified indirectly from evidence about Smith's prior and subsequent behavior, his accuracy in reporting other kinds of observations, and the reports of other introspectors.

This reply by the introspectionist, however, is based on a false analogy between indirect confirmation in cases of observations which can in principle be public but happen not to have been (e.g., Smith's observation of the meteor) and observations which cannot in principle be public (e.g., Smith's introspection of his feelings of hunger). The two differ fundamentally with regard to the relationship between the evidence and the report it supports. With public observations this relationship is based on empirically determined regularities that are themselves established by public observations (e.g., ash and meteors). With private introspections, however, this is not the case. For Smith's publicly observed eating behavior to serve as an indirect verification of his report of feeling hunger, it is necessary to establish a correlation between the two. Hunger, rather than mere reports of hunger, must be observed and correlated with behavior. However, by hypothesis, hunger can be observed only by private observation, and the validity of such observation is precisely the question at issue.

A second reply to the behaviorist objection that introspection is private and therefore subjective is to argue that all observation is private. An introspectionist can claim that knowledge of the external world is really an inference from the immediate but private percept. Verification is not achieved through shared public experience but rather through the congruence of private experiences.

Some behaviorists grant this introspectionist claim but substitute a methodological distinction for the original public–private distinction. They reason that science is ultimately a social enterprise, with its success heavily dependent on communication and cooperation within a community of scientists. A key element is the choice of a data base that affords the greatest possible degree of agreement and communication among observers. Reports about the physical world meet this criterion whereas reports about phenomenal experience do not. Thus, a fourth objection to introspection is that it does not achieve intersubjective agreement. Public/private becomes intersubjective/subjective.

Some behaviorists view this failure to achieve intersubjective agreement to be a contingent empirical fact. Others view it as a necessary truth. According to the latter, the introspector's observational reports are formulated in a private language (i.e., one that refers exclusively to private experience), and a private language is not logically possible.

A fifth objection to introspection is that it is unreliable. Behaviorist theories suggest how a verbal community trains its members to respond discriminatively to private internal stimuli by making use of public stimuli tied to the private stimulation. However, because the correlation between the public and private stimuli is not perfect, the verbal community cannot ensure a rigid discriminative relationship between the verbal response and the private stimuli. Therefore, the link between introspective reports and internal stimuli is not a reliable one, and introspection is not an objective method of observation.

The behavioral data language. The next major node in the behaviorist conceptual tree diagram is the selection of a domain for psychology. Behaviorists generally support molar behaviorism, the position that an autonomous science of behavior independent of physiology is both possible and desirable. Behaviorists differ, however, in distinguishing between behavior and physiological events. Despite numerous behaviorist attempts to define this boundary, it remains elusive. A more promising approach is to abandon such a priori distinctions and to seek definitions dependent on actual developments within a science of behavior. With advances in behavioral research, laws are discovered that govern not only the original paradigms of behavior, but a wider range of phenomena as well. This range, then, determines a scientific domain, with "behavior" defined as whatever conforms to these laws. At a more advanced stage, laws are organized by comprehensive psychological theories, and the terms of the science are extended to whatever phenomena are covered by a theory.

Behaviorist advocacy of an autonomous science of behavior independent of physiology rests on a behaviorist family trait: the belief that the goals of scientific psychology are the prediction and control of behavior. Autonomous behavioral laws achieve these purposes because the variables entering them are both relatively easily observable and manipulable, as compared with physiological events. Furthermore, many behaviorists fear that a concern with physiology tends to divert attention away from behavior and its environmental causes. Others argue that physiological psychology often finds a proximate cause for behavior but then leaves this cause unexplained, giving the impression that it is brought about by an agent or act of will hidden within the central nervous system.

The next node concerns the selection of a descriptive language in which to express observational reports. Some epistemologists argue that no datum is purely empirical because all observation is contaminated by theory, thereby undermining the behaviorist search for an empirical and objective data base. Some behaviorists respond by arguing that although it may be true that theory influences observation, this effect can be overcome by careful scientific observation. [p. 35–37; all page numbers in square brackets refer to pages in *Behaviorism: A conceptual reconstruction* where references to the material discussed can be found.] Indeed, it is difficult to evaluate the epistemological thesis on this point. To demonstrate that there are instances in which observation is theory laden or that rival theories are incommensurate, it is necessary to use either theory-neutral facts, which the thesis denies, or the theory-laden facts of yet another theoretical paradigm. Moreover, if the thesis is itself a paradigmatic theory, then it, too, will dictate observations of scientific activity confirmatory of itself.

Although the arguments for the thesis are therefore not conclusive, its antithesis, namely that there is a clear and sharp distinction between the observational and the theoretical, is equally difficult to defend. Because perception and report are necessarily conceptual, observation cannot be entirely independent of knowledge and belief. However, it should be possible to establish a continuum based on the degree to which knowledge and belief contribute to a data report. Surely there is a difference between the report "Smith ran five miles" and the report "Smith unconsciously tried to impress his friends with his running" as descriptions of the same event. No metric for precise measurement along this continuum currently exists. However, in practice, many behaviorists tend to use intersubjective agreement as a convenient index. Descriptions that command universal assent from observers are at one end of the continuum while those generating much disagreement are at the other. At some imprecise location in between, the description is said to be inferential, interpretative, or theoretical rather than observational. Even if the degree of agreement reflects merely the degree to which observers share a common paradigm, this relativization of objectivity and "empiricalness" to a paradigm is still not particularly damaging to behaviorism. According to the thesis, relativity is necessarily true for all of science, and the science of behavior can proceed as normal science.

Behaviorists attempt to characterize the kinds of observational reports that achieve the required degree of intersubjective agreement. Some suggest that the behavioral data language be restricted to physical descriptions of behavior and the environment. However, in practice, this restriction is difficult to observe, and no behaviorist has ever implemented it. Moreover, the restriction is based on the erroneous notion that the properties used in physics are inherent in nature and therefore objective. On the contrary, there are many systems for classifying the world, and those of the natural sciences are only a small subset of them. The properties attributed to nature are always relative to identity criteria created by humans. Criteria for all properties, including those of physics, depend ultimately on human pattern recognition. In no sense does one particular set of properties, rather than another, describe nature "as it really is." A behavioral science can therefore use a broad range of properties, including those that are not the standard properties of the other sciences yet achieve an acceptable degree of intersubjective agreement.

Because of these considerations, some behaviorists argue that behavior should be described as movements rather than as actions [ch. 3]. However, in practice, most behaviorists use action language almost exclusively in describing behavior. Action-neutral descriptions of behavior are difficult to formulate, and action language is therefore used for convenience. There is a tradeoff between observational purity and usefulness. Also, it can be argued that the criteria for describing a movement as an action are objective and observable, and therefore that action language is merely a higher-order description of what is observed. Indeed, some have argued that these higher-order descriptions are so basic that we normally *see* behavior as actions rather than as movements, and further interpretation is unnecessary.

Most behaviorists reject both intensional and purposive language for the behavioral data language. They argue that intensional and purposive descriptions are highly interpretative and therefore do not achieve the required degree of intersubjective consensus. Furthermore, they contend, the manifest purposive and intensional qualities should not be taken as fundamental but should instead be explained as the result of more basic properties of behavior. On the other hand, purposive behaviorists maintain that intensional and purposive qualities of behavior are emergent and belong among the basic descriptive properties of behavior [p. 45–47]. These purposive behaviorists argue that scientifically defined purposive and intensional concepts can be attributed to organisms on the basis of objective criteria, and intersubjective agreement can be maintained.

One strategy that behaviorists often adopt in making these judgments is to select predicates that sort behavior and the environment into classes experimentally found to be lawfully related. This "functional" approach can be illustrated by a schematic example [p. 48–53]. An experimenter begins with an initial class of behaviors defined by some set of features. Certain aspects of this response class (e.g., its frequency of occurrence) are found to be functionally related to certain aspects of the environment. By judicious variation of the environment and careful observation of the behavior, the experimenter can delineate a class of environmental events covered by the functional relationship. This class is a "functional stimulus class."

In exploring the limits of the functional relationship, the experimenter may find that the functional stimulus class controls a class of behavior not perfectly congruent with the initial class of behaviors. The experimenter then delineates the class of behaviors empirically found to be under the control of the functional stimulus class, thus determining a "functional response class." Further experimental study may suggest additional adjustments in the membership of the fucntional stimulus class and the functional response class. This process of titration eventually distills functional stimulus and response classes that optimize the lawfulness between behavior and the environment.

Once functional classes are selected, it is necessary to specify them. To ensure that the functional definition is not circular, it is necessary that the functional stimulus and response classes be specified independent of one another. Often these specifications will generate descriptive properties not used by the physical sciences. Once again, intersubjective agreement in using them to identify a class is an important criterion for their acceptability.

Theoretical concepts. To introduce terms outside the behavioral data language is to risk abandoning objectivity and "empiricalness." Behaviorists therefore insist that theoretical concepts be linked securely to the behavioral data language. They disagree, however, on the nature of this linkage, thereby forming the next major node.

One procedure to ensure that all concepts are linked to observations is offered by operationism [p. 58–63]. However, operationism does not provide satisfactory criteria for the individuation of operations. There are an infinite number of ways in which any two activities are similar and different. Without a criterion of individuation, there is no way to decide when to ignore the differences and classify the two activities as the same operation and when to ignore the similarities and consider the two as defining different concepts.

In practice it is only a posteriori that such distinctions are drawn. If the initial intuition is incorrect in that certain ignored differences are found to be important and the class is too broad, then the resulting empirical laws and correlations will not have the same simplicity and orderliness as a classification that distinguishes on the basis of those differences. Conversely, if the concept is too narrowly defined, then the resulting laws will display redundancy. Expansion of the classification results in the convergence of many laws into fewer but more general ones. There are no formal rules for constructing categories that maximize both simplicity and comprehensiveness. Operationism is therefore not an algorithm for concept formation. Furthermore, although an operational definition may be formulated at any given stage, it is subject to revision in so far as its criteria for individuation of operations are subject to change. Viewed in this way, operational definitions acquire the open-textured quality of reductive chains, which provide only partial definitions of concepts.

Few behaviorists are satisfied with a theoretical vocabulary restricted to operationally defined concepts. Most are willing to make use of intervening variables, concepts that represent a relationship between a set of interrelated independent variables and a set of interrelated dependent variables. Intervening variables are interpreted as merely labels for observed relationships and do not stand for unobserved entities, events, or processes in an organism's body or mind. Because intervening variables do not symbolize events or entities, it is a matter of choice how they are formulated, and in this sense they are conventions. Consequently, there is no unique set of valid intervening variables, just as there is no unique shorthand summary of observations. Similarly, they have no unique representation. They can be symbolized by concrete mechanical models, flow charts, or electrical fields.

Many behaviorists are willing to go beyond the conventionalism of the intervening variable and permit the introduction of hypothetical constructs, concepts explicitly intended to refer to unobserved events, entities, and processes within the organism. Proponents of hypothetical constructs argue that science, in fact, does not limit itself to intervening variables. Physics, for example, has introduced a vast array of theoretical terms referring to unobservables. In response, it can be argued that there are several reasons why hypothetical constructs are more vulnerable to misuse in psychology than in physics. First, psychology is nowhere near the axiomatization necessary for the introduction of theoretical terms through a postulate set. Second, as compared to psychology, physics is a far more mature and well-developed science. Third, unlike physics, psychology must contend with the fact that researchers bring to their theorizing a wealth of prescientific concepts about human action derived from their own experience.

A second argument against limiting the behavioral science to intervening variables is that in practice psychologists do not treat any concept as if it were an intervening variable. However, this argument is not conclusive because it is not clear whether the practices referred to indicate an underlying hypothetical construct or implicit *ceteris paribus* clauses in the definition of an intervening variable. These clauses cannot be exhaustively stated and require an intuitive grasp of the concept. Certainly they do not imply the existence of an unobserved construct [p. 74–75].

Yet a third argument in favor of hypothetical constructs is that they bridge the gap between psychology and physiology. However, this argument presupposes two questionable assumptions. First, it is not clear that hypothetical constructs are more efficacious for theoretical reduction than intervening variables. With a hypothetical construct, the possibility of attributing invalid properties is ever present. In contrast, a well-formulated intervening variable, in not exceeding observations, is more likely to correspond to a physiological construct than is a hypothetical construct with incorrect features. Second, the argument assumes that the subdivision of an organism into physiological systems that correspond to hypothetical constructs is a given rather than the consequence of human conceptualization. Neural "mechanisms" are created on the basis of a number of considerations, including functional relationships as well as anatomical connections. Therefore, physiological mechanisms are conventional in the same sense that intervening variables are.

A fourth objection to theories limited to intervening variables is that such theories are not truly explanatory. At best, it is argued, these black-box theories provide input–output laws but do not explain those laws. Only by reference to events inside the black box can behavior be explained [p. 76–77]. If by "explanation" the nomological-deductive pattern is meant, then this objection is incorrect. In black-box theories, particular instances of behavior are explained by deducing statements describing them from general behavioral laws in conjunction with statements about initial conditions. Instead, the objection may be that the general behavioral laws used in black-box deductive explanations are simply descriptive of observed regularities. They are stated as givens rather than as the results of the internal events mediating them.

It is true, as this argument notes, that black-box theories must have a few fundamental behavioral laws from which other laws are deduced. However, the argument is mistaken in its implicit belief that the situation can be otherwise. Even explanations referring to hypothetical constructs must ultimately appeal to fundamental laws which are left unexplained in the same sense that the fundamental behavioral laws are. It is always the case that the fundamental laws of one science are possibly the derived theorems of another more fundamental science.

A fifth objection to limiting the behavioral science to intervening variables is that to do so would impede progress. Numerous examples in the history of science demonstrate how unobservables were hypothesized to explain certain observations, and then later these hypothetical constructs were found, through direct observations, to have many of the hypothesized properties. The surplus meaning of hypothetical constructs enables a theory to integrate diverse observations which would not otherwise appear to be related. The attempt to characterize fully the hypothetical construct acts as a heuristic to organize and direct research.

These five arguments in favor of the inclusion of hypothetical constructs are strategic ones, stressing that such constructs will enhance scientific progress. Similarly, the case against hypothetical constructs is not that they are logically suspect but rather that they lead to consequences harmful to the behavioral science. It is feared that the use of hypothetical constructs will encourage unwarranted speculation, resulting in premature "physiologizing." Such constructs are less open to experimental test because properties can be attributed to them *ad hoc* to accommodate any experimental result, and they therefore provide only spurious explanations. Furthermore, they are dispensable, because lawful relations between behavior and the environment can be found independent of them. Their introduction therefore unnecessarily complicates a theory. Moreover, theorizing devoted to determining the properties of the hypothetical construct diverts attention toward inner mechanisms that are not readily manipulable and away from the environmental factors most important for the prediction and control of behavior. Again, the argument is tactical rather than philosophical.

Despite these arguments against hypothetical constructs, the majority of behaviorist theories do, in fact, include them. The most common are covert stimuli and responses, long-term physiological mechanisms thought to underlie behavioral states, and hypothesized overt stimuli and responses that have not actually been observed, identified, or recorded. To ensure that these constructs do not compromise the behaviorist commitment to objectivity and empiricalness, behaviorist hypothetical constructs generally conform to four conditions. First, the properties assigned to constructs do not differ substantially from those of observed stimuli and responses. Second, they are generally located peripherally rather than in the central nervous system. Third, they are typically thought to operate under the functional control of environmental variables, that is, their activities are not autonomous. Fourth, it is preferable that a hypothetical construct be linked by a functional relationship to an observable environmental variable, or an observable behavioral variable, or, ideally, to both.

Theorizing. Behaviorists create theories in a number of ways, each of which represents a branch from the next node. The two most influential methods are those of Hull and Skinner.

Hull's "hypothetical-deductive" technique (Hull 1943) cannot be understood in the same sense as it is used in logic and mathematics. Instead, Hull begins with a small number of empirical findings – response rate as a function of number of reinforced trials, for example. Although the number of reinforced trials is the only variable manipulated, it is obvious that the response rate is a function of a number of other variables, either kept constant or unknown. It is possible to formulate a "long equation" to speculate on how all these variables combined determine response rate. Because this long equation would be unwieldy for suggesting further hypotheses, Hull divides it up into smaller groupings, each of which defines a new quantity such as habit strength. Thus, one role of Hullian postulates is to define these new concepts as hypothesized quantitative functions of empirically defined variables. Another is to hypothesize how these theoretical variables combine to determine the final behavioral outcome.

For Hullians, the main purpose of a theory is the deduction of theorems about observables. These theorems can be experimentally tested, and if the results conform to the theorem, then the theory gains in its degree of confirmation. Lack of observed congruence with a theorem calls for revision of the theory. By this process of test and revision, a theory is developed that generates increasingly greater numbers of valid theorems, thereby organizing and guiding research.

Theorems may be understood as statements about either future or past behavior and how they are determined by environmental variables. They therefore play a critical role in the prediction, control, and explanation of behavior. This identification of explanation and prediction with logical deduction from a theory exemplifies behaviorist views on objectivity. Logical deductions from clearly stated postulates are more likely to command intersubjective agreement than explanations or predictions based on intuition or empathy.

In contrast to Hull, Skinner argues that theories of the hypothetico-deductive kind have had a detrimental influence on the development of psychology. Instead, he advocates the "experimental analysis of behavior" (Skinner 1966). Research should be guided not by the attempt to test theorems but by the search for orderliness in behavior. This search is based on the scientist's intuition rather than on the strict rules of "scientific method." Experimental research is directed toward controlling the subject matter, thereby discovering lawfulness. This lawfulness is stated in empirical relationships between behavioral and environmental variables and is expressed in concepts capturing the orderliness of the data. Thus, concepts emerge from the experimental program rather than from a priori derivations. An acceptable theory evolves in the attempt to present the collection of empirical fact in a formal and economical way. A formulation using a minimal number of terms to represent a large number of experimental facts is a theory. Theoretical concepts thus merely collate observations and do not refer to nonbehavioral events taking place at some other level of observation and described in nonbehavioral dimensions. [See *BBS* special issue on the work of B. F. Skinner, *BBS* 7(4) 1984.]

A Skinnerian theory is to be evaluated by its effectiveness in enabling scientists to operate successfully on their subject matter. The development of theory is hence closely tied to the psychology of the scientist. For this reason, Skinner emphasizes the importance of a science of science, that is, an empirical study of the behavior of scientists, to develop canons of scientific methodology.

In both the Hullian and Skinnerian approaches, theory development is seen as a continuous linear process. However, two assumptions of this linear model are open to serious question. First, it assumes that observation is independent of theory and can therefore test or broaden a theory. Second, the linear model assumes a clearcut relationship between theory and data in which the latter clearly dictates the former. Contrary to these assumptions, a nonlinear model suggests that the congruence of data and theory is partly a result of a theory's determining what is taken as fact; that theories can always be adjusted to accommodate apparently contradictory evidence; and that scientific progress is often the result of discontinuities, revolutions in which a rival theory wins the loyalties of scientists [p. 90–92].

The thesis that data are "theory laden," even if valid, is not particularly damaging to behaviorist metatheory. This thesis simply asserts that a science of behavior is like any other normal science. Each tests its theories by observation, and each draws a distinction between theory and observation that is relative to a paradigm. Within this paradigm, theory development is reasonably continuous.

At a more fundamental level, the linear and nonlinear models are not incompatible; they are simply on different levels of logical discourse. The nonlinear model conceptualizes scientific progress as a succession of scientific paradigms, each containing its own ideas about observation and theory. It is thus the paradigm that dictates normal science, not the nonlinear model. The behaviorist linear model is at the level of a scientific paradigm legislating how normal science is to be implemented. As such it is not incompatible with the nonlinear model, which is a theory *about paradigms,* and not about how research is to be carried out.

S–R. Behaviorism is closely associated with "S–R psychology," the meaning of which forms the next node. According to one interpretation of "S–R," the S–R reflex thesis, all behavior is reflexive, that is, all behavior consists of responses elicited by stimuli. Typically, the stimulus is conceived as the discrete and relatively brief impinging of energy on a sensory receptor immediately before a response. The response is given a movement-description and is invariant. The elicitation relationship is that a stimulus is a necessary and sufficient condition for a particular response. This simple reflexological model is artificial in that no major behaviorist adopts it. However, its conceptual extensions reveal the assumptions of the S–R reflex thesis [p. 102–8].

The most common extension is to incorporate learning principles by which environmental events that do not elicit a particular movement can acquire this capacity. Thus, a particular stimulus is not a necessary condition for a particular response. A second common extension is to expand the concept of the stimulus to include events occurring inside the organism. A third extension is to incorporate principles of integration and coordination. If observed behavior is the result of the dynamic integration of many reflexes, then the invariability of the simple model must be dropped. An important implication is that observed behavior, being the result of many reflexes, may exhibit properties not possessed by any individual reflex. This integrated behavior, the "specific response," is most usefully described as an act rather than a set of movements. Similarly, the stimulus is best described either functionally or as a distal object.

Mediation extends the simple model further. Mediators are behavioral events, either stimuli or responses in the organism which, once initiated by external stimuli, may continue on their own. One particularly important mediator is the anticipatory goal reaction. This response, and the associated concept of the habit-family hierarchy, are extremely powerful explanatory concepts in accounting for characteristics of behavior not consistent with the simple reflex model. They account for the smoothness, efficiency, plasticity, coherence, and structure of behavior.

These extensions of the reflex model leave very little of the original S–R reflex thesis. All that remains is the assertion that behavior consists of responses (defined functionally or as acts) caused by antecedent events (defined functionally or as objects) acting on the senses (internal or external). Stimuli "elicit" only in the sense that they are causally related to responses. The concept of the operant dispenses with even this last vestige of the S–R reflex formula [p. 106–10]. The operant response is emitted rather than elicited. With the concept of the emitted operant, the S–R reflex thesis is reduced to the assertion that all behavior is a dependent variable functionally related to environmental independent variables.

Even this innocuous assertion is open to the counterclaim that the environment and behavior are causally *inter*dependent, rather than the environment being always the cause and behavior the effect. Indeed, many behaviorist theories recognize this reciprocal determination. Behaviorist concepts of precurrent behavior, intellectual self-management, and countercontrol are instances in which organisms emit behavior that alters the variables controlling their own behavior. Similarly, many theories of reinforcement include dependent environmental variables along with feedback functions expressing these variables as a function of behavior.

The second major interpretation of "S–R," the S–R learning thesis, states that learning consists of the association of stimuli and responses. Critics argue that learning should be described instead as the acquisition of knowledge. A major behaviorist objection to this cognitive approach is that such theories typically fail to specify the relationship between knowledge and behavior. In theories that postulate cognitive maps or observational learning, behavior is derived from the theory by intuition or by assuming that the organism will behave appropriately given its knowledge. Such derivations are unacceptable by behaviorist standards of objectivity. In contrast, the S–R learning thesis provides for rigorous deductions of behavior from theory by including a response-term in the primitive learning operations postulated by the theory. This response-term is then the response appearing in statements about behavior deduced from the theory.

To be sure, this S–R solution is not the only method for deducing statements about behavior from a theory. It is possible to structure a behavioral theory so that the response-term appears somewhere other than in the learning principles, for example, in a performance principle. Because no response-term appears in the learning postulate, the theory may state that learning is a matter of S–S associations. With this maneuver, the difference between S–S and S–R theories appears only in their internal logic. One way to infuse empirical import to the difference is to give them each a physiological interpretation.

A second major empirical difference between S–S and S–R concerns the question whether learning can occur as a result of an event not involving the occurrence of the acquired response. With respect to this question an S–R theory is one which denies learning of this sort is possible. In contrast, S–S theories not having response-terms in a learning postulate can allow for observational learning. A related question is whether observational learning is a primitive principle or derived from principles that do include the occurrence of a response as critical to learning. With respect to this issue an S–R theory is one that claims the latter.

One common criticism of the S–R learning thesis is that it is "narrow" in that it reduces all knowledge to the connection of specific movements to specific stimuli. This objection might be appropriate to the simple reflexological model discussed above but not to actual behaviorist learning theories. First, most S–R theories use response and stimulus classes that are functionally defined. Therefore, stimulus and response can be as general and abstract as the data require. Second, most S–R theories include principles of generalization and induction so that changes due to learning can transfer to a range of stimuli and responses. Third, S–R theories do not assume that all learning occurs through primitive learning operations. Mediation and habit-family hierarchies, as secondary learning principles derived from the primitive postulates, permit learning phenomena of greater complexity.

The organization of behavior. Two nodes on the behaviorist tree diagram represent criticisms of behaviorist views on the organization of behavior. According to one objection, S–R theories are inadequate because they ignore purpose, an essential explanatory concept. Indeed, behaviorists reject teleological explanations, but they recognize that behavior manifests purposive characteristics (e.g., goal-directedness, persistence, flexibility). They accordingly account for these characteristics nonteleologically [p. 120–30].

One behaviorist view of purpose is that it is a state of the organism determining the relationship between stimuli and responses. Purpose can be conceived as either a hypothetical construct referring to neuromuscular states or a purely conceptual intervening variable. In either case, the purposive characteristics of behavior are explained as the result of a state variable which increases the probability of a certain class of responses and is brought about by certain motivational variables.

In another theory, purpose is explained in terms of maintaining stimuli which persist until a goal-response eliminates them. Because there may be a variety of goal-responses, these stimuli may become associated with many responses. Different goal-responses will continue to be emitted until these stimuli disappear, and behavior therefore shows persistence. New goal-responses that succeed in eliminating the stimuli will be acquired, and behavior shows adaptiveness.

The most common behaviorist interpretation of purpose involves reinforcement. In this view, a response occurs not to achieve *future* goals but because it was reinforced in the *past*. To the extent that behavior occurs because of prior reinforcing consequences it will appear to be goal directed. The persistence of behavior can be explained by the strengthening effect of intermittent reinforcement. Because all responses followed by a reinforcer are strengthened, an organism may possess a large repertoire of responses leading to a particular reward, thereby manifesting flexibility.

A second major objection, formulated most vigorously by Chomsky, questions whether behaviorist principles are adequate to account for the organization of verbal behavior, or by extension, for any higher mental process [p. 130–48]. This challenge encompasses at least five issues. In the first, Chomsky argues that speech shows creativity and is therefore independent of previous conditioning and current stimulation. If this attack merely denies that speech consists of a series of reflex movements each elicited by an immediate punctuate sensory stimulation, then the claim is both correct and acceptable to behaviorists, the vast majority of whom reject the reflexological model discussed above. If, instead, the claim is that verbal behavior is not functionally related to behavioral antecedents such as reinforcement, discriminative stimuli, and drive, then the assertion is an interesting speculation, but premature. Little or no evidence currently exists to support the hypothesis. If, on the other hand, Chomsky means to deny that verbal behavior is lawfully related to any antecedent conditions, then his argument challenges not only behaviorism but all scientific approaches that seek causal explanations.

A second issue in Chomsky's challenge is his demonstration that finite state grammars are inadequate to explain the organization of verbal behavior. However, behaviorist psychology is not limited to finite state grammars. Indeed, there are several S–R alternatives to such grammars.

State variables can be used to account for the sequencing of behavior. When a state variable assumes a high value, then a class of responses is simultaneously strengthened, permitting the interaction of responses before their emission. Responses to be overtly emitted late in a sequence can affect responses to be emitted earlier. Similarly, if mediation includes implicit trial-and-error, scanning, and transformations, then the speaker may be hypothesized to scan entire sentences prior to emission and to select the grammatical ones. Or, given that the entire sentence is available to the speaker prior to emission, it can be transformed so that early parts are made grammatically consistent with later ones.

In a functional approach, response classes are commonly defined in achievement terms. The movements by which achievements are effected are not specified and are not necessarily organized by left–right sequencing. Thus, even if functional responses occur in left–right sequences, the internal structure of each response may be determined by other principles. Similarly, in a response hierarchy, each level of description represents a class of events at the next lower level. The order of responses at one level does not determine the order at lower levels. Functional laws involving highly molar functional response classes therefore place few constraints on the order of movements actually observed, which are not necessarily organized by a left–right sequence.

In yet another organizing principle, the open frame, behavior is organized by a schema into which verbal responses are filled (Skinner 1957). This schema can be as abstract as necessary to handle patterns found in verbal behavior. It is not limited by Markov dependencies.

The "autoclitic," a verbal response that is a function of other verbal behavior, is another powerful organizing concept (Skinner 1957). Initial verbal behavior, before the addition of autoclitic responses, is strengthened by environmental variables. It is all simultaneously present as a discriminative stimulus for autoclitic responses. Additional processes of composition and self-editing further affect it prior to overt emission. When verbal behavior is finally emitted, it occurs in an order determined by the properties and relationships among strengthened verbal operants concurrently available before emission, not by left-to-right sequencing.

A third issue dividing Chomsky and behaviorists is Chomsky's notion of competence. A competence model, he asserts, is necessary for a theory of language and exceeds the conceptual limits of behaviorism because it is expressed as a system of rules and refers to the reality underlying behavior rather than the superficial behavior exclusively studied by behaviorists. In response, behaviorists note that a generative grammar, Chomsky's competence model, can be understood as a set of functional descriptions of stimulus and response classes. A set of functional descriptions characterizes a potentially infinite class, can assume the form of rules, and can refer to operations or quantities not directly expressed in observed data. Such functional descriptions refer, however, solely to structure and are not descriptions of causal processes within the organism. It is conceivable that the optimal way to formulate functional descriptions for verbal behavior is a generative grammar. This grammar would then define the dimensions of generalization and induction for verbal responses. The third issue thus reduces to the question of what descriptive apparatus is best for a specification of stimulus and response classes, and as argued above, behaviorists need not be limited to the properties found useful in physics.

A fourth issue in the debate is the relative importance of a structural versus a functional approach to language. Chomsky argues that one must know what is learned before one can seriously study the process of learning. In reply, behaviorists argue that Chomsky's structural principles are derived not from the behavior of everyday speakers but from the behavior of the linguist in constructing sentences and intuitively judging their grammaticality and interrelationships. The competence model is hence tied very loosely to objective observations. Little attention is devoted to the environmental conditions responsible for the creation of the functional class. Nor is the grammar carefully linked to actual verbal behavior. Therefore, the generative grammar is open to all the behaviorist criticisms of theoretical concepts reviewed above.

In behaviorist practice, structural and functional analyses usually proceed concurrently. In a Hullian type of research program, behavior as a function of environmental variables is studied directly. The resulting relationships are then analyzed into various constructs, some of which are related to what is learned (e.g., sHr), or competence, whereas others are performance variables (e.g., D). Likewise, in a Skinnerian research program experimental variables are manipulated until "smooth curves" result. These optimal functional relationships determine structure in that they specify functional units for the operant response, the discriminative stimulus, and the reinforcement. Thus, Chomsky's structural approach is not the only sensible way to begin the study of behavior.

The fifth issue between Chomsky and behaviorists is an unresolved empirical question about the degree to which knowledge of language is innate. Behaviorists tend to assume that primitive functional descriptions are simple in form. When a functional description proves to be extremely complex, behaviorists tend to assume that it is derived from a secondary learning operation and is therefore learned. Nativists, in contrast, readily assume that a complex functional description is primitive and therefore innate. Because behaviorists assume only simple primitive functional descriptions, they must assume a complex secondary learning operation to explain a complex functional description. In contrast, nativists can hypothesize that a complex functional class can be formed by a simple learning operation, the mere exposure to a small number of spoken sentences, for example.

Because behaviorists see complex structural descriptions as the result of complex learning operations, they tend to assume that there is a great deal of flexibility in the form of functional descriptions depending on the organism's particular conditioning history. Uniformity of functional classes across individuals and behaviors must be the result of uniformities in conditioning histories. For the nativist, however, there is little flexibility in the form of a functional description. Complex descriptions are primitive and therefore innately given. Uniformities across individuals are the result of the innate constraints on possible functional classes.

Complex processes. Behaviorists propose a variety of theories to extend the S–R framework to the complexities of human behavior, including cognitive activity. Under one interpretation, a thought is an intervening variable, not an episode. According to this view, there is no empirical sense to the debate about whether therapeutic techniques work directly on behavior or whether they affect behavior only via cognitive processes. Behavior is also said to be thoughtful, in a dispositional sense, when it is organized by functional response and stimulus classes describable in very abstract and complex terms.

Because virtually all changes in behavior involve classes of behavior and classes of stimuli, it is nearly always possible to find systematic relationships among these classes and to construct a cognitive intervening variable. Still, many behaviorists object to cognitive concepts because of the general behaviorist opposition to theoretical concepts reviewed above. Objections to cognitive constructs, in particular, center on the claim that these are not properly linked to behavior or to the environment.

Under a second interpretation, thought is a hypothetical construct postulated to consist of covert events occurring within the body. These events include covert mediators, covert trial and error, and covert verbal behavior. To the extent that these constructs violate the behaviorist conditions for hypothetical constructs discussed above, they acquire functional autonomy and merge with the constructs of neomentalism.

In yet another approach, thinking is identified with any behavior, covert or overt, which plays a role in solving a problem. This precurrent behavior modifies the environmental variables of which the actor's own behavior is a function so that a solution response emerges. The precurrent behavior is identified as thinking only because of the role it plays within a certain behavioral context. Therefore, a behavior cannot be identified as thinking on the basis of its form alone, and thinking is not limited to any particular type of response.

Contrary to these behaviorist approaches, according to the information-processing paradigm of neomentalism, it is necessary to postulate internal activities of a nonbehavioral sort. These activities include internal representations and internal operations to process information [p. 160–71]. The argument in favor of these cognitive constructs is based on situations in which behavior is not in simple correspondence with the environment, and internal processing is at work. As was argued in the discussion of molar behaviorism, however, the mere existence of a process does not entail that it must be included in a particular scientific theory. In general, the experimental data from which cognitivists infer constructs permit behaviorist alternatives without the cognitivist processes.

Consider psychophysics, in which it is commonly found that the subject's judgments correspond not to the stimulus energy but to some function, F, of the energy. The cognitivist claim is that there must be an internal representation, R, directly judged by the subject, and F must be the transformation applied to the stimulus input. First, note that F, the operation which "must" be postulated, is relative to the physical scale used. A different scale would "require" a different operation. Second, even after F transforms the energy into R, it is still necessary to get from R to the response. All that is accomplished by the internal processing is that behavior is now related to R by an identity function rather than to the stimulus by the function F. Why should one function be preferred over the other? Both are lawful and equally useful.

Consider a second example. A hydrophobic patient finds water threatening, and the fear is apparently related to the patient's beliefs rather than properties of the water. Accordingly, the cognitivist argues that the patient is reacting to an internal representation of water rather than to the water itself. However, this argument is based on a rather narrow construal of environmental properties. It ignores properties of the environment based on past interactions with people. For the behaviorist, the quality of being threatening to a person is a current dispositional property of the stimulus acquired through its membership in a stimulus class which previously interacted with that person.

As these examples show, the differences between cognitivism and behaviorism are not resolvable on the basis of empirical findings alone. Instead, underlying the differences is a conceptual disagreement concerning explanation. When a behavioral change occurs, at least three changes may be said to have occurred. First, there is a change in the organism. Second, there is a change in the relationship between behavior and the environment. Third, there is a change in the environment in that it now has different effects on behavior. Cognitivists prefer to view behavioral changes in terms of changes in the organism, such as a change in internal representation. The other two changes are explained in terms of changes in the organism. Jones avoids water because of changes in his representation of water. In contrast, behaviorists tend to emphasize changes in the environment and to explain the other changes in terms of the former. Jones avoids water because it has become a conditioned aversive stimulus.

Thus, cognitivists insist that an adequate explanation must refer to internal features of the organism, features that are contemporaneous and in correspondence with behavior. In contrast, behaviorists prefer to explain behavior in terms of the environment, including dispositional properties the environment possesses only by virtue of its previous interactions with the organism.

Exorcising the agent. A central behaviorist family trait is the rejection of the concept of agency. Behaviorists object to this concept because it implies free will, refers to an unobservable cause qualitatively different from those investigated by the other natural sciences, and is often used in spurious explanations which assign to the agent whatever properties are needed to account for an otherwise inexplicable action. Thus, the concept is incompatible with behaviorist requirements for objectivity, observability, lawfulness, prediction, and control.

Another possibility is that behavior is caused by internal causes which are deterministic and physical and not, therefore, subject to the above objections. Nevertheless, behaviorists generally reject this possibility and subscribe instead to the theses of externalism: (1) Virtually no behavior is brought about by internal events not causally related to the current or previous environment; (2) although the relationship between behavior and its inner causes is lawful, an epistemically satisfactory explanation of behavior must not restrict itself to these relationships but must relate behavior to the external environment and represent internal causes as either parameters or inferred theoretical terms. Externalism is a working assumption believed to further behaviorist goals of prediction and control. Explanations that stop at internal causes are not useful because observation and manipulation of these inner causes are severely limited. There is, furthermore, a strong temptation to speculate freely about internal causes or to derive them from introspection.

One consequence of this externalism is the behaviorist mistrust of psychological explanations which account for one bit of behavior by relating it to other behavior rather than to the environment. Therefore, several concepts of R–R psychology, including traits, fields, and instincts, are rejected by many behaviorists [p. 180–86]. A second implication is that most behaviorists, although recognizing the reciprocal causal dependence of behavior and environment, prefer explanations that interrupt this reciprocal system in such a way as to produce laws portraying behavior as a function of the environment. Features of externalism are also to be found in the reflexological model with its de-emphasis on the central nervous system and its adoption of complete arcs and peripheralism.

Because of this exorcism of agency, behaviorists are often accused of harboring a "mechanistic" view. In one sense of "mechanistic," the charge is just: Behaviorism assumes that behavior is an invariable, lawful, and therefore "automatic" consequence of antecedent environmental causes. However, the accusation is unfounded in the sense of "mechanistic" in which behavior is thought to be reflexive, unintelligent, and stereotyped. Nor are S–R explanations "mechanistic" in the sense of dealing only with the material parts of the body, treating behavior as the movement of bodies in space, or ignoring the dynamic interaction of behavioral causes.

Another consequence of the exorcism of agency is the debate between cognitivists and behaviorists over whether the effects of reward are automatic, or whether they are mediated by selective attention and cognition. In a variety of S–R theories, behaviorists interpret instances in which the effects of reward appear to depend on the subject's attentional or cognitive state either in terms of mediational behavior or the control of behavior by abstract properties of the environment. Thus, given these theories, both behaviorists and cognitivists agree that the effects of a learning operation depend on other accompanying conditions, termed "attention and cognition" by cognitivists and "orienting and mediating responses" by behaviorists. One major difference between them concerns whether these accompanying conditions are under the control of the external environment as behaviorists contend, or are relatively autonomous, as some cognitivists imply. A second difference is whether the mediating behavior found in the adult human must itself be acquired by learning operations which are not mediated, as the behaviorist contends, or whether *all* reward is cognitively mediated as some cognitivists imply [p. 188–92].

Although behaviorists reject agency, many of them attempt to explain why we have a concept of agency and how we distinguish "voluntary" from "involuntary" acts. According to one view, the point at which behavioral variables converge to determine behavior is a locus that can be identified with the self. In another approach, voluntary behavior is identified as that behavior caused in part by chains of mediating responses because this behavior is the result of thought. Another possibility is that the voluntary–involuntary distinction corresponds roughly to the operant–respondent distinction. Yet other behaviorists note the importance of learning to treat oneself as an other in the development of the self-concept [p. 192–94].

In a contextual theory of agency, an "automaticism" is defined as a response that follows from an event without any intervening action on the part of the person. The contextual theory maintains that an automaticism can be the act of an agent, depending on the behavioral context. For example, when Smith merely looks at his map and then "automatically" proceeds to turn right without any thoughts popping into his mind, that turn may still be the intelligent act of an agent if it is the result of a long history of learning to read maps, if he has a history of reinforcement for using maps, and if he has a disposition to explain his use and to correct mistakes. To say that a response is an intelligent act of an agent is not to say something about the mental events that precede it but rather to say something about the broad context of that response. In contrast, to say that a *response* has occurred is to say little about the context. Thus, action language is of a higher order than response language in that the former implicitly refers to relationships between behavior and environment that are of greater complexity. Mixing these two levels of discourse leads to conceptual confusions, including the misguided charge that behaviorists regard organisms as "passive."

Behavioral interpretation. The mentalistic language of everyday speech is incompatible with behaviorism. Behaviorists treat this language in a variety of ways that comprise the next node. One approach is simply to deny the legitimacy of mentalist language. Mental concepts, it is argued, are merely the residue of prescientific animistic notions which will be replaced by scientific ones. In a second approach, methodological behaviorism claims that psychology must limit itself to the observable facts of behavior, whereas mental language finds application in the private phenomenal world beyond the scope of science. Logical behaviorism suggests that the meaning of mental concepts is given by other concepts referring to observables which are the conditions of application and verification. Similarly, operationism proposes that the meaning of a mental concept is to be found in the operations used to measure it.

On another account, mental concepts are identified with intervening variables associated with the everyday concept. One criticism of this approach is that someone may be said to be in a certain mental state, for example, anger, without ever acting angry. In reply it should be noted that in order for an intervening variable to be reflected directly in behavior, test conditions must obtain and many other variables must assume appropriate values. In the absence of any anger-behavior, the value of the anger-intervening-variable may be derived from the values of the independent variables of which the intervening variable is a function. Closely related to the intervening-variable account is analytic behaviorism, according to which statements about mental events are statements about behavior and dispositions to behave, not about private consciousness [p. 202–9].

A major challenge to these interpretations arises from neomentalism, which rejects the thesis that the mental can be identified with behavior. Instead, it claims, behavior is merely an external symptom caused by mental events in the form of functional states which are the referents of mental terms. In reply to neomentalism, it can be said that the identification of the mental with functional states is an empirical hypothesis, whereas analytic behaviorism is concerned with giving the current everyday meaning of mental concepts. Although neomentalists argue that their proposed identification would not constitute a change in the meaning of mental concepts, our concept of meaning is not precise enough to permit a definitive judgment of their claim.

Furthermore, the neomentalist's functional states are not the "mental" events analytic behaviorism seeks to exorcise. For neomentalism, functional states are neither actions of the person nor conscious processes. Analytic behaviorism certainly does not deny either that (1) behavior has a neurophysiological cause or that (2) events which are neither conscious nor acts occur within the person's body. The tasks of analytic behaviorism and neomentalism are complementary rather than contradictory. The former provides the behavior that is the meaning or criterion of application for mental terms, and the latter supplies a causal explanation of behavior.

In contrast to the conceptual treatments reviewed above, behaviorists also offer empirical treatments of mental concepts which make use of empirical discoveries. In an empirical reduction, the interpretation of a mental concept consists of two stages. In the first, the features of behavior that correspond to the mental concept are determined. Then these features are explained with an empirical behavioral theory that incorporates no mental concepts.

Another empirical treatment, that of empirical translation, is based on a behavioral theory of meaning. According to this theory, the meaning of a mental term is to be found in the variables controlling its emission. These independent variables include, of course, the features of behavior that function as discriminative stimuli for the verbal response. In addition, they include the current contingencies of reinforcement for the speaker.

Both empirical reductions and empirical translations are criticized for extrapolating behavioral findings from the laboratory to complex everyday human action where the findings do not clearly apply. For example, the application of terms such as "stimulus" and "conditioning" is attacked as "metaphorical." This criticism ignores the fact that the extension of a theory to a new domain is important in scientific progress. Theoretical terms are assigned their initial applications through demonstrations in paradigmatic situations. Extension of theoretical terms to new situations requires the ability to see the novel circumstances as instantiating the theoretical concept. Far from being pejorative, the adjectives "metaphorical" and "analogical" are at the very core of the best scientific thought. A heuristically fertile theoretical concept is open in the sense that its meaning develops as it is applied to new domains. Thus the extrapolations involved in behaviorist empirical interpretations should not be rejected on a priori grounds but should be empirically evaluated as scientific hypotheses. If the extension is confirmed, if it brings an increased degree of prediction and control to the new domain, and if the concepts can be applied to the new domain with intersubjective agreement, then the extension is justified.

Another behaviorist approach to mental language is the view that mental concepts are theoretically reducible to covert events which are hypothetical constructs in a behavioral theory. Because hypothetical constructs are conceived as physical events, the dualist connotations of mental language are eliminated. This approach differs from neomentalism in the ways that behaviorist constructs differ from cognitivist constructs as discussed above.

First-person report. In first-person reports, speakers apply mental concepts to themselves. Behaviorists must explain not only the meaning of these reports but also the verbal behavior constituting them. For logical behaviorism, the meaning of such reports involves only the publicly observable conditions constituting the verification of the statements, including observable behavior and physiological states. On an operationist interpretation, "sensations" are not private events reported by the subject but rather concepts operationally defined as that which is measured experimentally by means of the observable operations of instructions, stimulus presentation, and the subject's verbal discrimination behavior.

Many behaviorists interpret the relationship between first-person report and the object of the report as the relationship between a verbal discriminative response and the discriminative stimulus for that response. For some first-person reports, the discriminative stimuli may be the reporter's own behavior or the environmental independent variables controlling that behavior. For others, the discriminative stimuli may be covert stimuli within the speaker.

Although the first-person report may convey information about its discriminative stimulus, most behaviorists treat a first-person report as a datum rather than as a report of a datum. The subject's first-person report, R_1, for example, "I am in pain," is of a lower logical order than the experimenter's third-person report, R_2, "Smith said, 'I am in pain.'" A description of the object of R_2 (i.e., Smith's behavior) enters the behavioral data language, whereas the putative object of R_1 (i.e., a covert stimulus inside Smith) does not. Talk of covert stimuli can enter scientific discourse only via the theoretical language if the experimenter uses R_2 to infer a hypothetical construct. Thus, first-person reports function as data from which covert events are inferred rather than observational reports of those events. Hence, scientific descriptions of the covert events do not have to conform to the subject's first-person descriptions.

A common objection to this discrimination interpretation of first-person reports is that it seems to base first-person reports on observation. In reply it should be noted that not every verbal response to a stimulus is an observation or a report of that stimulus. For example, although our ability to localize a sound depends on our discriminating differences in sound waves between the two ears in arrival time and intensity, we cannot be said to "observe" those differences or to be reporting them when we say where a sound is.

Another interpretation of first-person reports is that they refer to behavioral dispositions or intervening variables. Possibly the discriminative stimulus for the report is either the set of independent variables or the set of dependent variables which comprise the intervening variable. Another possibility is that the report is not a discrimination. Just as it is not necessary for a rat to discriminate hours of food deprivation or the intervening variable of hunger in order to determine its rate of lever pressing, so a speaker does not have to discriminate the intervening variable of anger (or the independent variables of which it is a function) in order to emit the verbal response "I am angry." That verbal response is simply one of the behaviors that covaries with the anger-intervening-variable in the same way that screaming and kicking do.

One particularly important kind of first-person report is the one that occurs in experiments designed to measure psychological magnitudes and to construct psychological scales. According to the intervening-variable interpretation, the measurement of sensory magnitude and the construction of a scale are performed by the experimenter in the pragmatic metalanguage, not by the subject. The correlation between the subject's report and the scale is not an empirical finding. It is rather a stipulation of the measurement procedure, which is a kind of defining experiment for the intervening variable. The subject, on the other hand, is engaged in a transfer-of-training task. Having learned the verbal responses in a context in which precise rules of use are enforced, the subject is transferring this training to another public context, one in which no rules of use have been defined.

This analysis of scaling can be generalized to a large class of reports traditionally conceived as reports of "subjective experience." What distinguishes subjective from objective reports is that the former consist of the transfer of verbal responses from contexts in which there are public rules of use to contexts in which there are no such rules. The speaker generalizes from the situations in which the terms were learned with public discriminative stimuli and correction to situations in which correct use has not been defined. Reporters respond in the way that they deem appropriate on the basis of their previous experience, and in this sense they may be said to be following "inclinations" rather than rules. Under the appropriate conditions, the report is taken as the result of the transfer, and there is no question of the report being "correct," just as there is none when a pigeon's generalization gradient is tested along a wavelength continuum following training on specific wavelengths. This inclination, or disposition, to transfer behavior from one stimulus to another may constitute the subjective quale of stimulation.

Behavioral epistemology. An analysis of behaviorist theories of knowledge provides a coherent pattern to characterize the behaviorist conceptual framework reconstructed in preceding sections. In behaviorist epistemology, knowing is intimately linked with behaving. For some behaviorists, knowing is a kind of behavior. For others, knowledge is represented by cognitive intervening variables defined solely in terms of the relationship of behavior to the environment. Science in particular is viewed as a kind of knowing acquired and maintained in much the same way as everyday knowing [p. 251–55].

Although behaviorists differ in their theories of behavior and hence in their explanations of scientific research, theorizing, and theory confirmation, they all view science in terms of the behavior of the scientist. Therefore, knowledge can be analyzed and explained by theories developed by a science of behavior, and epistemology becomes the psychology of knowing. In contrast to this psychologistic approach, in a purely formal epistemology, the structure and contents of knowledge are considered independent of the behavior that generated them. For example, in a formal analysis, logic is conceptualized as independent of human activity. In contrast, for a behavioral epistemology, logic is a property of verbal behavior, a set of rules describing certain relationships extracted from speech. Just like other aspects of behavior, these special relationships appear because of certain laws of behavior, and several behaviorists attempt to account for logic with a behavior theory.

This association between knowledge and behavior means that knowing, like behaving, must be understood in relation to its function. For behaviorism, the function of behavior is the adaptation of the organism to its environment. Epistemic behavior can accordingly be seen as part of the overall adaptation of the organism and can be evaluated with regard to how well it executes this function. This link between knowledge and adaptation is closely related to American pragmatism, which regards knowledge and belief as instruments to satisfy human needs.

Just as the "validity" of an instrument depends on its effectiveness, so for behaviorist pragmatism the truth of a statement and the meaning of a concept are matters of their usefulness rather than transcendent properties of words. Similarly, just as an instrument is useful at one time but not another, for one purpose but not another, so human knowledge is always provisional and relative. Just as instruments are "known" by using them, so for behaviorist pragmatism, the world is known not by passive sensing but rather through the consequences of our interaction with the environment.

Behaviorist pragmatism is woven into the more comprehensive intellectual tradition of positivism, especially nineteenth-century "empiriocriticism." In many ways, behaviorism can usefully be understood as a psychological version of positivism.

Positivism is characterized by its repeated attempts to demarcate "positive knowledge" by describing what sorts of beliefs deserve the designation "knowledge" and which should be discarded as "metaphysical" because they generate endless disputes. For behaviorist positivism, "positive" is equivalent to "scientific," and behaviorism can be characterized as a positivism devoted to the demarcation of scientifically acceptable methods, questions, and concepts. Based on its pragmatism, behaviorism suggests that the criteria delineating positive knowledge, or science, are those which psychology determines best enhance the knower's ability to predict, control, and therefore adapt to the environment.

The positivist preference for knowledge based on direct experience expresses itself in behaviorism as the emphasis on experimentation, direct observation, and scientific empiricism. This emphasis underlies the assumptions of molar behaviorism, peripheralism, and externalism. All three are adopted to ensure that the objects of behaviorist investigation and theory will be useful for prediction and control and that scientists will not be tempted to speculate about the "metaphysics" of unobserved events inside the organism. This attitude also underlies the behaviorist insistence that theoretical terms be securely linked to observables.

Behaviorism and positivism also share a commitment to the development of "positive methods," that is, objective methods that ensure positive knowledge. Hull's hypothetico-deductive method, as well as Skinner's experimental methods, conforms to positivist tradition. Both base their recommendations on a psychological analysis of science as the behavior of scientists. Both aim at a positive methodology producing indisputable knowledge either because it is derived by logic and observation (Hull) or because of the conspicuousness of behavioral control (Skinner).

Positivist recognition of the psychological origins of knowledge draws in its wake an emphasis on the social character of science and the importance of clear communication. Precision and social consistency in verbal behavior are hence critical features for the objectivity of positive methods. These assumptions underlie the significance attached to intersubjective agreement throughout behaviorist methodology, including behavioral data language, theory construction, and the rejection of introspection.

To assign an epistemic function to abstract terms without forfeiting the primacy of experience, behaviorists and positivists assume a nominalist stance with regard to concepts. For both, hypostatization is a cardinal error. This opposition to reification underlies behaviorist objections to mentalism, cognitivism, and R–R psychology. Instead, behaviorists often interpret abstractions in terms of relationships among observables.

Behaviorists share the antimetaphysicalism of positivism. To be sure, behaviorists generally maintain a materialist monism; however, in their case against consciousness, they set forth methodological arguments rather than ontological ones. Their position is more pragmatic and positivist than metaphysical.

Both positivism and behaviorism avoid the issue of whether there is a transcendent reality beyond scientific investigation because the question is "metaphysical." For behaviorist positivism, the distinction between appearance and reality must be replaced by distinctions within positive knowledge. For the appearance–reality distinction, the distinction between effective and ineffective behavior can be substituted. In a similar fashion, some behaviorists conclude that scientific laws are not descriptions of how the universe fundamentally operates but instead are rules for effective human behavior with respect to nature.

A common criticism of behaviorism is that in its attempt to regulate scientific activity, it may become overly restrictive, thereby curbing the creativity of the scientist. One reason why behaviorist positivism need not fear this possibility is that behavioral epistemology implies that scientific activity is underdetermined by rules of scientific methodology. Because in behavioral epistemology science is the behavior of scientists, the underdetermination of science by rules follows from the general underdetermination of behavior by explicit rules. Similarly, according to behaviorist pragmatism, science is also underdetermined by the world it studies. Science is always relative to human needs and goals.

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There's reconstruction, and there's behavior control

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Confronted by an extraordinarily competent conceptual reconstruction of behaviorism, what's a behaviorist to do? We can argue with any or all of its details; and we can ask what its function will be for our future behavior – if any. I choose the latter, partly because it seems more interesting, and partly because there is very little in Zuriff's reconstruction with which I can disagree.

The conceptual reconstruction of behaviorism offered by Zuriff is at least an occasion for alternative conceptual reconstructions, especially those that will include Zuriff's reconstruction as a part of what is being reconstructed. I offer the hint of one here; very likely, other commentaries will do similarly. Then each that does will constitute an occasion for yet another reconstruction of behaviorism, one that includes it as well. The first question is whether that becomes an endless reiterative process, or whether it turns on itself such that the newer reconstructions, now inclusive of all prior reconstructions, begin to repeat the older ones identically rather than to extend them. Does reiterative reconstruction reveal *one* behaviorism, or a handful of behaviorisms, or an infinitude of them – or at least one for every reconstructer?

If there is one behaviorism that will emerge from conceptual reconstructions of everything that Zuriff has included, plus all of the reconstructions that result from including his and theirs, then that is an optimistic outcome for the questions to follow. If many conceptual reconstructions are possible, that seems to me less optimistic for those questions. It also calls for considering criteria for choosing among those multiple reconstructions, if a situation should ever present itself that called for choosing among them. (We could, after all, simply enjoy them all in whatever numbers they might require, much as we enjoy a zoo. But note that we do not fully use zoos that are too large.) Choosing will no doubt depend on the purpose of that choice, that is, on the existence of some consequence to be gained or avoided by that choosing. That consequence could be the purpose of conceptual reconstructions of behaviorism.

Because Zuriff's initial pages suggest that at least one of his purposes is to achieve something that philosophers call "soundness," a second question parallels the first: Is there one such soundness, or many? Do philosophers achieve consensus on such questions? Or do they, like the psychologists and behaviorists Zuriff so accurately describes, go round and round on these issues without apparent progress, in a process that a student of family interactions might well label "fussing"?

Either answer to either question suggests an "and/or" pair of questions about other purposes of conceptual reconstructions: Is a conceptual reconstruction of behaviorism meant to reveal the logics (the patterns of discriminative stimuli) that control the distinctive behaviors of behaviorists? And/or is a conceptual reconstruction meant to be a better logic (a somewhat different pattern of stimulus controls) for the future distinctive behaviors of those people who may then properly call themselves behaviorists?

If the answer to either of those questions is yes, then a fifth question arises: Is the actual practice of behaviorism – its statements of theory, its research, and its applications – different, depending on which conceptual reconstruction – on which set of stimulus controls – is operative? Especially, are the practices of a behaviorism described by a reconstruction that Zuriff would consider sound distinguishable from the practices of a behaviorism described by a reconstruction that Zuriff would consider unsound?

Asking the fifth question may serve to conceal a sixth, more fundamental question, at least for tactics: Does the practice of behaviorism by behaviorists – its statements of theory, research, and application – *actually* follow the putative functions inductively revealed by its conceptual reconstructions? That is, are behaviorists' practices controlled by their behaviorism's conceptual reconstructions, or are its conceptual reconstructions controlled by their practices, or both interactively? *Ipso facto*, Zuriff's reconstructing behavior is controlled by the practices of the behaviorists he has studied. But are *their* practices controlled by (in contrast to describable by) the reconstruction that he can discern in their practices? Much of their practice belongs at least topographically to the family explained by Zuriff's conceptual reconstruction of the putative practice-controlling functions. But it need not be true that all of those practices are under the same functional control, or under it to the same extent, or without competition from other, quite different, functions: Topography almost never reveals function, and circumstance can allow identical topographies to result from quite different functions, just as it can allow quite different topographies to serve the same function.

The fifth and sixth questions automatically generate a seventh question for most behavior analysts: Are the fifth and sixth questions empirical ones, or are they to be fussed over by philosophical argument independent of data, as if conceptual reconstruction were functionally similar to psychohistory? This question arises not as a quibble, but because it seems to me that much of the practice that Zuriff considers behavioristic is not much under the control of the functions that he induces from that practice. Some of it is, surely, but perhaps not much. If most of it is, then Zuriff's conceptual reconstruction, partly because it is so competent, is an event of major importance to behaviorism, as any such reconstruction would be (unless there are too many of them, as queried above). If little of behaviorists' practice is actually under the control of the functions Zuriff has induced from that practice to be its conceptual reconstruction, then, to that extent, any conceptual reconstructions of it, when equally competent, can have only an admirable heuristic value as intellectual exercises at the intriguing level of explaining the behavior of explaining behavior.

Perhaps such explanations need not be comprehensively true to be heuristic, or to be good exercise. Aristotle argued that history need not be true to teach us about the future; instead, it needs to show us what should have happened (considering how the world works, which he knew), which may not always be what did happen (mainly because conceptually irrelevant circumstances occasionally derail the fundamental processes at work). The same may be so for conceptual reconstructions. (Perhaps conceptual reconstruction is what Aristotle meant history to be.) Indeed, a slightly true conceptual reconstruction, if as competent as this one and widely studied, could by that process become increasingly true for the future practice of the behaviorists: a self-fulfilling induction.

Meanwhile, what other functions control the practices of behaviorists? Even as one of them, I can only guess, of course – we never analyze our own behavior experimentally, and so I have no facts. In both our basic and our applied research, and in our nonresearch applications, it seems to me that we continue to work more on the same problems we did yesterday, until they are either clear or at a dead end. Call that a "Mount Everest function": climbing a problem simply because it is there to be climbed. Sometimes we ask whether the reason a problem is at a dead end is that there is a better way to state it, perhaps one that would make it a special case of a larger problem that, in the diversity usually correlated with largeness, would show us a more profitable tactic. Sometimes we ask ourselves what we can do that would be new and distinctive – to reveal an Everest that no one knew existed before. We ask those questions of course with the special insightfulness and under the special constraints that our personal (and typically implicit) conceptual constructions of behaviorism impose upon us.

But sometimes we ask ourselves whether we can use our behaviorism to solve some local but important real-world problem in a way that will establish the solutions of all real-world problems of the same class – a deliverable applied behavior analysis. So far, that does not exist. But sometimes it seems that it almost does, and in its tantalizing apparent nearness, it offers one more function that may explain the practices of some behaviorists, and may establish for them the conceptual reconstruction they can use self-consciously to try to control their future practice, and that of their students: The correct conceptual reconstruction is one that explains why their problem-solutions work when they do, and how new ones could be achieved systematically rather than by trial and error, and how a society could be convinced to use those solutions when they work (as society manifestly is now convinced to use many pseudosolutions that do not work).

In behavioristic application, perhaps the key conceptualizations are that (1) any problem can be solved by reducing it to the right set of behavior changes in the people involved, (2) there is a beginning technology for accomplishing those behavior changes, and this technology desperately needs and is ready for immediate *experimental* expansion, (3) the results of changing those putative problem behaviors are always to be measured accurately and in convincing experimental designs, (4) if people do not like a solution and its results, it is not yet a solution no matter what other criteria it satisfies, and (5) the criterion of a good technology is that it works consistently in the real world, whether or not it achieves something called soundness.

Competent conceptual reconstructions of behaviorism may be multiple; it all depends on how many ways there are to induce explanatory functions in and for the distinctive practices of the behaviorists. We can wait to see whether more Zuriffs appear. But if there are multiple conceptual reconstructions of applied behaviorism, their application requires that they all include demonstrable and attributable success in a much larger world than the Skinner box and its users. My guess is that in submitting to that criterion as well, multiple reconstructions will be sharply reduced in their number, but sharply increased in their visibility. Put another way: If behaviorism ultimately works to solve important problems for large numbers of people – for society – in ways that people like or can be brought to like, then that is the behaviorism that will survive outside of the universities. Its conceptual reconstruction may not be identical to Zuriff's conceptual reconstruction of behaviorism in general, because it seems to me that behaviorism in general operates and competes for survival in a less naturally selective domain than does applied behaviorism (not a less natural domain, and not an unselective domain, but only a less naturally selective one). We shall see. But perhaps the best characterization of Zuriff's reconstruction, for now, is that I would not be surprised if it were the one that survived.

Why behaviorism won't die: The cognitivist's "musts" are only "may be's"

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Despite the existence of reportedly devastating logical critiques of behaviorism (e.g., Bever, Fodor & Garrett 1968; Chomsky 1959), there still exists a very large and, in fact, growing number of behaviorists. As evidence, the Association for Behavior Analysis now claims approximately 2,000 members, and more than 30 behavioristically oriented journals are currently being published. For those who wonder why behaviorism should continue to be a view held by a significant number of presumably intelligent people, Zuriff's book should be required reading.

Attackers of behavioristic views have been prone to use the words "must" and "necessary" when referring to the role(s) presumably played by "representations" or other alleged mediators between environment and behavior, and it is my supposition that those words and close relatives will find their way into some of the other commentaries that accompany this one. What Zuriff has done, and has done convincingly in my view, is to show, through a remarkably scholarly treatise, that behavioristic views of the sort actually held by people who call themselves behaviorists are not *logically* unsound. He points out that the major critiques of behaviorism have focused on a type of behaviorism, which he refers to as simple reflexology, that has essentially no proponents. Modern, more complete behavioristic views are immune to the criticisms raised. Having dispensed with logical arguments against behaviorism, Zuriff carefully and thoroughly presents behavioristic arguments for avoiding mentalistic or cognitive explanations and comes to an obvious conclusion: Whether one opts to be a behaviorist or a cognitivist is a *strategic* choice, not a logical one. As he notes, "Lawfulness between behavior and the environment either exists or it does not, and lawfulness in the intermediate steps does not change that fact" (p. 32). Consequently, whether to focus on lawful behavior–environment relationships or on hypothetical functional intermediaries boils down to a choice based on pragmatic criteria regarding which approach holds more promise.

To his credit, Zuriff enters the "debate" between cognitivists and behaviorists without engaging in polemics, although he does not mince words either. Instead, he illustrates the basic differences between the two approaches regarding strategic reasons for embracing or eschewing hypothetical intermediaries. Cognitivists adhere to the "bead theory" of causation (causes must be spatially and temporally contiguous with effects) whereas behaviorists are tolerant of temporal gaps (the contents of which, presumably, are to be filled in by physiologists eventually) and point to the dangers inherent in postulating entities that are not closely tied to observation.

In the final analysis, the strongest arguments for adopting a cognitivist position are those that are based on the heuristic value of such approaches. This conclusion leads me directly to the first of six questions that I hope Zuriff can take the time to address in his Response to the commentaries.

1. Cognitivists make the legitimate claim that their theories work heuristically to guide research and to organize data. Thus their theories help decide which experiments are important to do. How do behaviorists decide which are the important experiments?
2. "Representations" are certainly a challengeable concept (see Malcolm 1977), but they obviously have considerable appeal. Among the most frequently cited findings illustrating the interpretative importance of "representations" are results demonstrating what is called "iconic memory" or "sensory memory" (e.g., Sperling 1960). [See also Haber: "On the Impending Demise of the Icon" *BBS* 6(1) 1983.] How does a behaviorist speak about experiments that are easily interpreted as indicating that a sensory copy (usually visual) of events persists for a short time after stimuli are presented?
3. A major behavioristic view of how we learn to talk about private events (e.g., pain, hunger pangs, etc.) is that the verbal community makes use of public accompaniments when training its members to talk about such things. [See also special issue on the work of B. F. Skinner, *BBS* 7(4) 1984.] Might there not be empirical evidence that makes this view less reasonable? Specifically, I refer to the phenomenon of overshadowing during the development of discriminative stimulus control. Stimuli that are more precisely "aligned" with the contingencies of reinforcement "overshadow" those which are less well correlated; the development of stimulus control by less well correlated stimuli is blocked when more regular predictors of the contingencies are present (see Mackintosh, 1974, for an overview). If in training our children to speak about private events we rely on publicly available stimuli, are these public stimuli not those most regularly related to the contingencies? If they are, why is control by the presumably less well correlated private stimuli not overshadowed?
4. Some cognitive approaches to behavior can be characterized as attempts to use current behavioral repertoires as evidence of particular histories (thus avoiding the temporal gap), and then these current repertoires (actually, often internal surrogates for the remote and unexaminable history) are used to predict or explain current behavior. Given the unavailability of the relevant history, why is this apparently pragmatic view not widely adopted by behaviorists?
5. As an exercise in behavioristic interpretation, why do we find that behaviorists refer to as "fictional" explanations of behavior so appealing? Our everyday speech is filled with such accounts, and they frequently seem rather satisfying.
6. My last question has two parts. First, Zuriff's reconstructed behaviorism seems to have very much in common with Skinner's recent descriptions of his views (Skinner 1974). [See also *BBS* 7(4) 1984 and B. F. Skinner's commentary, this issue.] Is that a fair assessment? If it is, then given that the behavioristic position as it is reconstructed in this book is a logically sound view with several reasonable arguments suggesting it as a good choice strategically, why has such a view always been a minority one within psychology? Even in the so-called heyday of behaviorism, Skinner's variety held only a minority of adherents.

I would like to close my review by stating again that I view this book as a monumental piece of scholarship that more clearly than anything else I have read illustrates the differences between cognitive and behavioristic approaches. Kudos to Zuriff.

Viewing behaviorism selectively

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I am sure I will be coming back to this book. I found much of interest in it, and it struck me as the sort of work in which I would be likely to notice new things when I returned to it. If that had been my only reaction, however, it would have been best to save my commentary until some future date, when I could have given its depth and scholarship adequate due. But something seemed missing, and for that reason it seems inappropriate to wait.

A book such as this is bound to be selective, and I will not take issue with the particular topics Zuriff has chosen to address. Yet selection itself was to me conspicuous by its near absence in the book. Darwin does not appear in the index of references, and Skinner is mentioned as a selectionist only in passing.

The omission is especially curious because a selectionist view seems implicit in so much of Zuriff's account. In the index, "evolution" refers the reader to "Contingency, of survival" and to "natural selection," and together these two entries refer the reader to seven places in the book (each has five; they share three of them).

The three shared references are to the possibility that "behaviorists can study the *environmental* contingencies of survival ultimately responsible for the organism's genetic endowment through the process of natural selection" (p. 178, author's italics), to the argument by Skinner that "reinforcement . . . has evolved by natural selection through phylogenic contingencies of survival" (p. 260), and to the claim that the "principles that bring about . . . [scientific] behavior are explained . . . by phylogenic contingencies" (p. 274).

Of the two references "Contingency, of survival" has to itself, one is "In the long run, contingencies of human survival come to influence the behavior of the scientist" (p. 90), and the other is "in the long run, contingencies of human survival will control the behavior of scientists" (p. 93). In what seems to raise a question of group versus individual selection, Zuriff offers a contrast to these long-term contingencies: "In the short term, however, scientists will adopt whatever they individually find most rewarding" (p. 93).

"Natural selection" also has two page references to itself. One of them leads to a discussion of Hullian theory in which logical confirmation is contrasted with confirmation taken in the sense of natural selection (p. 257). The other leads to a prominent passage on the final page of the concluding chapter: "A community which uses a methodology that is most effective for its adaptation will, by definition, have the greatest probability of adapting and surviving. Other communities, not possessing equally effective methodologies, will not survive as well in this cultural form of competition and natural selection" (p. 278).

No doubt the above material could have been expanded with presumed allusions to selection that appear elsewhere in the book, but even if so the total text devoted to this topic would remain relatively small. Yet the content of the brief passages that have been quoted here looms large, and there is much more that is implicit. It is clear that Zuriff is at the least sympathetic to the selectionist view, and for that very reason it seems strange that selection plays so small an *explicit* role in the book as a whole. Perhaps this commentary will deliver into his hands an opportunity to enlarge upon the role of selection in the varieties of behaviorism.

Behaviorism and the education of psychologists

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Zuriff's book is a long overdue and much needed contribution to the task of restoring scientific standards of theory construction in psychology. While reading it, I noted passages that will probably lead those of my colleagues adhering more closely to Skinner's personal point of view to voice some objections, but criticism of the particulars pales in significance compared to the overall sophistication of the book. Zuriff has done a careful, insightful, and scholarly job.

When behaviorism was first promulgated as a formal program (Watson 1913), it immediately became the primary focus of debate among the psychologists of that period, as may readily be verified by scanning the contents of the *Psychological Review* during the years that followed (see also Logue 1985). Behaviorism achieved an early victory in that debate and became the dominant and indeed the definitive point of view in academic psychology (e.g., Bergmann 1956; Chaplin & Krawiec 1968; Leahey 1980, p. 312; Schultz 1975, p. 358). Other schools largely disappeared, rendering the very concept obsolete. Nevertheless, discussion of the issues that had been raised by the behaviorists continued to occupy center stage in theoretical discussions and to play an important role in the education of incoming generations of psychologists for several more decades.

Eventually that changed. Less innovative and less talented writers took advantage of the continued emphasis on theory construction to publish their views. The major issues had been thrashed out and the residue was less stimulating. Interest waned. At the same time, important changes were taking place in psychology as an organized profession. When applied and academic psychology joined forces in 1945, the newly constituted organization included only a few thousand members (Wolfle 1947). But during the postwar period, the influx was enormous (Sanford 1951). The capacities of the major graduate departments were strained, and many institutions of lesser quality began offering graduate programs. Moreover, the majority of the newly trained psychologists were applied in their interests and not greatly impressed with the relevance to their work of such "academic" issues as the importance of operational definition or the proper use of intervening variables.

Among those who were genuinely interested in teaching and research an increasing specialization occurred. For these individuals, the graduate faculty furnished the role model: To secure similar positions, in which they too could obtain grants, furnish their laboratories, train additional students, and make names for themselves, they had to perfect their skills in specific research technologies and to produce an initial string of papers that would attest to their competence and their dedication. More than ever, the competitive pressure to publish at a high rate came to dominate graduate training, both for the head of the laboratory, who needed federal grants to buy the necessary equipment and to hire personnel, and for the student about to embark on the search for a job. As research technologies became more sophisticated and the literatures on specialized topics burgeoned (see successive volumes of *Psychological Abstracts*), the best and the brightest among our graduate students had little time left for the comparative luxuries of general theory or philosophical underpinnings. Although skilled in their specific areas of research, they achieved little sophistication on broader issues.

As far as I have been able to determine, behaviorism has not lost favor because any new arguments have been brought to bear. Indeed, there has been little in the way of thoughtful and responsible examination of the issues during the past 20 or 30 years. The interested reader can check the extensive bibliography that Zuriff has furnished. I have searched in vain for any specific criticism of behaviorism per se – as distinguished from the empirically derived theories of behavior, which are much more open to debate – that requires an extended answer. Most critiques have depended upon gross misrepresentations of the behaviorist position, bordering on caricature (Dinsmoor 1983).

Behaviorism appears to have lost ground simply because academic psychology has failed in its task of educating the latest generation of students. Note that we are not born behaviorists; it requires training. Like folk medicine, what might be termed folk psychology may have its moments of insight, but it is scarcely a scientific approach. The beginning student arrives on the scene already imbued with incompatible patterns of thinking derived from the surrounding culture – popular beliefs and above all popular concepts reinforced by both parents and peers. Contemporary cognitive psychology accepts these traditional conceptions of human functioning and builds upon them in a relatively uncritical way. It ignores the data obtained with other nonverbal species and thus loses the heuristic values of such research. Its basic assumptions are not very different from the popular view, and what little is new is readily assimilated. No significant change is required in the pattern of one's thinking to become a participant in cognitive research or to teach it to others. The basic tenets of behaviorism, on the other hand, and the basic concepts of conditioning theory, as an associated subject matter, are quite foreign to the beginning student and require a significant amount of effort and experience to understand. It is only through a patient process of wrestling with alternative interpretations of events, coupled with exposure to a modicum of instruction in the techniques of theory construction, that the student comes to comprehend and to accept a more sophisticated approach.

If the present situation is to be remedied, renewed efforts must be made to call attention once more to the broader reaches of psychological theory and to simple matters like the need to tie one's theoretical terms – whether they be hypothetical constructs, intervening variables, or simply classifications of environmental and behavioral events – more closely to the original observations. The task is neverending, and seen in this light the results may not appear to be highly reinforcing. But in the past, the behaviorists have served as the scientific conscience of psychology. It seems to me that we have a moral obligation to continue to do so. If we take this obligation seriously, exemplary interpretations of substantive research are not enough. We need to recommend Zuriff's book to our friends. We need to add courses in theory construction to the departmental curriculum. Even those who disagree with major features of Zuriff's analysis could use his book as the text for a graduate seminar, and those who agree more consistently could teach directly from it. And finally, we need to applaud significant efforts by other writers to convey the necessary information to the psychological public. This is one of the things I am trying to do in reviewing Zuriff's book.

Behaviorism as the praxist views it

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Zuriff has missed the forest for the trees. His thorough reexamination of dozens of variations and repetitions of Watson's manifesto is everything it appears to be: yet another variation and repetition of the manifesto. He does more than explicate and organize. He *defends:* Information-processing concepts are inadequate *because*. . . . Determinism is a necessary "working assumption" *because*. . . . The self is a faulty concept *because*. . . . The postulation of internal representations, transformations, and conscious contents is unnecessary *because*. . . . Stimulus-response formulations of language are adequate *because*. . . .

Zuriff's is not the first exercise of this kind; it is merely the first exercise of this kind to contain so many references. Why has it been necessary, again and again and again for 70 years, to repeat and explicate Watson's position? Could it be, as Zuriff would have us believe, that the message is so incredibly profound?

In a series of recent papers (e.g., Epstein 1984; 1985a; 1985b; in press a; in press b), I have offered a different solution to this vexing problem. As Zuriff correctly notes, these days the word "behaviorism" refers to a school of philosophy. But the referent was different in 1913. Behaviorism was *a movement for the reform of psychology* – specifically, a movement to replace psychology's traditional and etymological subject matter, mind, with a new subject matter, the behavior of organisms.

The psychology that emerged in the mid-1800s was an exciting enterprise – the attempt to apply scientific methods to the understanding of age-old philosophical subject matters: mind, feelings, volition, and so on. "From the most ancient subject," declared Ebbinghaus (1885), "we shall produce the newest science."

In the 1800s and early 1900s, another new science was in the making, a science of behavior. J. S. Mill proposed such a science in 1843; the zoologists Parker and Haswell did so in 1897; London physician Charles Mercier called for such a science in 1911, and his call was echoed by Dunlap (1916), Hunter (1925), and others. But in 1905, the new science was effectively derailed. William McDougall, in his *Primer of Physiological Psychology*, defined psychology as "the science of conduct." "What was needed," he later wrote, "was not a new science of behaviour under a new Greek name, but rather a reform of psychology; consisting of greater attention to the facts of behaviour" (Watson & McDougall 1928, p. 57).

Watson (1913) turned this curious suggestion into a conspicuous, though not entirely effectual, movement, and to McDougall's innocuous program he added a prohibition against the study of mind. Watson's program was outrageous; declaring another field your own does not make it so. The existing field was bound to resist, and it did so successfully.

Undaunted, Watson and his followers continued to wave the flag and to repeat and elaborate the original manifesto. The science of behavior hobbled along, isolated from biology, in departments where, for the most part, it was not welcome. As its proponents struggled valiantly for floor space and journal pages, they continued to elaborate Watson's message, until behaviorism emerged as a vast set of interrelated assumptions and assertions, as exemplified by Zuriff's book. Modern behaviorism is, in effect, *the rationale for why students of behavior should be allowed to take over psychology departments.*

Zing Yang Kuo, in a brilliant paper (not cited by Zuriff) published in the *Journal of Psychology* in 1937, saw the futility of the behaviorist movement and argued strongly for the creation of a biologically based science of behavior. Psychology, said Kuo, should be left to the psychologists. It is not an "ism" we need, he said, but a true empirical science.

The time has come to clear the air. For one thing, we must stop telling psychologists how to do good science. The studies of cognition of which I am aware are rigorous, empirical, and objective, almost to a fault. As Bergmann (1956) pointed out, behaviorism's major impact on psychology was to make it empirical and objective, contrary to Zuriff's assertions. What the movement failed to secure was the new *subject matter*.

Psychology must be set free of its intruders, and the intruders must somehow find a way to establish a comprehensive, biologically based science of behavior – a *praxics,* if you will – along the lines suggested by Kuo and others early in the century. Finally, the new science must be set free of the 70 years of philosophical baggage that has accumulated while praxists struggled, unsuccessfully, to appropriate a field that was not theirs.

If that science had gotten off the ground when it was first proposed, behaviorism would not have to be reconstructed today; it would never have come into existence in the first place.

Genetic factors in behaviour: The return of the repressed

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Zuriff's book is an excellent exposition and defence of neobehaviourism. Having rightly dismissed primitive forms of the simple reflexological model, he expounds doctrines admitting intervening variables and hyopthetical constructs, thus making the theory flexible enough to cope with many of the facts it has to encounter. The resulting structure is the only theoretical system of any inclusiveness that we have in psychology, and hence must command respect. It contrasts dramatically with the utter failure of cognitive psychology to produce a similar worked-out system of description and explanation. As Allport (1975) has pointed out, the field of cognitive psychology is characterised by "an uncritical, or selective, or frankly cavalier attitude to experimental data; a pervasive atmosphere of special pleading; a curious parochialism in acknowledging even the existence of other workers, and other approaches to the phenomena under discussion; interpretations of data relying on multiple, arbitrary choice-points; and underlying all else a near vacuum of theoretical structure within which to interrelate different sets of experimental results, or to direct a search for a significant new phenomena" (p. 141; see M. W. Eysenck, 1984, for a more positive evaluation).

Yet, alas, neobehaviourism, even in the enlightened form given by Zuriff, also contains in places a "frankly cavalier attitude to experimental data"; it too carries "a pervasive atmosphere of special pleading"; and here too we find "a curious parochialism in acknowledging even the existence of other workers, and other approaches, to the phenomena under discussion" (Allport 1975, p. 141). This is particularly true in relation to the important fields of *individual differences* and *behavioural genetics.* There is nothing in principle to prevent behaviourists from incorporating the facts of genetic determination of phenotypic behaviour, and the universal appearance of individual differences into their system (Eysenck & Eysenck 1985). Can it really just be the preference for a "black box" approach of Watson and Skinner, or the unreasoning environmentalism of both, that has prevented behaviourists from looking squarely at the facts? If, as Watson maintained (Watson & Rayner 1920, p. 1), "psychology as a behaviourist views it is a purely objective experimental branch of natural science (whose) theoretical goal is . . . prediction and control," then surely there is nothing to prevent the recognition of genetic factors and individual differences; indeed, it might be said that both are vital aspects of the prediction and control of behaviour. Equally, if, as Zuriff maintains, "the conceptual reconstruction of behaviourism is the elaboration of the fundamental premise that psychology is a branch of natural science given the two corollaries of empiricalness and objectivity" (p. 10), it can hardly be denied that behavioural genetics and the study of individual differences are both empirical and objective, and hence must form part of behaviourism (Fulker & Simmel 1983).

Zuriff's discussion on pages 178 to 180 is inadequate and factually wrong. He admits that "internal causes of this type [the organism's genetic endowment] do not necessarily upset the S–

R formula." As he points out, "Behaviorists can assume the genetically determined structure of the organism as an initial condition and seek to explain behavior as a function of environmental variables, given this structure" (p. 178). Of course behaviourists not only *can*, but *should* do this, but they have completely failed to do so, and my own attempts to make behaviourists aware of this necessity have met with a stony silence (Eysenck 1983). Taking prediction alone, we can predict a person's intelligence (Eysenck 1979), or his criminality (Olweus, Block & Radke-Yarrow 1986), better from a knowledge of the behaviour of his biological parents than from knowledge of his conditioning history in the environment furnished by his adoptive parents. Equally, we can predict the effects of a given positive or negative reinforcement much better by knowing the personality structure of a given human subject, or even a rat, than we could possibly do without that knowledge; does that not make it relevant to Watson's criteria for a science of behaviourism? These points are not even discussed by Zuriff or other behaviourists.

Zuriff, like Skinner (whom he quotes), seems to imagine that once genetic factors are implicated in phenotypic behaviour, this puts them beyond the pale of environmental intervention. This is not what modern genetics is all about. Heritability is a population parameter, which applies strictly to a given population, at a given time. Change the environmental parameters and you may change the heritability of the phenomenon in question. Indeed, the knowledge given us by genetic experiments may determine the kind of environmental intervention that may best change the situation and enable us to obtain better control.

To talk, as Zuriff does, as if external and internal causes were entirely separate, foreordained, and immutable, is to disregard all we know about biology. Internal and external causes are constantly interacting, and a refusal to look at one on the basis of worn-out shibboleths is scientifically inexcusable. Where 50% or more of the variance in the phenotypic behaviour classified as personality (Fulker 1981) or ability (Eysenck 1979) is genetically determined, it is futile to try to exclude such factors on an ideological basis. Indeed, Skinner conceded the fundamental importance of behavioural genetics and individual differences in his reply to my presentation at the debate between us at the Annual APA Conference in Montreal (1980), and even Watson recognised the importance of these factors in his paper on Little Albert. As he points out, commenting on Little Albert's long maintenance of the conditioned fear he demonstrated, "One may possibly have to believe that such persistence of early conditioned responses will be found only in persons who are *constitutionally inferior*" (Watson & Rayner 1920, p. 14; italics mine).

Given these admissions, why have behaviourists resolutely refused to incorporate genetic factors into their system? Zuriff does not give an answer but sidesteps the problem. Surely a philosopher familiar with David Hume should not quote without comment such questionable statements as Sechenov's that "the real cause of every human activity lies outside man." He also quotes Skinner to the effect that: "We cannot account for the behaviour of any system while staying wholly inside it; eventually we must turn to forces operating upon the organism from without" (p. 179). This is certainly true, but no geneticist has ever denied it, and it is irrelevant to the suggestion that no explanation of behaviour is possible or meaningful which relies *entirely* on external factors!

One can understand that Watson – in whose time knowledge of genetics was extremely limited and the study of individual differences was in its infancy – should have put forward a programme minimising the contribution of both these factors. It is more difficult to account for the position of Skinner, who, to judge by his response in our debate really knows better. [See also *BBS* special issue on the work of Skinner, *BBS* 7(4) 1984.] Zuriff fails to look at this issue squarely, although he set himself the task of an impartial investigation of behaviourism. This cannot be done by neglecting crucial determinants of behaviour. Behaviourism is too important a movement in psychology to be left to its own devices; it must be dragged kicking and screaming into the second half of the twentieth century, and forced to recognise factors it has been repressing for too long. It seems a pity that Zuriff neglected to point out this huge gap in modern behaviourists theories.

First-person behaviorism

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> For the behaviorist enterprise to make any sense, the absence of a free self must be assumed. (G. E. Zuriff, p. 177)

The behaviorist enterprise includes the prediction and control of behavior (e.g., 8–9, 31–32, 177–78, 180–81, 248–49, 262–66, 277–78). If behavior can be predicted and controlled, the absence of a free self must be assumed. But the claim that behavior can be predicted and controlled is permanently controversial, because of the naturalness with which we think ourselves free and the unpredictability and uncontrollability we seem to notice in behavior. It can seem, when one looks at behavior from the point of view of behavioral science, that there is no room for freedom of self at all; but when we regard ourselves from within the patterns of daily life, the absence of a free self can seem impossible to assume.

It is difficult not to see this as a dilemma. It is clear that the way science works is to suppose there are impersonal causes to everything. And it may be that science needs to make this assumption not just to predict and control phenomena but to explain them. The assumption may provide the best hope of rationally justifying explanations of events; but offered as an assumption to be made about ourselves, it seems overridden by the impulse to take ourselves to be free.

Professor Zuriff has written a careful and stimulating book. It is particularly commendable because he has surveyed the variety in behaviorism and either developed or sketched replies to standard objections. I simply wish through this brief commentary to stimulate in him the attitude I have that we might not be able to construct a behaviorism for ourselves. We can think of *others* as products of conditioning and genetic endowment, but this is a difficult if not impossible view to have of ourselves.

The place we occupy with respect to our sense of personal freedom is different from the one occupied with respect to (to use Zuriff's analogy for Chapter 9) belief in demons and ghosts. To learn, for example, that seizures are caused by neurological dysfunction is to learn something which can and does displace belief in demonic possession and the necessity of exorcism. But to think, for example, that behavior is caused by reinforcement histories and discriminative stimuli, is this to think something which we can believe of ourselves? What would it be like to conceive of oneself as the product of conditioning? Wouldn't we feel trapped in certain respects, by externally imposed limitations and unchosen conditions of action? Shouldn't our sense of choice become defused?

Consider the sense of choice. Choosing – consciously – feels like this: There are various reasons for and against each of the alternative actions or courses of actions one considers, and it seems as if one could do any of them. In considering reasons, one arrives at a picture of which reasons are important, which ones are more pressing. One decides what reasons to act on; or one may decide to act on none of them but to continue deliberating. [See Libet: "Unconscious Cerebral Initiative and the Role of Conscious Will in Voluntary Action" *BBS* 8(4) 1985.]

The feeling of choosing (among open possibilities) is essentially first-personal; it is not the feeling that a particular, publicly identifiable body moves freely, though its qualitative content requires the movement of something like that, but the feeling that I am moved by events which I control. I move me (see Kane 1985). Perhaps I shall eventually tire of the feeling, but at the moment I cannot imagine what it would be like to choose and not have it. Nor can I understand those theorists (e.g., Dennett 1984) who promise to explain the feeling as the product of a mechanism in me, something I take to be unfree. Is it possible that my feeling of free choice is the upshot of a randomizing element in my brain serving me up with unanticipated thoughts, motives, and reasons? It is the prospect of being such a machine – perhaps protected from the feeling of being constrained, by random events which girder my behavior against tropism – which at times most seems incomprehensible to me.

Although I have no explanation for the feeling of free self, I believe it must be shared by people other than me. In other words, it can't be accounted for by facts from just my history, any more than my interest in food, clothing, and shelter can be explained because I was born and raised in Brooklyn. Also the constituents of the feeling are complex; it is not just a transitory sensation, like heat or warmth. Within it there is apprehension at the possibility of failure or misfortune, and pride and self-respect in choices well-made, decisions well-done. I imagine doing things for which, as far as I am concerned, my biography has not prepared me; there is emotional incredulity when I discover that something I had thought I had done freely is given a causal explanation. Feeling free I am sensitive to the presence of any causal undertow carrying me in one direction rather than another. And powerful and appreciated support for the feeling, for the conviction of being free, comes from the moral rights which I ask others to respect in me, to respect my freedom and autonomy.

In choosing, we think ourselves free. In embracing the behaviorist enterprise, we assume we are unfree.

Near the end of Chapter 9 Zuriff remarks that the two languages of choice and agency, on the one hand, and response patterns and reinforcers, on the other, "categorize the world in different ways, and the two resulting systems are not easily related" (p. 197). Indeed. One seems forced on us in the first person; the other seems to place us in the third. One is the language of the inner view of the agent; the other is the language of the outer, impersonal view of the behavioral scientist. The outer, impersonal view of the behavioral scientist is the one which must be adopted by a behaviorist. For from within the inner view even a behaviorist will seem to himself to be free.

"Higher criticism" of behaviorism

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There are ways in which a reconstruction of behaviorism, as Professor Zuriff sees it, is like a reconstruction of the Bible. There are those who hold that the Bible was written by God. Zuriff does not suggest that the book of behaviorism was written by any similar deity, and B. F. Skinner appears merely as one of the prophets. But the version of behaviorism which results is nevertheless rather like the results of "higher criticism." Themes have to be derived from a survey of the material, things have to be put in their place, and the wilder fantasies of the movement have to be "demythologised," so that what remains has scientific respectability, seen in terms of what the theory does for the prediction and control of behavior.

There are three important questions to be raised about all this. There is, first, a question arising from the philosophy of science. Is it true that the aims of a scientific theory should be merely prediction and control? Suppose what is dubiously the case – that on the basis of behaviorist theory it is possible to predict what is likely to be the case with respect to a certain kind of animal or human being in a variety of contexts. Suppose too that on this basis it is possible to control the behavior of this kind of animal or human being in those contexts. That would not be sufficient to make the theory acceptable. The question would still arise whether the correctness of the predictions derived from a proper understanding of the phenomena. After all, Ptolemy could predict the movements of the heavenly bodies on the basis of his theory, and so could Aristotle up to a point. But their theories were just wrong and involved a totally mistaken understanding of what they were concerned with.

My other two questions derive in a way from this first one. Because Zuriff's account is a "reconstruction" he is continually put in the position of trying to set out what a behaviorist *might* say on a number of issues. One of the great obstacles to a behaviorist's understanding of his subject matter, including behavior itself, is that he has to ignore first-person reports of what is going on in the subject's mind, and so ignore also the reasons which the subject may have for doing whatever he does. Zuriff suggests that such first-person reports may be ignored on the grounds that they have no implications for the prediction and control of behavior. It is very questionable whether this is true. Can one ignore first-person reports in this way? Much more argument on this point is needed, and a clearer attention to philosophical discussions of it. As a second line of defense Zuriff suggests, as others have often done in the history of behaviorism, that first-person reports might be construed as discriminative responses to stimuli. It might be suggested that this proposal has its philosophical precedents too. Did not Wittgenstein propose that first-person remarks on feelings should be interpreted simply as expressions of those feelings? But the expression of a feeling is not, strictly speaking, a response to a stimulus, and to the extent that such discriminations are involved they are insufficient to account for all that is involved in the expression of feeling. That expression must reflect the character of the feeling as well.

Chapter 11, which discusses these matters, is a good example of Zuriff's tendency to go through possible behaviorist ploys in response to a problem. The discussion is very defensive in a way that reinforces yet again my comparison with "higher criticism." Something has to be preserved, it seems to be suggested; there are such and such possibilities of doing so. But surely what is wanted is the truth – the correct way of understanding the situation, not merely how it might be understood if we were determined to observe supposed criteria of scientific acceptability of a positivist kind.

My third question is concerned with the terms of reference of the whole exercise. Zuriff recognises in Chapter 3 that there is a problem about what is to count as behavior. He rightly wishes to avoid the reduction of psychology to physiology which would result from identifying behavior with muscular and other physiological movements occurring in the body. On the other hand, he will not have in the "behavioral data language" anything that savours of the "inferential, intensional, purposive, molar" or what is expressed in "action language." What then is behavior? It is not bodily movement, and it is not action. This has always been a problem for behaviorism, and behaviorists, ever since J. B. Watson, have fudged it. It is fudged even in the language of stimulus and response; for these notions are not used in a sense that implies simply the application of some form of energy to a nerve-ending, with corresponding reaction. As many critics, both philosophical and psychological, have noted, "stimulus" and "response" are two of the most ambiguous terms in psychology. Zuriff suggests that this sort of thing does not matter; we must appeal simply to intersubjective agreement and so-called scientific judgment. That is not good enough for acceptable theory.

Zuriff's book is really an attempt to shore up what ought to be abandoned forever.

Rebuilding behaviorism: Too many relatives on the construction site?

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Especially for a reader who identifies with the behaviorist community, Zuriff's initial chapter sets up an attractive project. The rationale of conceptual reconstruction allows for the inclusion of ancient, modern, notorious, and obscure contributors to the behaviorist tradition, while finessing the potential distraction of historical precedents and credits. I was left curious as to just how behaviorism would be taken apart and reassembled, and especially wondering whether the technique would mitigate behaviorism's enduring problem of being easily misunderstood and/or maligned. The notion of "family resemblance" is an engaging one also, leaving one interested to see which grandparents, uncles, aunts, and cousins will be included (and of course whether one's own prespective will be evident or at least well represented in the portrait).

Unfortunately, the two priorities seem to have conflicted. Too much residue of earlier constructions was left on site. Perhaps the lumbering with old chestnuts, objections, and outdated criticisms was essential to preparing the site. However, these often seemed to serve mainly to introduce those who cleaned them up, populating the family portrait but obscuring the reconstruction. At various points in picking through the rubble of old arguments Zuriff gave too little notice to the special pieces that offer exciting possibilities for contemporary construction.

Some newer timber could have provided a stronger foundation. Rather than challenging introspection as a valid method for proceeding, one could challenge a prior premise, of "the mind" as a thinglike entity to be observed. In letting references to *the* mind pass unchallenged one unnecessarily grants coherence to conventional notions that are predicted on mind as stuff (whether physical or mental). Carefully, consistently using *mind* instead of *the mind* would help to make introspection more suspect at the outset and to make clear that the examination of sensing and the like is a quicksilver business. Excision of *the* from *mind* also undercuts later counterarguments to the behaviorists position on agency, and helps obviate the need to address questions that are raised mainly by the implicit dualism of ordinary language. Furthermore, *mind*, as "ongoing part of your own doing," is an inviting and interesting notion, useful when introducing behaviorism as an unusual viewpoint.

A related gambit would be to consistently invoke "we speak of," when addressing certain terms that originate outside the behaviorist rubric. This enables one to avoid appearing to accept assumptions that are often implicit in those terms. Thus: "If we could speak of mind as integral entity of behaving, rather than as a thinglike entity, we could speak in a way consistent with a behavioral account." A few other examples: On page 149, instead of "attempt to account for purpose and intention" one might say "attempt to account for what is involved when we speak of purpose and intention." On page 171: "Behaviorist theories *are* obliged to explain the behavioral phenomena to which the term 'thinking' refers" could be replaced with "behaviorist theories *are* obliged to account for the situations and phenomena wherein we speak of thinking." And on page 192, "many [behaviorists] attempt to explain why we have a concept of agency" could be replaced with "many behaviorists attempt to account for our speaking of agency."

Prediction and control should still be an early node in the exposition, but when introducing behaviorism to the world at large, we need not follow Watson by declaring these as fundamental goals. That traditional stance is too easily misunderstood as advocating a society controlled by psychologists in white coats. It would be better to introduce prediction and control as behaviorists' bases for doing science and for enhancing the individual's effective participation in the world of living organisms. As bases for experimental science, these come prior to objectivity and agreement between individuals. To be sure, bases for coherent agreement or disagreement between individuals evaluating a theory or interpretation are bases for objectivity, and they are standard accoutrements of science. Furthermore, one's learning to behave scientifically originates in a social environment, and one's continuing scientific activity benefits from joint participation with others. However, by the behaviorist account, one can in principle do science while entirely alone (Skinner 1945). [See also special issue on the work of B. F. Skinner, *BBS* 7(4) 1984.] Thus, for both strategic and conceptual reasons, prediction and control are best embedded in a discussion of epistemology rather than presented out front in the invitation to consider our viewpoint. (Incidentally, could one not achieve an initial exposition without including the vernacular expressions "goals" and "purposes?")

As conceptual reconstruction, the book contains one major strategic blunder, namely, the perpetuation of the S–R label. Zuriff provides a thorough exposition of the ways in which this does not characterize the behaviorist position; yet the label is given prominent and repeated use that suggests it is a central and apt metaphor for the contemporary behaviorist position. Away with it, please!

The early and the late sections of the book, then, impress me mainly as a family portrait rather than a conceptual reconstruction. As a portrait they are informative. As a reconstruction, these sections contain too much rehashing of old arguments without emphasizing the exciting possibilities of a particular, internally consistent, contemporary behavioral viewpoint.

In between, there are sections that strike me as conceptually clear, current, and strong. The arguments and counterarguments vis-à-vis the Chomskian and the cognitive positions are especially well done. Identifying the Skinnerian treatment of private events as an appeal to hypothetical constructs certainly gives me pause, and the discussion of intercurrent behavior identifies some promising features that I had not recognized in my own position. The progression beginning on page 163, from psychophysics to pattern recognition to meaning, is as incisive a piece of writing as I have ever seen.

Thus, I would like to see a sequel, a lean and spare exposition of Zuriff's own viewpoint. It would draw less from distant blood-relatives, who have carried the behaviorist label, and perhaps more from the "in-laws" of adjacent fields, such as J. J. Gibson in visual perception (Costall 1984) and Marvin Harris in anthropology (Lloyd 1985; Vargas 1985). Behaviorism is an unconventional, even countercultural viewpoint, but it shares distinct features with other intellectual traditions.

Zuriff on observability

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Gerald Zuriff has written a very fine book. Better than any that I know, it shows what behaviorism is about and why it has appealed for so long to so many people. In doing so, the book provides us with an account that is far closer to the real thing than the caricatures that often represent it, in the minds of its critics. It also shows how behaviorists solve some problems that are often thought to be insoluble using their assumptions.

As Zuriff clearly sees and says, behaviorism is the insistence that psychologists limit themselves to what is publicly observable. But what is the test of observability? According to some behaviorists, it is physics. Things describable in the language of physics are observable; things not so describable are not. Thus, bodily reflexes are observable; the contents of the mind are not. To believe this is to believe that psychology can be reduced to physiology.

Zuriff does not believe it. On his view, the contents of the mind are also publicly observable, because they are manifest in behavior. Thus, we can quite literally see a man's fear in the expression on his face and hear it in the tremor in his voice.

This is an audacious claim. At one time, everybody thought psychological conditions to be observed by nobody but the person in them: I observe my fear; you do not. Not so, says Zuriff. My fear is observable to you, and yours to me. Fear is not an introspectible but otherwise unobservable quality of the mind; it is a function relating publicly observable stimuli to publicly observable responses. So, most of us know how to recognize it – not just in ourselves but also in others.

Doesn't this claim confuse observation with theory, datum with interpretation? No, says Zuriff. The distinctions between these is not fixed in the nature of things. Every fact involves some interpretation. So, what counts as datum and what as theory depends on the agreement of the inquiriers. That the man ran away in fear counts as a datum if it secures agreement; only if that interpretation of his behavior is subjected to dispute need it be relegated to the status of theory. The test of observability is consensus.

That is why physics puts no limits on the vocabulary of psychology. If we were to take physics as our measure of reality, we would have to conclude that there is no such thing as color, because there is no word for it in physics. Color would have to be regarded as an illusion – not something in the world but the way we perceive the world. Ditto fear, but not matter. For most of us, the green color of the grass counts as observable, because we all perceive grass as green. Similarly, a man's fear counts as observable, so long as we all perceive him as being afraid.

In my opinion, this is the most compelling defense yet written for behavioristic psychology; but there may be a snake in the woodpile. Normally, we define observability in terms not of consensus but reality. Thus, the stick that is half in water is not proved to be observably bent by the fact that it looks bent to all observers. Consensus is only a test of reality; it is not a guarantee thereof. Suffering from DT's, I see pink rats; you do not. That proves that the rats are unreal; if they are unreal, they are also unobservable. But they would remain unobservable if, suffering from DT's like me, you saw them too.

As Zuriff understands, the problem here is ambiguity in the concept of observability. Zuriff tries to resolve it by choosing a conept that is "pragmatic" rather than "metaphysical." In other words: No claim is made that consensus guarantees truth. Rather, the claim is simply that we should be satisfied to count as true that to which we are all agreed. Whether our agreement with each other secures agreement with reality is a question we need not address.

At the risk of being thoughts a stick in the mud, I must point out that the question cannot be evaded that easily. One might as well say, "I find it satisfying to believe that I have a million dollars in the bank. So, I will. Never mind whether I really do have a million dollars. Such metaphysical queries bore me." That would be very bad policy.

Sensing as much, Zuriff sometimes takes a slightly different line: He *defines* truth as agreement not with reality but among ourselves. Unhappily, doing that is worse. As Peirce pointed out to James, requiring intersubjective agremeent helps to eliminate error, but it does not guarantee truth. Thus, consensus that the earth is flat never made it so. Besides consensus, there are other requirements, namely, workability in practice and coherence with the rest of what we believe.

The question that color and fear pose is this: Given that belief in them has consensus and helps us to find our way around in the world, can we reconcile it with the story that physics tells? And if we cannot, what must we conclude? That only physics is true, and psychology is just a useful social myth? Or that physics and psychology are independent enterprises which tell not contrary but only different stories, each true in its own terms?

These are hard questions, so hard that behaviorists, who are practical-minded by definition, do not wish to take time to resolve them. Echoing other behaviorists, Zuriff says: Let us put them aside and get on with the work. Fair enough, with either eliminating it or answering it.

Zuriff's counterrevolution

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"What is needed . . . is an accurate portrait of behaviorism and an honest search for what is still valuable in it." This concluding sentence of Zuriff's *Behaviorism* not only describes a worthy objective but also neatly summarizes Zuriff's own substantial contribution. That a clarification of behaviorism is needed is illustrated by two recent experiences. While discussing Tolman and Hull in my history class, a student resisted my classification of them as neobehaviorists because his text on cognitive psychology (whose author shall remain anonymous to protect the guilty) states that behaviorism is opposed to theoretical formulations. The other case, with the identity of the culprit also remaining anonymous, was a strong appeal to delete Mandler's perceptive comment from a particular historical analysis (Kendler 1987): "We [cognitive psychologists] have not returned to the methodologically confused position of the late nineteenth century, which cavalierly confused introspection with theoretical processes and theoretical processes with conscious experience. Rather, many of us have become methodological behaviorists in order to become good cognitive psychologists" (Mandler 1979, p. 281).

The opposition to Mandler's comment stemmed from the conviction that cognitive psychology represents a complete revolutionary break from behaviorism. The natural science orientation of many "cognitive psychologists," according to the criticism, is not inherited from behaviorism but is instead an intrinsic property of cognitive psychology. Why is there such a powerful hand, revealed throughout the history of psychology (Kendler 1987), to break with the past? It would seem that "truth value" is automatically assigned to "revolutionary" paradigm when the "inadequacies" of a prevailing paradigm are "demonstrated," just as behaviorism has served as a convenient "whipping boy" to elevate the "truth value" of cognitive psychology.

Zuriff's *Behaviorism* is a counterrevolution against the prevailing opinion that cognitive psychology was a successful and progressive revolution against behaviorism. His message is clear: Behaviorism is, for the most part, misunderstood and unappreciated, thus denying to psychology important methodological lessons as well as potential contributions that reside in the unexplored notions of behaviorists and neobehaviorists. Zuriff's message is conveyed in a detailed analytic manner that offers subtle methodological distinctions that have gone unnoticed or have been completely ignored. I was reminded of a comment made to me by Ernest Nagel, the distinguished philosopher of science, who expressed admiration for another philosopher by remarking, "He makes so many fine distinctions!" If this criterion for methodological analyses is used, and I believe it has some merit, then Zuriff's effort must be admired, although it does carry the risk of losing readers whose commitment to detailed analyses is weak.

Zuriff's "conceptual reconstruction" emerges from the belief that psychology, in order to be a natural science – to be objective and empirical – must be behavioristic. However, he carefully points out that behaviorism is not a monolithic paradigm as many of its critics would like to believe. Although behaviorists share the assumption that behavior is the dependent variable of psychology, fundamental differences prevail about the meaning of theory (Skinner is wisely classified as a descriptive theorist), the potential of stimulus–response language, purposivism, active versus passive conceptions of organisms, mechanistic explanations, behavioristic analysis of mentalistic language, and behavioral epistemology. No doubt Zuriff's defense of behaviorism will be criticized because his epistemological orientation is "misguided": It is too dependent on "old-fashioned" and "rejected" logical positivism, he "naïvely" believes that theory and observations are separable, he fails to fully appreciate the intrinsic "subjectivity" of science. These and similar broad arguments will be advanced but they should be recognized more as debater's points than critical epistemological analyses. Zuriff has forced himself and his readers to grapple with fundamental methodological issues which cannot and should not be ignored, even at the risk of threatening a nice, comfortable view of psychology.

To demonstrate that his views are not accepted uncritically, I would like to offer some reservations, more if space were available. The root problem is that Zuriff sometimes conflates radical behaviorism with behaviorism (Kendler 1981; 1985; 1987). Most of the time Zuriff is careful to qualify the term *behaviorists* with such adjectives as "most" and "many," but sometimes he uses the universal mode: "Behaviorists tend to be deeply suspicious of [R–R] laws and explanations" (p. 181), and "Behaviorism's rejection of information-processing constructs stems from its claim that these constructs divert attention away from behavior, the environment, and the long conditioning history responsible for the construct" (p. 150). Behaviorism (Kendler 1985) can most clearly be conceptualized as the paradigm that selects behavior as the dependent variable of psychology. This may appear to be a trivial definition but only to those who are ignorant of the tortuous history of psychology during the period it was the science of mind and its major method was the direct examination of consciousness. There is no reason to consider stimulus–response and response–response laws incompatible. The litmus test is not how closely psychological laws are tied to environmental events but how much they contribute to the prediction and control of behavior. In actual fact, S–R and R–R laws can complement each other in arriving at a fuller understanding of behavior.

In a similar fashion, methodological behaviorism is not necessarily threatened by such concepts as representation and transformation. Behaviorists sharply disagree about the *strategy*" of using "mentalistic" concepts. At one extreme is Hull's (1943) position that mentalistic language creates a false sense of understanding and therefore supports the strategy of viewing "the behaving organism as a completely self-maintaining robot, constructed of materials as unlike ourselves as may be" (p. 27). At the other extreme is the frank mentalism of "subjective behaviorism" (Miller, Galanter & Pribram 1960) that has encouraged theoretical constructs with rich mentalistic connotations.

A historical experiment is now being conducted to determine the pragmatic value of a mentalistic strategy in psychological theorizing. And, as was the case when self-observation (e.g., introspection, naïve phenomenology) was judged to be the core method of psychological investigation, the meaning of "mental" is a source of much ambiguity. Are all cognitive psychologists – experimental information processors, computer simulators of human cognition, psycholinguists, cognitive developmentalists, developers of artificial intelligence – using a common language, or can the conceptual properties of their idiom be ordered on a mentalistic–physicalistic continuum? Was cognitive psychology truly a revolution against antimentalism or against the excessive restrictions of stimulus–response language, which stimulus–response mediational theorists were trying to overcome? Is the commitment to a model of the mind which characterized early cognitive theories being weakened by an increased concern with neurological processes (e.g., Posner 1984; Tulving 1985)? These and many other questions can be raised about the methodological–historical relationship between behaviorism and cognitive psychology, about their continuities as well as their discontinuities (Kendler 1984). In clarifying basic issues, Zuriff's book will serve as a source of bright illumination. Further light can be thrown on core problems by someone who will accept the challenge to offer a portrait of cognitive psychology that will match the accuracy and depth of Zuriff's interpretation of behaviorism.

The reconstruction of a conceptual reconstruction

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As I started to review this new text, I immediately resonated to Zuriff's description of his own conceptual foundations in behaviorism derived from Keller and Schoenfeld. I too had shared the influence of these two remarkable behaviorists, a bit earlier in history. This was clearly going to be a reminiscent, fruitful tour through the real behaviorism that we all know and love so well. Actually, my initial impressions and anticipations turned out to be fulfilled, albeit with a few minor caveats.

Throughout the book Zuriff exhibits a remarkable ability to present both sides of key issues. There is often a brilliant exploration of alternatives which cancel each other out. He makes clear that there are no easy answers, either psychological or philosophical.

An archbehaviorist might argue that a key element in conceptual reconstruction consists of the biases/values/belief systems of the reconstructor. Conceptual reconstruction is an act of creation.

As Zuriff indicates, it is true that behaviorists are usually not "overly concerned with philosophical nuances, nor are they averse to changing their views. Therefore, their pronouncements often reveal ambiguities, contradictions, and gradual changes" (p. 4). Thus by taking these "thoughts of individual behaviorists" out of their historical context, Zuriff is creating order where none existed before, and inventing consistencies, a process of creation for which he is to be congratulated. It is Zuriff who is inventing concepts and doing an exciting job – and since he is writing the book he is certainly entitled to do so.

Zuriff is to be commended for clearly and explicitly stating and justifying the "basic assumptions" and "sets of beliefs" on which this book is based. He offers a conceptual reconstruction of virtually the entire scope of behaviorism. This reconstruction "is organized around conceptual issues rather than historical periods or important individuals" (p. 3). He contends that historical reconstructions involve a focus on particular individuals, thus missing the "abstract continuity that transcends historical demarcations" (p. 3). It is *as if* the concepts take on a life of their own that transcends historical developments and individual behavior. Thus these intervening "concepts" become reified as if they were real. Zuriff is absolutely fair in explicating and justifying this basic philosophical approach. However, one might well wonder whether the approaches to the relationship between the individual, history, and "concepts" might not be more consistent with behaviorism itself. My own bias is that conceptual positions are not independent of the holders of the position, or the historical context in which they are developed.

The heart of a book on "behaviorism" is of course a definition and conceptualization of just what behaviorism is. Defining it (the label, the noun, the concept) has been a game in which everyone in and out of psychology and philosophy can and has played. There has been a multitude of literature on the various attempts to delineate just what it is and what is isn't, and mutual contradictions, confusion, duplicity, certainty, and doubt abound. This commentator's view is that are dealing with an *idea* (concept, model), expressed in verbal behavior, about the nature of human nature, as to what man *is* and *could become*. "Polemics, intemperate invective, *ad hominem* argument, and caricature pervade discussions of behaviorism by those who seek its demise" (p. 278) and by its supporters as well.

Zuriff nicely sets up the rules of the game in his first chapter and then proceeds to play by them. He is certainly entitled to do so since we all do. In fact, there is no avoiding this; but the interesting thing about the behaviorism game is that it can be played by different rules. For example, you could refer to behaviorism as "the behavior, mostly verbal, of those individuals identifying themselves as behaviorists." Zuriff himself states his position clearly in referring to the epistemology of Skinner, Hull, and Tolman. "Each views knowledge in terms of the behavior of the knower, and science, in particular, in terms of the behavior of the scientist" (p. 255). Yet, it is only in the last paragraphs of the book that Zuriff recognizes this as a possible way of dealing with the issues; but by then the reconstruction based on the mentalistic concept of "concept" has been com pleted. Much of Zuriff's conceptual reconstruction would probably have remained the same, but there may have been some sharp differences.

Zuriff refers to John B. Watson as the "founder and popularizer of the movement" (p. 7). As is true of every aspect of psychology (and of philosohpy), controversy abounds as to the origins and prehistory of behaviorism. Historians of psychology who were contemporaries of Watson such as Boring, Heidbreder, and Woodworth clearly credit Watson as being the "founder" of behaviorism. However, more recent writers of history such as Kazdin (1978) tend to credit Watson with "catalyzing a movement toward objectivism" (away from "consciousness") "that was already well in progress"(p. 64).

There is one major *concept* that Zuriff recognizes, then gives short shrift. That is the conceptual relationship between behaviorism and the concept of human "values." Zuriff presents as a basic premise one concept which he takes for granted: that "psychology is a science," and science itself is taken as a given, which must be "empricially based" and "objective" (p. 9). He recognizes that there is an association between the individual scientist-researcher and the ultimate goals of the science and seems to be leaning in the direction of science, hence behaviorism, as value-laden as against being value-free (Krasner & Houts 1984; Mahoney 1976). It is indeed noted very early in the text that "behaviorism also represents a certain set of values. . . . Values are even more salient with respect to applied behavioral science in which behaviorism promotes applications congruent with particular social aims" (p. 3). This observation goes to the core of the science and value issue. He further notes that behaviorism must be viewed as an ideology as well as a philosophy of psychology; one cannot be fully stated without the other. However, having made this astute observation at the very beginning of the book, Zuriff doesn't really follow up by including "values" among the reconstructed concepts. In fact, there is virtually no further discussion of the value issue until the last two pages of the book. Thus, in not including the value issue as one of the concepts he reconstructs, Zuriff gives implicit endorsement to the view of a value-free behaviorism, or at least implicitly it is not considered to be a major issue. For example, the reference section cites 50 publications by Skinner, yet *Walden Two*, which was Skinner's earliest avowal of a science-value linkage in behaviorism, was not included. For that matter, the 18 Watson citations do not include Watson's 1929 paper offering his value conception of an ideal society. Any field involving the "prediction and control" of behavior cannot, and should not avoid dealing with value issues.

We feel that it is symptomatic and symbolic, not a coincidence, that the two major influences in behaviorism – Skinner and Watson – have written utopias which attempt to apply their value version of the concept of behaviorism to the society at large. Bakan (1966), in an article aptly entitled "Behaviorism and American Urbanization," notes that:

> Behaviorism has been of no small concern to the society at large. Leaders of the behavioristic movement, from Watson to Skinner, have addressed themselves to the public at large with program notes on social reform; and public interest has not been small. However, the entailment of behaviorism with the culture at large should not only be regarded as from the laboratory to the larger society, but also inward, with the larger cultural and ideological features of the scoiety playing a significant role in the development of behaviorism itself. (p. 5).

Thus, implicitly and explicitly, within behaviorism are social value concepts which are worthy of "reconstruction."

The pragmatics of survival and the nobility of defeat

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Zuriff's book is an awesome achievement in analytic and synthetic scholarship. Nothing like it has appeared in recent memory. In an age of edited patchworks and conceptual vacuity in psychology, this book, regardless of one's views on behaviorism, reflects the highest standards of depth, balance, clarity, and organization. The title notwithstanding, the content touches on every significant area of psychological thinking and, by extension, the whole of scientific practice.

The book displays the conceptual richness, complexity, and diversity of views labeled "behavioristic." There is an enduring tendency to stereotype behaviorism among its critics and, in particular, to apply the epithet "simplistic." As this book makes clear, "simplistic" is a term better applied to the critics' description of the enterprise than to the enterprise itself.

An important characteristic of the behaviorist position is its focus on meeting the adaptive needs of the human community through effective prediction and control, that is, its pragmatic stance. What influence (i.e., effective control) can this book exert? Sadly, little, I believe. Certainly, very little in relation to the intellectual talent and effort devoted to its creation. Alternative conceptualizations comprising the collective known as cognitive psychology are so thoroughly entrenched that no reformulations or reconstructions of behaviorism are likely to exorcise it, or even to modify it significantly. Some of the reasons can be found in the principles outlined in Zuriff's book. Science is a social enterprise, the product of the behavior of scientists. Such behavior is under the control of special histories and current contexts manifested through contingencies of reinforcement. The practice of cognitive psychology provides sources and categories of reinforcers behaviorism cannot or has not provided. Many of the principles of cognitive science reflect intuitions and rule-governed behaviors established in a folk psychological environment; there is the special comfort of metaphor, especially one based upon the most influential and effective devices in history – the computer; there is at least the illusion of effective practice and progress in a variety of areas of special and direct interest to human society – memory, thinking, language, perception, and so on – areas of major human concern since recorded time (and, until recently, receiving little effective empirical attention from most behaviorists). Hypothetical constructs, processes, and modes of representation have provided heuristic energies to fuel thousands of studies and research programs encompassing fields as diverse as the effects of aging on human performance and the acquisition of concepts in the pigeon. This work is published in prestigious journals, supported by grant monies, and recognized and praised in the popular press. "Contingencies of reinforcement" and "stimulus control" are phrases not often seen in the cognitive literature, must less in *Time* magazine. As Zuriff points out, there may be no *logical* reasons for repudiating the cognitive program; it is largely a question of scientific strategy. The social contingencies clearly favor that strategy.

There is a kind of historical irony to the fading of behaviorism that relates to the reinforcing aspects of cognitive theory. Part of the appeal of at least some cognitive theories lies in their *foundationalist* positions. There are assumptions of processes lying behind mere "appearances," of a psychological versus a physical dichotomy leading to various forms of copy theory and supporting a realist metaphysics, framed in some positive ontological position. Formal logic and scientific activity itself are seen as foundational tools for the investigation of cognitive processes rather than as part of them. Formal categories, a priori and transcendental forms, and reified concepts are, along with the need for a proximal causality, highly characteristic of the cognitive enterprise.

In contrast, the direction of other significant intellectual endeavors over the last century has been to repudiate a foundationalist stance. Mathematics, once thought to reflect incorrigible knowledge of a transcendental realm and based upon a foundation of logic and selfconsistency, has lost its certainty (Benacerraf & Putnam 1984; Kline 1980; Wittgenstein 1978). Physics, which once flourished under a mechanistic, deterministic, rationalistic, materialistic, absolutist, and realist position woke up in the twentieth century to question all of these perspectives (French 1979; Herbert 1985; Jammer 1974). Philosophy itself, under siege by the likes of Dewey, Heideigger and, especially Wittgenstein, has lost its theorietical and universal foundations and with them the hope of achieving *episteme* (Rorty 1979; Wittgenstein 1953). Behaviorism can be seen as part of a general intellectual retreat from foundationalism. Indeed, a behavioristic perspective may be seen as a primary source (one hesitates to say a foundation) of this retreat – in physics, mathematics, and philosophy. I cannot develop this thesis further here, but Zuriff's book contains most of the elements.

Finally, in its antifoundationalist stance behaviorism shares a problem with mathematics in the establishment of its verifiability, its self-consistency, and its "effectiveness." A kind of Gödel's proof (Nagel & Newman 1958) must apply to the "bootstrap" character of the behaviorist position which must surely limit the possibilities of a behavioral epistemology. The "proof" itself would, of course, have to be a part of the system whose consistency it is questioning. One would seem to require a *theory* of effectiveness, of adaptation, of prediction, and of control. Can behavioral epistemology encompass that theory?

Is behaviorism under stimulus control?

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Behaviorism may or may not be dead, but it certainly won't lie down. In terms of its own principles, one might have expected the bad press behaviorism has received over the last few decades to have reduced the emission of verbal behavior that radiates positive S–R sentiments. I can therefore say, truthfully, that I consider Zuriff's scholarly and thoughtful book a tribute to the freedom of the human spirit, in the sure knowledge that Zuriff will not know whether to take my remark as a compliment or an insult.

Nonetheless, one does wonder exactly how much real radical behaviorism is left after Zuriff's "conceptual reconstruction." Much of Zuriff's argument boils down to a plea for as much "objectivity" and "evidence" as we can obtain before accepting any particular psychological claim. In principle, Zuriff seems prepared to accept any intervening variables and hypothetical constructs, however baroque they may be, but always provided that there is evidence in their favour or that they aid the discovery of new facts. Who could possibly disagree? Thus, Zuriff appears to have only strategic objections to the hypothetical constructs of generative grammar. The question of their *ultimate* validity he leaves open, as would anyone else. Zuriff's conclusion – "while Chomsky's theory may force a development within S–R psychology, it does not refute it, or, at least, a sophisticated version of it" (p. 149) – impels one to inquire what Zuriff would acknowledge as a refutation. And at this point one moves from science to ideology, as Zuriff himself is well aware (p. 3).

Behaviorism, with the emphasis upon the *ism*, "represents the assumptions, values, and presuppositions implicit in this science" (p. 2). What then are these values? Prediction and control, says Zuriff. Behaviorism will succeed because "it provides greater rewards in the form of prediction and control . . . and therefore will be chosen by scientists" (p. 278). The methodology of behaviorism, as practised by communities of scientists, is particularly well suited to achieving these goals, Zuriff argues (p. 278). By contrast, "other communities, not possessing equally effective methodologies, will not survive as well in this cultural form of competition and natural selection" (p. 278).

I am totally at a loss to understand how anyone in the second half of the twentieth century could conclude a book on this note.

Some forms of control are desirable. If in the hospital where I work, a phsyician succeeds in controlling a patient's dangerously high blood pressure, everyone (the patient included) is happy. But Zuriff knows as well as I do that we live in a century in which regime after regime has attempted to control the behavior, thoughts, and feelings of anyone they could lay their hands on. Radical behaviorists have sometimes argued that kindness (= positive reinforcement) is a more effective means of control than rubber truncheons (= negative reinforcement). This may or may not be true, but the moral issue remains: Under what circumstances can the exercise of control over other human beings be justified?

Zuriff mentions the traditional position that "the question of values" may lie "outside science" (p. 277). If this were so, would the philosophy of behaviorism be refuted? If values are not outside science, is behaviorism thereby committed to the ethics of Auschwitz?

Is it too much to hope that scientists will adopt as one of their central values that it is important to *understand* the natural world? And that questions of control should only be raised within the framework of belief in the autonomy and dignity of living creatures? Hillel taught: Do not do to others what you would not have them do to you. That is the whole law; the rest is commentary.

Neglect of psychology's silent majority makes a molehill out of a mountain: There is more to behaviorism than Hull and Skinner

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Behaviorally inclined psychologists have long been divided on the question of how much emphasis should be placed on behaviorism's method and how much on behaviorism's theories. John Watson himself vacillated between a moderate methodological behaviorism – in which the necessity of behavioral observations was pivotal – and a more radical or metaphysical behaviorism – in which only behavioral concepts were permitted free theoretical rein.

Zuriff's book resolves this issue implicitly, by almost entirely neglecting methodological behaviorism and concentrating on a "family" of behavioristic theories, mainly those of Hull and Skinner. In light of Zuriff's statement that "what is needed . . . is an accurate portrait of behaviorism and an honest search for what is still valuable in it" (p. 278), it is ironic that he misses so golden an opportunity to point out its foundational methodological advantages over competing perspectives. The essential arguments are in the book, but they are not appropriately marshalled; the necessity of behavioral observations is seen only as a means to behavioristic theory, when in fact a very strong case can be made that behavioral observations must underlie any type of psychological theory.

Zuriff's essentially theoretical approach to behaviorism is clearly indicated in the opening sentences of Chapter 1: "Behaviorism . . . the conceptual framework underlying a particular science of behavior. . . . consists of a philosophy of science, a philosophy of mind, an empirical background theory, and an ideology" (p. 1). Four components – but no method. For many, perhaps most psychologists, it is the method that is the solid rock on which the enduring value of behaviorism rests.

Zuriff does acknowledge, more or less in passing, that empirical results can have value independent of their theoretical interpretation. But the full implications of this most important point are not adequately developed. The primary thrust of Zuriff's pro and con arguments concerns the adequacy of behavioristic theoretical interpretations.

Why is the distinction between data and interpretation so crucial a point? Mainly, I think, because there is always more room for differences of opinion on interpretations than on "facts." Experimental psychology has plenty of problems – and theories. What is needed is a more solid factual basis for development of theory. The future of psychology as a science depends on how effectively it can use behavioral methodology rather than on either the philosophical/ideological underpinnings or the finer details of the behavioristic shape of its theories. Overconcern with the latter at the expense of the former can only serve to retard scientific progress.

Zuriff's treatment of behavioristic theory raises some interesting questions, which will not be answered in my opinion by the essentially philosophical concerns of his final chapter on "behavioral epistemology." The central question that I see is whether our investigations are better guided by restrictions on empirical methodology or on interpretations. As Skinner has consistently argued, the immediate products of our empirical work – our data – will themselves provide sufficient restrictions on their interpretation. Much of Zuriff's concern for exacting guidelines for proper behavioristic theory is premature, because by and large our most immediate need is to devote more energy to smaller-scale behavioral interpretations that can be more meaningfully tested.

Having said all this, I hasten to add that Zuriff has succeeded admirably in achieving his *own* primary objective. Perhaps he thought that his "conceptual reconstruction" of behaviorism required putting the theoretical cart before the empirical horse. Certainly the book is timely. Under strong attack from cognitivists and disenchanted ex-behaviorists of various types, this once mighty champion of objectivity and empiricality in psychology has been reeling from criticisms from all sides. Zuriff's book offers an extremely well-balanced rebuttal. He is fair – almost to a fault. In reading especially the first third of the book, I often wished that he would come to a quicker decision. Nevertheless, the book is a most important – indeed, a unique – contribution.

One illustration must suffice to suggest the provocative character of Zuriff's many analyses of theoretical problems. In considering the Chomsky–Skinner confrontation on the nature of language, Zuriff points out that Chomsky's structural principles are in fact derived from behavior; but it is the poorly understood and more complex behavior of the linguist that underlies them, "not that of the everyday speaker" (p. 145).

In summary, I feel that this is an extraordinarily valuable book but one whose message is in danger of being seriously misdirected. If Zuriff had paid more than lip service to empiricality and objectivity, and emphasized the necessity of behavioral methodology quite apart from particular behavioristic theories, or even families of such theories, the book would be both a more faithful portrayal of psychological science and a more attractive beacon to a new generation of young behaviorists.

Temporal molarity in behavior

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Zuriff has done a great service to psychology and philosophy. He has shown that the standard philosophical dismissals of behaviorism are based on a narrow and inaccurate understanding of what behaviorists have said and done. Zuriff needs no help in defending behaviorism from antibehaviorist critics. His book is well fortified against attacks from that direction.

My comments here have to do with a more fundamental object of the book: to characterize behaviorism as a conceptual framewoek. The family portrait Zuriff draws does not fairly represent molar behaviorism, the branch that seems to me to be the best conceptual tool for prediction and control of human behavior.

For Zuriff, molar behaviorism is a belief that the behavior of whole functioning people may be studied on its own terms without analysis into components. This is the "personal level" theory that Dennett (1978, p. 154) dismisses in a footnote as "not a psychological theory." But there is another sense in which the molarity of behaviorism may be understood. In addition to spatial molarity (whole persons interacting with their environments rather than a person's parts interacting with each other) there is temporal molarity – irreducible patterns of behavior defined over long (or at least not instantaneous) durations.

Skinner broke away from traditional S–R theory, not only in defining a response by its consequences (as an emitted operant) rather than by its antecedents (as an elicited reflex), but also by insisting that the fundamental measure of emitted responding is *rate* rather than *intensity* or *latency*. Response rate is a temporally molar concept; it has no meaning at an instant. At an instant, a rat is either responding or not responding; rate of response, *as behavior* has meaning only over an extended temporal interval much as amount of precipitation has meaning only over a specific period (a dry year may have several wet months, a wet month may have several dry days, etc.). Even Skinner's preferred measuring instrument, the cumulative recorder, implies a certain period of integration through the paper speed and step size needed to produce "smooth curves." Zuriff recognizes this temporal molarity in his discussion of the technicalities of operant conditioning (pp. 106–8, sure to be skipped over by philosophers) but not in the more philosophical sections of the book. Perhaps this is because Skinner himself has ignored extensions of his own conceptions of temporal molarity. One such extension was Herrnstein's (1969) argument that, in avoidance conditioning, along with response rate as the fundamental *dependent* variable, rate of aversive stimulation is the fundamental *independent* variable (a fundamental variable being defined as one at the focus of prediction and control). This, together with Herrnstein's (1970) reformulation of the law of effect (in terms of relative rates of response and reinforcement) and Premack's (1971) relativistic conceptions of reinforcement and punishment, forms the basis for molar behaviorism as it is currently practiced (Baum 1973, Catania 1971; Herrnstein 1970; Nevin 1969; Rachlin, Battalio, Kagel & Green 1981; Timberlake & Allison 1974; Staddon 1979; and others).

The issues involved are not confined to the operant laboratory. They call into question Skinner's (1948a) notion of "superstition," by which he explains much seemingly irrational human behavior; they call into question his concept of private behavior (internal operants), by which he explains sensations, emotions, and other everyday mentalistic terms; and, perhaps most important, they raise questions about his view (Skinner 1948b) of an ideal society, free from aversive control. (If Premack is right that punishment and positive reinforcement are two ways of looking at a single process, then no control is possible without aversive stimulation; a rat avoiding electric shock would be no worse off than a rat avoiding starvation; if starvation may be made less severe and still be effective, so may electric shock.)

In its interpretation of mental terms, molar behaviorism concentrates on the prediction and control of behavior. If classical and operant conditioning (conceived as molar processes) are the best available methods to predict and control molar behavior, mental events that can be described in molar behavioral terms will thereby be susceptible to prediction and control. In an article in this journal (Rachlin 1985) I attempted to show how "pain," a term generally considered to be an irreducible, private "raw feel," might be interpreted as molar behavior. To the extent that its interpretations of "pain" and other mental terms are successful, molar behaviorism is a valid theory of the mind.

In its interpretation of mental terms, molar behaviorism is a subcategory of logical behaviorism as discussed by Zuriff (p. 204–5): "The meaning of 'Mr. Jones is angry' is given by the observable conditions by which one tests to determine if Jones is angry. . . . For example, the conditions for the application of 'angry' can be formulated with terms such as 'pounds the table,' 'turns red in the face,' and 'screams "I hate you"'." Zuriff, in his role of referee, then states, "An obvious problem for this analysis is that Jones may be angry but may not be manifesting any of the behavioral symptoms because he is, for example, suppressing them." Later in the chapter, Zuriff considers several behaviorist responses to this objection, including the postulation by behaviorists of brain states, hypothetical constructs, intervening variables, and behavioral dispositions. Molar behaviorists, however, need none of these postulations, because the original objection does not apply to their theory. For a molar behaviorist, the conditions for the application of "angry" are not only present conditions but past (and future) conditions also. It may be objected that a person may be angry, suppress his anger, and die before revealing it. But to ask whether a person can be angry and *never* show it (whether a person can be in pain, in love, have an image, an expectation, an intention, a sensation, perception, cognition, dream, etc., and *never* show it) is a pragmatically meaningless question like whether one of William James's (1907/1975) two squirrels (chasing each other around a tree trunk) is going around the other. Molar behaviorism views present anger as a temporally extended pattern of behavior, a pattern that encompasses the present moment. The difference between anger expressed and anger suppressed is in that overall pattern. Suppressed anger contains a (limited) period during which component responses such as pounding the table occur at a reduced or zero rate (just as a rat's suppression of lever pressing must consist of at least two distinguishable – perhaps temporally distant – intervals with lever-pressing rate suppressed in one interval *relative* to the other, while certain other conditions prevail across both intervals).

The pragmatic meaninglessness of many supposedly deep philosophical questions about mental terms may be made apparent if we apply those questions to something that we all agree is a temporally extended pattern of behavior – the playing of a Beethoven piano sonata, for example. If three notes are played and then the pianist dies, was he playing the sonata or not? What if he has played half of the notes? every other note? everything but the last note? What if he adds on an extra note at the end? What if, in the middle, he switches and starts playing chopsticks? How we answer these questions depends on our definition of what it means to play a sonata. Correspondingly, a person whose behavior bears elements of the pattern most of us call anger is actually angry, feigning anger, not *really* angry, but only thinks he's angry (as mothers often say of children), in conflict about his anger, and so on, depending on how we define anger (just as one squirrel is going around the other depending on how we define "going around"). Our definition of "anger," like our definition of "playing a piano sonata," depends, in turn, not on the existence or absence of an unseen internal state but on the effectiveness of the behavioral pattern.

(To go still further: whether an angry person is a *person* and not a robot acting angry, depends, not on whether his insides are bones, nerves, flesh and blood rather than steel rods, wires, foam rubber, and gasoline, but on whether his behavior over long periods shows the functional characteristics of people or machines. If a person has been your friend since childhood, helped you, comforted you, entertained you, through both of your lives – say he dies at 90 and you live on – what difference does it make what his autopsy might reveal about the composition, the structure, *or* the internal functioning of his parts?)

Zuriff asserts (p. 206) that operational definitions retain no "causal force." A *molecular* operational definition of "anger" as "galvanic skin response" (Zuriff's example), or of playing a certain Beethoven sonata as playing two or three notes, indeed has no causal force. But if our definitions of "anger" and "playing a sonata" required a closer approximation to the complete pattern, its causal force would be evident. Only a relatively intact pattern of anger will affect the behavior of other people (scare them, for instance), and only a relatively intact pattern of piano playing will bring forth applause (payment, etc.) for the piano player. Intact patterns may not have strong causal effects every time, but it is those effects that (a) keep the patterns intact and (b) justify our operational definitions.

This interpretation of mental terms (as overt temporally extended behavior patterns) is similar to, but not identical with, the dispositional interpretation. A disposition says what *would* happen if some conditions *were* to occur and is open to the objection of Geach (1957) that mental terms seem to refer to something that *is* occurring currently. A pattern of behavior *is* occurring when a person is observed (by himself or others) to be angry – even though, at this instant, not pounding the table.

Technically, perhaps, a temporally extended pattern of overt behavior is a hypothetical construct, because it is (a) often inferred (from observation of a brief sample) and (b) potentially directly observable. But, unlike the hypothetical constructs of both molecular behaviorism and cognitive psychology, what is being inferred is more overt behavior, not covert events or events in another "sphere." However it is categorized, this form of behaviorism, in my opinion particularly resistant to the usual objections of cognitive psychologists and philosophers, is hardly acknowledged by Zuriff. It is a mark of his ability that he nevertheless successfully defends behaviorism.

Average behaviorism is unedifying

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It is a monumental compilation that Zuriff has delivered unto us. Never before has so densely representative a sampling of views by so many behaviorists over so broad a range of our indigenous philosophy-of-psychology concerns been collated in one document. By all rights, this should be the definitive sourcebook for exhibiting the vision, audacity, and zeal of what behaviorism once was and could yet again become.

It grieves me, therefore, to observe that the ambitious experiment of Zuriff's compositing technique is at best only modestly successful. For inasmuch as the behaviorist literature has never been an exception to Sturgeon's Law,[1] pooling abstract position summaries across multiple sources, even when these are selected for similarity, can only degrade peaks of insight and choice delicacies of conception in a regression to mediocrity. Despite the scattering of Zuriff's own astuate comments, the prevailing result here is a mush wherein classic slogans retain their rote verbal familiarity even while their meanings, too, remain as elusive/incoherent as was unhappily their norm. I shall work through one example that has more importance than Zuriff's review allows one to appreciate.

Following a suggestion by Feigl in the 1945 APA Symposium on Operationism, a recurrent behaviorist argument for interposing an "intervening variable" Z between m independent data variables $X_1, \ldots, X_m$ and r response variables $Y_1, \ldots, Y_n$ has been that when this can be done tidily it reduces the $m \times n$ pairwise input/output relations $\{X_i \rightarrow Y_j\}$ ($i = 1, \ldots, m; j = 1, \ldots, n$) to $m + n$ relations $\{X_i \rightarrow Z\}$ and $\{Z \rightarrow Y_j\}$. On pp. 64–66, Zuriff recapitulates this line of reasoning with a minor twist of his own (namely, inferring one of the supposed empirical laws from three others) without pointing out (a) that this bivariate-lawfulness model is inchoate, and (b) that under a more adequate conception of simultaneous multivariate dependencies, the intervening variable is seen to have a very different ontological status from what Zuriff, speaking for the past norm, assigns to it. (Zuriff himself recognizes fragments of the more sophisticated story when on p. 66 he cites disposition-manifesting "circumstances"; but by clinging to the received model's inappropriate formalisms he garbles the account's proper punchline.)

Letting $m = n = 2$, suppose that X_1, X_2, Y_1, Y_2 are certain measures of water deprivation, dietary-salt concentration, lever-pressing rate, and ad-libitum water intake, respectively, as suggested by Zuriff. How the first two of these affect the last two in living organisms is severely conditional on many other causal antecedents, some of which (e.g., past training, species, and maturation) we can control by manipulation or selection, whereas others remain unknown. But even with all relevant background factors held constant, there is no tight dependency of lever pressing or water intake on water deprivation alone and another on dietary salt alone. Rather, there is one determination of lever pressing, and another of water intake, *jointly* by water deprivation, dietary salt and the relevant background factors; and the environment in which outputs on Y_1 and Y_2 are observed – call this variable "stimulus setting" S – also makes an enormous output difference. Thus the absence of the lever or of water necessitates zero lever pressing or water consumption, respectively; whereas the presence of both the lever and water will elicit extensive interference between these two response variables (strongly conditional on how that water is distributed) in blatant contradiction with Zuriff's equation (11) on p. 68. The proper empirical model, idealized as customary with linear residuals, is

$$Y_1 = \phi_1(X_1,X_2,S,B) + e_1\ ,\ Y_2 = \phi_2(X_1,X_2,S,B) + e_2\ , \quad \text{(1a,b)}$$

where B comprises all relevant background factors we have managed to identify and e_1, e_2 are residuals reflecting additional unkonwn behavior sources. It is important to be clear that the laws schematized in (1) are able to govern a common domain comprising all subjects of some broad kind (say, all living mammals) at all times in any environment, for example, regardless of whether levers or water dispensers are present, because they include stimulus setting as input variance.[2] How much we can learn about functions ϕ_1 and ϕ_2 in (1) empirically when in practice we can sample them under just a few of the copiously diversified alternatives on S needn't concern us here.

If the details of functions ϕ_1 and ϕ_2 are suitably cooperative, we may now find that equations (1a) and (1b) cry for explanation in terms of a hypothesized intervening variable Z – heuristically call this "thirst" without, however, presuming anything about its relation to mentalistic appetitive experience – such that lever pressing and water intake are each due jointly to thirst, stimulus setting, and background factors B, but not to water deprivation or dietary salt through any causal route unmediated by thirst, under certain laws

$$Y_1 = \psi_1\,(Z,S,B) + e_1'\ ,\ Y_2 = \psi_2\,(Z,S,B) + e_2'\ ; \quad (2)$$

while thirst, in turn, is caused jointly by water deprivation, dietary salt, and background factors B with indifference to stimulus setting, in accord with some law

$$Z = \psi_0\,(X_1,X_2,B) + e_0\ . \quad (3)$$

Space restrictions prevent my detailing how we achieve inference to (2,3) in practice, except to hint that it involves our being able to predict from what subjects do on levers in press-permissive environments to the water intake of those same subjects, under unchanged values of X_1 and X_2, in S-settings that allow unrestricted drinking. (A procedure we do *not* follow here is to write down equations (1a) and (1b) and observe that their right-hand sides both embed the right-hand side of (3). Not only is that impractical, but (2) and (3) do not even reproduce (1) exactly unless the functions in (1)–(3) are linear.) We can infer (2) and (3), however, at least for selected values of S and B, albeit, as in statistical reasoning, the inference is fallibly ampliative.

The crucial point here is that "intervening variables" in cases like this are not *invented*, as explicitly defined abstractions from data measures, to simplify empirical equations. Rather, we *discover* them by a logic of explanatory induction (see Rozeboom 1972) that with imperfect reliability but often overwhelming conviction discloses to us the hidden sources of intercorrelated observed phenomena. When interpreting real data, working behaviorists have made such inductions intuitively, with neither supervision by a received metatheory of their logical forms nor sufficient expertise in technical philosophy-of-science to explicate that on their own. Indeed, not until rather late in the behaviorist game did its more thoughtful partisans begin to recognize that their cherished "operationally" defined concepts were no different in kind from more conspicuously theoretical terms given meaning/referents by their nomological-network roles (see Rozeboom, 1984, and additional references cited there). Meanwhile, the mid-century bivariate-lawfulness account of intervening variables so misleadingly travesties the logic of theoretic discovery that to endorse this without significant upgrading is to portray seminal issues at the cutting edge of advanced epistemic engineering as empty symbol bashing.[3]

Technicalities of multivariate lawfulness aside, Zuriff's regression-to-mediocrity emphasizing of typical past slogans on intervening variables and operational definitions regrettably reinforces the tediously repeated slander by hostile outsiders that behaviorism was dedicated to positivistic rejection of the inner organism. It cannot be denied that a few influential behaviorists, notably Skinner (e.g., 1950), the early Spence under Bergmann's tutelage (e.g., Bergmann, & Spence 1941), and H. Kendler (e.g., 1952), ardently proscribed conjectures about internal mediators. And it is also true that MacCorquodale & Meehl (1948), in their brilliantly definitive paper on this matter, unwisely used the label "intervening variable" to distinguish logical abstractions on observables from covert factors hypothesized to explain data regularities. But both Hull and before him Tolman, who introduced the notion, were emphatically clear that *their* intervening variables were hypothesized causal mediators (see, e.g., Hull 1943; Tolman 1936). (Zuriff recognizes this, but buries the acknowledgment in a footnote (Chapter 4, fn. 33 & 53) when its rightful place is in his text's foreground.) And although many mid-century behaviorists would have found congenial Zuriff's normative characterization of intervening variables as "summaries" of input/output correlations that are "generally conceived of as having no causal status" (p. 207), that is mainly because no one had yet made clear how the ontology of an explanatory induction's conclusion transcends the data patterning which impels its inference.

The aim of all behavioristic approaches to psychological science, the essential unity behind the splendorous diversity of specifics so amply documented by Zuriff, is not to avert attention from covert sources of overt behavior (even Skinnerians grudgingly theorize) but to insist that *scientific* conclusions about these require tough-minded epistemic warrants – hard evidence, if you like – that free-spirited theory spinners and fantasizers in folk psychology consider unbearably spoilsport.[4] Behaviorism has no yen for empty organisms; rather, it recognizes the enormous gulf between our desire to comprehend the innerness of subjects and the modest reach of our commonsense ability to attain such knowledge; and it accepts responsibility for engineering reductions of this gap to whatever extent current technical epistemic competences make possible. It is precisely because our reach of credible understanding is *not* positivistically confined to observables, but has potentially unlimited scope if properly disciplined, that the behaviorist outlook is so important for continuing psychology; and this is why its repudiation by the current cognitive zeitgeist is such a scientific disaster. (There is nothing wrong with targeting mentality for study; it's *how* this study is pursued that makes all the difference.) Despite his evident goodwill, Zuriff has done us a considerable disservice by exhibiting behaviorism mainly as a midden of past metatheoretical muddles. It would have been far more beneficial to make clear that our profession's need for the behaviorist *program* – never mind the polemical excesses and generally limited, though far from insignificant, achievements of its early implementation – is more urgent than ever.

NOTES

1. Ace science-fiction writer Ted Sturgeon's legendary retort to the accusation that 90% of science fiction is crap was "Hell, 90% of *everything* is crap." I have never encountered an observation on the Human Condition that generalizes so robustly.

2. In contrast, when the import of Zuriff's "defining-experiment" rider on his p. 64f. equations is explicated, it can be seen that his empirical laws (1), (4), (6), and (9) have narrow disjoint domains that preclude any one of these being entailed by the others.

3. If you have been indoctrinated by Popperian philosophy-of-science, you probably don't believe that any epistemically significant "logic of discovery" exists. But Popper was simply wrong in this; and cogent theorizing is something about which practicing scientists cannot afford to be romantically naïve.

4. Savor this passage from Hull (1943, p. 23): "Driesch's entelechy fails as a logical construct or intervening variable *not because it is not directly observable* [my italics] . . . but because [its] general functional relationship[s to its observable causes and effects] are *both* left unspecified. This, of course, is but another way of saying that the entelechy and all similar constructs are essentially metaphysical in nature. As such they have no place in science. *Science has no use for unverifiable hypotheses* [Hull's italics]." Hull's understanding of what it takes for a conjectured mediator to have strong empirical support is somewhat ingenuous; but his intuition correctly shouts at him that this must consist somehow in the mediator's having nonarbitrarily theorized connections to hard data that pin it down as the Lilliputians did Gulliver.

The gentrification of behaviorism

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Can an edifice reconstructed from the conceptual detritus of three-quarters of a century, no matter how clever the restoration, produce a sturdy structure? The problem facing Zuriff is that in the reconstruction of a position out of the accumulated arguments for behaviorism from 1910 onward, the resulting structure necessarily has a certain dated quality about it. Here a pediment from Guthrie, there a cornice from Hull, the entry by Watson – but does the whole thing hang together with any kind of style? Indeed it does, and it strikes me as Iowa City gothic, circa 1953. (See Estes, Koch, MacCorquodale, Meehl, Mueller, Schoenfeld & Verplanck 1954, for the general tenor of the times; Bergmann, 1953, for the particulars.) True enough, the upstairs hall may be carpeted in a modern weave, but the contemporary touches are insufficient to overcome the somewhat dowdy visage of the whole design.

Behaviorism originated as a (primarily methodological) reaction to the failure of introspection in human psychology on the one hand, and the failures of anecdotalism and anthropomorphism in animal psychology on the other. The heavy concerns with objectivism, empiricism, externalism, operationism, intersubjectivity, verifiability, data language, prediction, and control that lace Zuriff's reconstruction provided the means through which behaviorism attempted to defeat the old way of doing psychology, and to establish a new foundation for a science of behavior. But something went awry in the 80 years since the project got underway. Specifically, (a) introspectionism, anecdotalism, and anthropomorphism no longer constitute the competition in psychological science; and (b) behaviorism's accumulated complex of assumptions and presuppositions has taken on a life of its own, thus obscuring the fact that such a metatheory is not required to justify an interest in the adaptation of the organism to its environment as the psychologist's subject matter.

I will leave it up to the cognitive psychologists to comment on the adequacy of Zuriff's reconstruction as a challenge to the new competition in psychological science, and address instead the second point. As Zuriff makes clear, behavioristic theory blends empirical with conceptual conjectures and assumptions. The relationship between the two is often more apparent than real. For example, the essentially conceptual position that Zuriff calls "externalism" is at most *consistent* with certain behavioristic empirical theories such as peripheralism or S–R mediational chaining, but it neither entails these questionable theories nor is it falsified when such theories are found to be inadequate. Furthermore, the totality of behavioristic assumptions and presuppositions goes considerably beyond what is required to motivate a science of behavior of at least roughly the sort currently being pursued by practicing behaviorists. In conseuqence, it is not clear that behaviorism *needs* a conceptual reconstruction of the sort developed by Zuriff; instead, perhaps it just needs a good housecleaning.

Indeed, one would think the only assumptions necessary to motivate a science of behavior would be rather simple – something on the order of "because it is there" and "because it is important." After all, such assumptions suffice in most areas of science. But perhaps this is too jejune; it is precisely because the majority of psychologists have decided that a science of behavior lacks its own subject matter that behaviorism is so roundly rejected by the mainstream. That is, the mainstream view seems not to be that behaviorism is struggling, somewhat unsuccessfully at the moment, with immensely important problems that it alone uniquely addresses, but that behaviorism is an antiquated general theory, now replaced among the enlightened by cognitivism. At least in its pure form, however, cognitivism does not fight with behaviorism for dominion over a common subject matter. Cognitivism is about the way the mind works, whereas behaviorism is about the manner in which organisms adapt to their worlds. The import of Zuriff's book is that behavioristic metatheory is what defines the subject matter of behaviorism. What it fails to do is to make a decisive case that the adaptation of organisms to environment forms a subject matter in its own right, regardless of the particulars of behavioristic metatheory. We know immensely more today about the intricacies of environmental constraints and the nature of adaptive processes as organisms attempt to cope with those constraints than was known 30 or 50 or 70 years ago. Those phenomena in and of themselves make the most persuasive case for a science of behavior.

The issue becomes critical when one realizes that, over the past 15 or so years, mentalistic psychologists and philosophers have discovered the importance of "context": the circumstances within which some event of interest occurs. In this respect, behaviorism now *is* in the process of losing the exclusivity of its subject matter. For example, the psychology of language has largely freed itself from the hegemony of Chomskyism, and becomes increasingly pragmatic. Much of this new work ought to be absolutely fascinating to behaviorists. (Compare, e.g., Carroll, 1986, with Fodor, Bever, and Garrett, 1974, for the difference a dozen years can make.) Currently, however, most behaviorists interested in language seem capable of doing little more than continually rehashing Skinner's *Verbal Behavior* (1957). It is my conjecture that this is neither because Skinner said everything worth saying about verbal behavior, nor because language is a substantially context-free phenomenon (the pragmatists – see Levinson, 1983, for a survey – have established sufficient reason to lay such neo-Cartesian pronouncements to rest), but because behaviorists are so conceptually bound by the very edifice that Zuriff has reconstructed that they cannot fight through it to get a fresh look at their nautral subject matter.

Despite thee heavy reservations, there is much to admire in Zuriff's book. An example of the best it has to offer is the section of Chapter 9 entitled "A Contextual Theory of Agency." This brief but subtle disentanglement of the problem of agency serves as a model for the promise a behavioral perspective (or should we say "contextual perspective?") offers to the significant conceptual problems of psychology. Unfortunately, however, it and similar passages tend to be so buried in reiterations of behaviorism's reactionary past as to lose their progressive impact. One can only imagine how much more powerful and persuasive a case for behaviorism might have been made if this reconstruction had not resurrected and refurbished every nitwit idea ever proposed by behaviorists.

The question before Zuriff's book ultimately comes down to the one faced by all renewal and gentrification projects: Now that reconstruction is complete and the slick brochure has gone to press, will anyone actually move back into the old neighborhood? Needless to say, I remain a pessimist.

"Suspicion," "fear," "contamination," "great dangers," and *behavioral* fictions

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Zuriff's most interesting and useful book will be judged, I should imagine, to be essential to an understanding of contemporary behaviorism, the strengths and weaknesses of which it directly reflects. One of the interesting uses of the book is to give the reader an exercise in the development of a kind of professional self-identity: It constantly encourages one to ask in what ways one is or is not a behaviorist. Such an exercise prompts me to consider the functions in behaviorism of three themes: behavioral theory, molar behaviorism, and animal cognition.

Zuriff's portrayal of Skinnerian behaviorism's metatheoretical position on theory is exemplified by his discussion of the dangers of "excessive theorizing," and by assertions such as "once the theory is rejected, the research becomes meaningless" (p. 88), and "it seems that the degree to which an observation is contaminated by theory varies" (p. 29). In my opinion, Zuriff's portrayal of this prototypical position on theory is an accurate one. He notes, however, that this position is not without its critics (Hanson 1958; Kuhn 1962; Rorty 1979). His response to these critics seems to me to jeopardize the coherence of his reconstruction of behaviorism. On the one hand, consistent with the idea that science is a cumulative enterprise, one reads that causal relations are observable (p. 177), that a functional approach does not contaminate the behavioral data language with theory (p. 29), that there are directly observable facts (p. 266), and that, more specifically, in a laboratory science of behavior the meanings of stimulus, response, and conditioning are fairly well defined (p. 220).

On the other hand, Zuriff also seems to agree with the contextualist position of Kuhn, Hanson, Rorty, and others, when he notes that there is no common opinion on what is meant by the behavior to be predicted or controlled (p. 93), that human knowledge has no claim to absolute validity (p. 258), that logic is a by-product of verbal behavior (p. 256), that the laws of nature are a human invention (p. 250), and that causal relations are not observable (p. 166). I do not see how Zuriff proposes to bring all these statements together in a coherent fashion. Nor do I find it particularly helpful that Zuriff's criterion for objectivity, a rather ill-defined notion of the extent of intersubjective agreement, is at the same time a contextualist criterion for the comparison of theories and a contextualist justification for the explanation of science in terms of political revolutions, if not exactly mob rule and violence (Kuhn 1962).

I would like to suggest that a way of dealing with this problem might be to abandon altogether the cumulative or additive growth picture of science, and instead to see whether a more coherent picture can be developed from contemporary cognitive research and theory on the nature of the development of concepts and of human knowledge in general. I have in mind especially work indicating that a cumulative or additive growth picture of the development of a concept is inadequate and that an interactive or multiplicative arrangement is required (Medin 1975; Medin & Smith 1984; Murphy & Medin 1985). This research seems to me to be a contemporary version of an earlier Gestalt psychological literature that originally contributed to the contextualist picture of science as developed by Wittgenstein (1953), Hanson (1958), Kuhn (1962) and others.

This contextualist position assigns a rather more central role to theory in the development of science and encourages a scientist to make as explicit as possible what his theoretical position is, so that it may more easily be evaluated and compared to other theoretical possibilities (Shimp 1984c). The contemporary theory to which Zuriff most frequently alludes, and which presumably best qualifies in his judgment as characterizing the theoretical achievements of a science of behavior, is molar behaviorism. It is not always clear to which of his two importantly different meanings of molar behaviorism he refers. His first meaning has a long history in behavioral psychology and is to be understood as referring to a nonreductionistic, nonmuscle-twitch behaviorism. As Zuriff notes, even what today is called a molecular analysis is in this sense molar. To distinguish this molar behaviorism from an important contemoprary special case, let us call it "big molar behaviorism," and the special case "little molar behaviorism." When he writes that behaviorism must "relinquish the search for immediate causes" (p. 265) and when he notes that it is permissible in contemporary behaviorism to leave unexplained temporal gaps in behavior streams, Zuriff refers to little molar behaviorism, perhaps the defining example of which is the generalized matching law. The distinction between big and little molar analyses is an important one to keep straight because the important special characteristics of the latter definitely do not apply to the former. In particular, molecular analyses, a branch of big molar behaviorism, do tend to try to explain the local temporal structure of behavior streams (Shimp 1984a; 1984b; 1984c), and in that sense they preserve a form of temporal contiguity and tend to reject unexplained temporal gaps. Similarly, when Zuriff writes that "The positivist emphasis on direct experience underlies molar behaviorism" (p.

250), he seems to be dealing only with little molar behaviorism. It is not entirely clear, at least to me, to what extent Zuriff intends his reconstruction of behaviorism to depend on these special features of little molar behaviorism.

Finally, researchers in the field of animal or comparative cognition (e.g., see Roitblat, Bever & Terrace 1984) will probably question the idea that "reinforcement is effective with lower animals to which the concepts of awareness or cognitive hypotheses do not clearly apply" (p. 191). Indeed, this idea about the scope of cognitive hypotheses can be reversed: In terms of a cognitive computational processing model such as AL (Associative Learner), operant conditioning itself, whether of animals or humans, is turned into a branch of cognitive psychology (Shimp 1984a; 1984b).

This example of a reversal of what is considered to be widely accepted and scientifically established illustrates what I consider to be the chief limitation of Zuriff's book and of contemporary behaviorism. The word "fear" occurs quite often in the book. Zuriff correctly describes behaviorists as fearing this and fearing that. But if one is to choose fear as a criterion for the evaluation of theory, then it seems to me one might as well reverse the usual fears and instead fear "operant," "conditioning," "behavior," and so on, as *behavioral* explanatory fictions.

Is it behaviorism?

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Behaviorism is, indeed, not a science of behavior but the philosophy of a science, I do not believe, however, that as such it "dictates canons concerning what sorts of psychological questions are worth pursuing and what methods are acceptable in searching for answers." Scientific methodologists reconstruct what might have happened in science; history usually tells a different story. The methods and facts of the science of behavior have not come from preconceived notions of a subject matter beyond the assumption that it is free of caprice. Samples of behavior have turned up from time to time, and scientists have searched for their causes. The search has sometimes been successful, and what has been discovered has contributed to a corpus of scientific facts.

My behaviorism differs from Zuriff's in several ways. It is "objective" in regarding a behaving organism as nothing more than a biological system, but not in the sense of ruling out introspection. It deals with introspection as a form of perceptual behavior. Because of defects in the contingencies that bring it under the control of private stimuli, however, little use can be made of what is thus observed, as Zuriff points out in Chapter 2.

Behavior and physiology are not two ways of approaching the same subject. In a given episode the environment acts upon the organism, something happens inside, the organism then acts upon the environment, and certain consequences follow. The first, third, and fourth of these events is the field of a science of behavior, which undertakes to discover how they are related to each other. What happens inside is another part of the story. It is studied with different instruments and methods. Psychophysiology does not tell us "what really happens when people think and have feelings."

I have never excluded from the behavioral data language any references to "action language, the intentional mode, purposive terms, or molar categories." On the contrary, I have argued that operant behavior is the field of purpose, intention, and expectation. It deals with that field precisely as the theory of evolution dealt with another kind of purpose.

I am particularly disturbed by Zuriff's close association of behaviorism with S–R psychology. An early emphasis on stimulus and response was encouraged by Pavlov's work and that of Clark Hull and his school, but the formula was largely restricted to responses involving glands and smooth muscles and was soon abandoned. Almost all the behavior that acts upon the environment is operant. The environment does not trigger it, it selects it through contingencies of reinforcement. Zuriff pays only scant attention to operant conditioning as a mode of selection.

Behaviorism does, of course, "exorcise" an agent as an initiator of behavior, just as evolutionary theory "exorcised" another kind of creator, but a behavioral analysis does not dispense with the concept of self. It defines self as a behavioral repertoire that results from a particular set of contingencies of reinforcement. Most people have a great many selves in that sense. They are different persons when they are with their families, with their business associates, with their friends in the locker room, when they are very tired or ill, and so on. The dramatic selves observed as multiple personalities interact with each other in unusual ways. The id, ego, and superego of Freud are selves traceable, respectively, to natural selection, contingencies of reinforcement in the immediate environment, and contingencies maintained by the ethical group. Important selves that have been interpreted in the light of an experimental analysis include the observing and observed selves in introspection and the managing and managed selves in problem solving and other kinds of thinking.

Author's Response

Conceptual reconstruction: A reconstruction

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The publication of *Behaviorism: A conceptual reconstruction* (henceforth *Reconstruction*) and this *BBS* multiple review seem to have occasioned a momentary deflection of attention from particular behavioral theories and research programs to sober reflection about behaviorism as a movement in modern psychology. Unexpectedly, instead of the usual polemics normally associated with discussions of behaviorism, the commentaries, even those most supportive of behaviorism, reflect a wistful, almost melancholic, spirit. Although many share my contention that behaviorism, in a modernized sophisticated form, retains its validity and significance, they recognize that behaviorism is declining in popularity and influence.

This spirit appears most clearly in the commentaries of **Marr** and **Dinsmoor**. Dinsmoor reconstructs the historical development of behaviorism in a way that emphasizes the validity of behaviorism while at the same time explaining its decline. Marr's illuminating insight into the relationship between a behavioral epistemology and the retreat from foundationalism suggests that the current decline of behaviorism is somewhat paradoxical. One important consequence of his insight is that behaviorist psychology is no worse off than the rest of science in its "bootstrap" character. Another consequence is that it is no better off either. If so, we must answer Marr's final question negatively and say, along with Wittgenstein, that the chain of reasons must come to an end, and then we simply act because that is our nature.

Conceptual reconstruction: Defense. The most frequent criticism in the commentaries, interestingly, is an objection not to the tenets of behaviorism but rather to my procedure of conceptual reconstruction. **Schnaitter, Marx, Hineline, Epstein,** and **Kendler** are all displeased that I reconstruct a behaviorism characterized by an elaborate conceptual scheme with positions on a variety of issues ranging from introspection to epistemology. They claim that a science of behavior, or "praxics" as Epstein labels it, is possible and desirable without this complex conceptual framework. Why not a simple science of behavior unencumbered by all the assumptions, prescriptions, and proscriptions of traditional behaviorism?

The simplest answer to this criticism is that it is not my goal to establish this "simple science of behavior." In Chapter 1, I state quite clearly that it is my purpose to develop a conceptual framework for the science of behavior that is both sound and true to the history of behaviorism. Obviously, powerful conceptual frameworks different from my own are possible and available. However, I explicitly declined to engage in what I term "*de novo* philosophizing," that is, developing a philosophy of psychology without regard to the past 70 years of behaviorist thinking. Because it was my stated goal to analyze and reconstruct behaviorist positions on nearly every conceptual issue of importance to behaviorists, I cannot be faulted for having done so. On the other hand, the worth of my endeavor, no matter how well implemented, can certainly be questioned, and it is to this that I now turn.

A major assumption underlying this question is that a science of behavior is possible without an elaborate metatheory. **Marx,** for example, believes we can have a methodology and "facts" without a conceptual framework. If these critics mean simply that one can do behavioral research without worrying about philosophy of science, then they are, of course, right. But if they mean that a behavioral research program needs no justification, or that "facts" are independent of a conceptual framework, or that there is no conceptual framework implicit in a research program, then I must disagree.

Behavioral "facts" are not like flowers, there for the picking. On the contrary, in transforming an observation into a data report, or "fact," we must, implicitly or explicitly, define the domain of behavior and select a descriptive language. If we are to have a science, something more than a disorganized mass of "facts" is required. We must systematize our facts with concepts, laws, principles, and even theories. Once we achieve this, we have, either explicitly or implicitly, made conceptual decisions concerning theoretical terms, explanation, and theory. Eventually, the "simple science of behavior" will have to confront the question of whether consciousness plays any role in the explanation of behavior and what to do about first-person reports of mental events. In short, I find it naïve to believe that a science of behavior, or praxics, can get very far without a conceptual framework, be it explicit or implicit.

This naïveté is widespread, with many contemporary behaviorists believing that all the major conceptual questions have been settled or are irrelevant. This is especially true among behaviorists who have adopted a standard methodology (e.g., the study of pigeons pecking keys) in which fundamental questions such as the criteria for the behavioral data language, the definition of "behavior," and the acceptability of theoretical terms can be safely ignored because the answers are implicit in the methodology. In truth, the basic conceptual issues are by no means resolved or irrelevant. The answers implicit in contemporary methodologies still need to be justified and defended against powerful objections and competing metatheories. Furthermore, many of these implicit answers are either inadequate or not clearly applicable outside the limited confines of the standard method.

Perhaps these critics prefer to keep all these conceptual decisions implicit so that they do not inhibit the intuitions of the scientist who remains unburdened by philosophy. In opposition to this, I argue on the Socratic principle, "know thyself," that scientists are better off knowing what they are doing. In any event, we need not worry: Those scientists who benefit from ignorance will not get past the first chapter of *Reconstruction* and will therefore not be harmed by increased self-knowledge.

Another assumption of the criticism under consideration is that the metatheories of the early behaviorists from whom I borrow are outdated and useless. **Schnaitter** speaks of the "detritus" and **Hineline** of the "rubble" of earlier conceptual thought. I agree that a conceptual framework can grow overly restrictive and stifle a science; I therefore explicitly address this question throughout *Reconstruction*. None of these critics, however, has identified a specific reconstructed position as outdated. I believe that they would encounter a great deal of difficulty invalidating any of the reconstructed framework. I also suspect that if they attempted to construct the minimum framework for their "simple science of behavior," free of everything they consider the unnecessary restrictions of behaviorism's past, they would discover quite a bit of disagreement among themselves. I even venture to predict that the debate among them will mirror controversies that have periodically recycled throughout behaviorism's history. Perhaps at that point someone trying to make sense of it all will write "Praxics: A Conceptual Reconstruction."

In one example of recycling, **Hineline**'s suggestion that we can solve certain problems about mind by changing the way we talk echoes behaviorist ploys going back at least fifty years. Although, as he notes, these gambits enable one to avoid appearing to accept certain assumptions, I prefer to analyze the assumptions and refute them if necessary rather than to reject them without a hearing. The latter practice may satisfy a committed behaviorist but will never persuade anyone who approaches the issues with an open mind. I doubt that I could have omitted my chapters on introspection, agency, and mentalism just by legislating how we are to speak.

On the other hand, Hineline and I fully agree that the behaviorist concern with prediction and control is too easily misunderstood as advocating a society controlled by behaviorists. **Marshall**'s comments are a good example of this misunderstanding and the emotions it stimulates. Nevertheless, I still maintain that the emphasis on prediction and control, properly understood, is fundamental to the framework of behaviorism. Behaviorist views on the behavioral data language, molar behaviorism, externalism, theorizing, and epistemology are predicated, in part, on the goals of prediction and control.

As for the S–R label, **Hineline** may well be right. After much thought, I decided to keep the term because of its historical and popular significance but to reconstruct a new liberalized definition which accurately captures behaviorism. If readers, including **Skinner** in his commentary, continue to confuse old notions of S–R, which I carefully show no longer apply to behaviorism, with my reconstructed concept of S–R, then I will indeed have committed a "strategic blunder."

Kendler offers a broad definition of behaviorism, with the advantage that it shows salient continuities between behaviorism and views generally considered antibehaviorist. A disadvantage is that anyone, cognitivist or psychoanalyst, who assumes behavior as the dependent variable for investigation must, counterintuitively, be regarded a behaviorist, regardless of their theoretical apparatus. I choose a much more restrictive definition and use the metaphor of family resemblance to exclude cognitivism from the behaviorist family while noting the continuities between behaviorist mediational theories and cognitive theories (Chapter 8). Definitions are not true or false; they are useful or not useful. Kendler and I choose definitions for different purposes.

Conceptual reconstruction: Elaboration. Having defended my reconstructive method, I shall use other commentaries as a forum for saying something positive about the process of conceptual reconstruction. A good place to begin is with **Baer**'s insightful inquiries. His first question is an empirical one. My guess is that different reconstructors will produce different reconstructions. My reconstruction is an inductive process – explicating the conceptual framework implicit in the work of many behaviorists over a seventy-year period. Because data always underdetermine inductive inferences, there is not only one true induction. I suspect that different reconstructors will differ from me in interpretation, emphasis, and selection. The criticisms of many of the commentators are good evidence for my hypothesis. Even within my own reconstruction, I show that there is usually more than one sound branch leading from a conceptual choice point node, and the choice of direction often depends on one's intuitions, purposes, values, and preferences. I therefore do not think that a consensus will be achieved (Baer's question 2). Similarly, because these same factors help determine one's criterion for "soundness," there is no one "soundness."

Since my conceptual reconstruction explicates what is for the most part implicit, it does not represent a set of verbalized rules and principles that function as discriminative stimuli for behaviorists. To be sure, much of the behavior of scientists is under the control of verbal stimuli in the form of rules and principles. Nevertheless, most of what the behavioral scientist does at the conceptual level, which is of concern to me, is shaped by contingencies of reinforcement maintained by teachers, thesis advisors, journal editors, and the data themselves. Such contingency-shaped behavior differs from rule-governed behavior. [See also Skinner: "An Operant Analysis of Problem-Solving" *BBS* 7(4) 1984.]

However, the contingencies as well as the behavior they control can be *described* by rules or principles. Hence, my reconstruction consists of an analysis of these descriptive rules and principles rather than a statement of verbal rules that function to control behavior (**Baer**'s question 3). I hope that by explicating sound rules and principles they will eventually come to function as discriminative stimuli controlling the behavior of future behaviorists (question 4). I assume that their behavior will differ from the behavior of other scientists controlled by a rival reconstruction – although because rules underdetermine behavior, this outcome is not a necessary one, as **Baer** notes (question 5).

Ironically, **Skinner** ignores his own distinction between rule-governed and contingency-shaped behavior in confusing a conceptual reconstruction with "preconceived notions." Throughout his distinguished career, Skinner has formulated rules stating that only certain psychological questions are worth pursuing and only certain methods are acceptable in searching for answers. For example, he has argued against research that uses the subject's age as the independent variable or that uses relative rate of response as the dependent variable. If the first point in his commentary is that these rules, or "canons," are derived *from* his research (contingency-shaped) rather than restrictions imposed *upon* his research a priori, then he has no disagreement with my reconstruction.

My own behavior in reconstructing behaviorism is mostly but not fully controlled by the practice of the behaviorists I have studied (**Baer**'s question 6). Some of the reconstruction represents my own contribution, the product of variables in my own personal history. The relationship between the resulting reconstruction, or others like it, and behaviorist practices is very complex, but I venture to say: (1) The relationship is interactive; (2) there is no behaviorist whose practices are functionally controlled by my reconstruction; (3) there may be no behaviorist whose practices are even described by my particular reconstruction. This does not mean, however, that the practices of behaviorists will never come under the control of a sound explicit reconstruction. If this does come about, I believe behaviorist practices will improve (question 7).

Baer's reconstruction for behavioral technology is as good an example of behaviorist pragmatism (Chapter 12) as one is likely to see. As he struggles in the naturally selective domain of applied behaviorism, I hope my Aristotelian reconstruction serves as a useful heuristic.

Krasner astutely uncovers two of the secrets of my conceptual reconstruction that I hoped would not be discovered. First, in Chapter 12 I distinguish between a formal epistemology, in which knowledge is regarded as divorced from its human context, and a behavioral epistemology, in which knowledge is seen as inextricably bound with the behavior of the knower. Although I identify the latter as the epistemology of behaviorism, *Reconstruction*, a contribution to behaviorist epistemology, is written from a formalist perspective. For this I apologize to any reader whose behaviorist sensibilities I offend.

Second, I identify ideology as one major component of behaviorism; yet, as **Krasner** notes, I say virtually nothing about behaviorist social values. One point in my defense is that I do thoroughly discuss certain values, although not the social values generally associated with an ideology. My interest is more in the values implicit in behaviorist science. These include: (1) the positive value behaviorists attribute to science, objectivity, prediction and control, pragmatism, and caution; (2) behaviorist standards for good scientific strategy, explanation, scientific goals, and understanding. These are values, although they are not often recognized as such. My second defense is more personal. Behaviorist views on social values do not form the same kind of family resemblance found in behaviorist philosophy of science and mind, and I do not find these views interesting or enlightening. More important, I am not willing to reconstruct and defend these views.

Substantive objections. Although many commentators criticize my reconstructive procedure, surprisingly few raise substantive objections to the final reconstructed product. **Hocutt, Branch, Graham,** and **Shimp** do note specific problems with behaviorism or my reconstructed version of it.

Hocutt (along with **Skinner** and C. S. Peirce) is right: Intersubjective agreement does not guarantee truth. In my reconstruction, intersubjective agreement is important, not so much for a definition of truth, but rather for the selection of terms for the behavioral data language and for theory. For these purposes, truth is not at issue but rather strategy. If intersubjective agreement is achieved for false beliefs (e.g., the earth is flat), the reinforcing value of consensus will be less than the reinforcing value of effectiveness. Presumably, a science with false beliefs will not be very effective for prediction and control and ultimately for survival (the final criterion for truth in behaviorist pragmatism, Chapter 12). Given the behavioral interpretations of mentalist concepts such as color and fear (Chapters 11 and 12), the truth of psychology and the truth of physics appear to be compatible.

Branch raises six challenging questions, most of which I have no expertise in answering. Nevertheless:

1. In Chapters 4 and 5 I try to show that behaviorist theories do guide research and organize data. For the operationist, experiments are designed to add to the partial definition of a concept. For Tolman, the intervening variable suggests defining experiments and facilitates inductive systematization. Hull's hypothetico-deductive method is intended to generate theorems representing possible experimental tests of a theory. Skinnerian theorizing is guided by the search for controlling variables, and data are organized by principles of effectiveness and "smoothness of curves." At the same time, I argue that neither theory nor method is a good algorithm for dictating research; at best they are heuristics. Decisions about research tactics are creative acts, underdetermined by the rules of "scientific method." This is true for cognitivism as well as behaviorism.

2. One of Hull's hypothetical constructs is the stimulus trace, which persists after the termination of the stimulus and has a decay function similar to that for iconic memory. Other solutions are left to the ingenuity of other behaviorists.

3. Although the public accompaniments are better correlated with the contingencies of reinforcement than the private stimuli, I suspect that the latter are considerably more salient to the child, who might not even notice some of the public cues. Remember also that the theory in question is only one version of how we might learn to talk about inner events.

4. Inferring past history from current repertoire is indeed routine practice in behavior therapy. Although it is useful and therefore justified in applied contexts, it is unsuited for science, which seeks general principles, not the inferred explanation of a particular instance. Only *after* an observed history is shown to be causally related to subsequent behavior can that kind of history be inferred when unavailable to observation in an applied setting. Skipping the first step and beginning with the inference is the objectionable practice.

5. Our everyday talk is filled with many kinds of explanations that are both useful and technically incorrect according to science. These folk explanations have their place, but they also have their limits. Without these useful fictions, life would be more inconvenient, but science demands more.

6. **Branch** suggests that my reconstructed behaviorism has a Skinnerian flavor, but **Skinner** and **Dinsmoor** disagree. **Kendler** sees me as a "radical" behaviorist, whereas **Schnaitter** locates me in Iowa City. I am gratified that my treatment of behaviorism is so balanced that these distinguished behaviorists cannot agree to which camp I belong. How one chooses one's camp is determined by more than just the soundness of a theory. According to a behavioral epistemology, it is determined by behavioral variables about which we currently have little understanding.

Graham vividly describes one of the reasons that several behaviorists over the decades have referred to the behaviorist science as the "psychology of the other one." Behaviorist psychology is necessarily a third-person psychology. At times we can adopt a third-person perspective on ourselves, such as when we examine the causes of our past actions. However, we cannot adopt this stance with respect to our current actions. This very behavior of discerning the variables of which our current behavior is a function is itself another determining variable. Therefore, a critical variable will always be missing in the simultaneous prediction of our own behavior: The effects of the behavior of predicting. The elusive self is thus always once removed from the behavior it seeks to explain.

Despite his preference for contextualism, **Shimp** insists on finding contradictions in *Reconstruction* by quoting out of context or else misstating (e.g., on p. 166 I did not say that causal relationships are not observable but that causal *necessity* is not observable). Far from rejecting contextualism, as Shimp claims, I make extensive use of this position and suggest (Chapter 5) that the behaviorist science can be regarded as normal science within its own scientific paradigm.

Shimp also misreads my references to molar behaviorism. In fact, all the statements he cites refer to what he terms "big molar behaviorism" not to "little molar behaviorism." He should also note that the molecular analyses he favors, although molecular in attempting to explain the local temporal structure of behavior, nevertheless leave temporal gaps. The time interval between the reinforcement of an interresponse time and its subsequent occurrence is not filled with the physiological mechanism responsible for the effect of the reinforcement.

You can't please all the people. In a work that attempts to cover all aspects of behaviorist metatheory, it is inevitable that some readers will judge that certain ideas or principles have not been given the attention they deserve. **Catania**, for one, feels that I do not give enough emphasis to the process of selection. He is right that selection is implicit throughout behaviorism and throughout my reconstruction of it. The adaptation of the organism to its environment is a central question in the science, the major criterion for validity in its epistemology (Chapter 12), and an underlying rationale for behaviorist extrapolation from simple to complex (Chapter 10). Darwinian concepts of evolution and natural selection profoundly affected the history of behaviorist thought, especially in its beginnings and now. I am sure that more could be said about the role of selection in behaviorism, but I shall leave that to others more qualified than I.

Eysenck also detects a lack of emphasis, and he is to be commended for reminding us once again that individual differences and genetic determination are important factors in understanding human behavior and that internal and external causes interact. Hull, for example, includes terms that represent individual and species differences in his equations representing the functional relationship between the environment and behavior (Chapter 5). As I note in Chapter 9, the genetically determined structure of the organism is assumed as an initial condition for the explanation of behavior as a function of environmental variables, given this structure. Where behaviorists may tend to differ with Eysenck is in his choice of traits, such as intelligence or criminality, as dependent variables, and personality structure as an independent variable (Chapter 6).

With his usual grace and wit, **Rozeboom** quibbles with me over the definition of "intervening variable." Adopting MacCorquodale and Meehl's (1948) definitions, I use "intervening variable" to refer to logical abstractions on observables. Certainly I did not "ardently proscribe conjectures about internal mediators." On the contrary, I thoroughly discuss hypothesized internal mediators under the label "hypothetical construct" (pp. 72–80), assuming the acceptance of such constructs throughout *Reconstruction* and stating: "In point of fact and contrary to the popular image, the majority of behaviorist theories include hypothetical constructs among their theoretical terms" (pp. 78–79). Whether Tolman's and Hull's theoretical terms are intervening variables under these definitions is a matter of historical debate, properly relegated to a footnote in a conceptual reconstruction.

My apologies to **Rachlin** for not including his version of molar behaviorism in my discussion of behavioral interpretations of mental concepts (Chapters 10 and 11). Indeed, his molar behaviorism has much to recommend it, and it avoids many of the usual objections against other behavioral interpretations. However, it is open to other objections: (1) Rachlin does not accept as meaningful that Jones may be angry and never show it. However, a behavioral interpretation must interpret mental concepts as they appear in everyday discourse, and in such discourse it is common to speak about having an emotion or thought without any characteristic overt behavior. (2) When Jones reports "I am angry" before showing any other anger behavior, what is the discriminative stimulus for this verbal response? (3) I may simply perceive a robin, count to ten in my head, or imagine a tree, without acting overtly in any special way attributable to these mental events. How does molar behaviorism handle such episodic mental events? (4) Jones may be said to be angry on Monday although he shows no overt anger until Wednesday. Does this large temporal gap not exceed even the temporal vagueness in dating molar events? (5) The "causal force" I discuss in Chapter 10 refers to the relationship between the mental event and the behavior. For example, we say that Jones's anger caused him to pound the table. However, on Rachlin's account, we cannot say this because the anger *is* (in part) the pounding of the table and therefore cannot be said to cause it. Given Rachlin's philosophical acumen, I do not think these objections will be decisive for him, and I look forward to his replies, which should be incorporated in the next reconstruction of behaviorism.

Although **Skinner** "deals with introspection as a form of perceptual behavior" (Chapters 2 and 11), he does not admit introspection as a form of scientific observation. I did not say that introspection is ruled out only because it is not objective. Instead, I reject this claim and offer four other reasons why introspection is not acceptable. Skinner's position is presented under a fifth objection: Introspection is not a reliable method (Chapter 2).

I did not say that behavior and physiology are "two ways of approaching the same subject," nor am I the source of the quotation about psychophysiology. **Skinner's** description of the relationship between physiology and behavior is precisely what I define as "molar behaviorism," described as a central trait of behaviorism (Chapter 3).

Skinner seems to confuse the behavioral data language (Chapter 3) with behavioral interpretation (Chapters 10 and 11). Although he offers a host of interpretations of purposive, intentional, and mental terms (his interpretation of agency, e.g., on pp. 109, 157–60, 193), he would object, I believe, to the report "The rat saw that he could get food by pressing the bar" as a data description of what is observed during operant conditioning.

Nostalgia. It is perhaps a sign of behaviorism's decline that nearly all the antibehaviorists invited by *BBS* to review *Reconstruction* did not deign to do so. Consequently the commentators are for the most part sympathetic to behaviorism, although not necessarily to my reconstructed version of it. In contrast, thirty years ago, no full-blooded antibehaviorist would have passed up the opportunity to bash behaviorism with the standard criticisms. I am therefore particularly grateful to **Hamlyn** and to **Marshall**, who rose to the challenge and demonstrated that behaviorism is still alive enough to rouse the passions of its critics. No discussion of behaviorism would be complete without a reiteration of their traditional objections.

Hamlyn questions the relationship between prediction and control on the one hand and understanding on the other. I readily agree that there are senses of "understand" in which prediction and control do not provide explanations or understanding. For example, one might feel that a phenomenon is not truly understood until the internal mechanisms are known or until it is experienced from within. Hamlyn does not specify the kind of understanding he seeks, but I do not deny the legitimacy of his search. I wish Hamlyn would likewise grant to behaviorists the right to seek an understanding of behavior at the molar level through principles that mediate prediction and control. To the extent that these autonomous principles are limited to functional relationships between environment and behavior, they cannot be "wrong" in the ways Ptolemy's theory was wrong. On the other hand, behaviorist theories that postulate hypothetical constructs are testable, when properly constructed, and disconfirmable even though they successfully mediate prediction and control over a limited range of behavioral phenomena.

I, of course, do not claim that "first-person reports may be ignored on the grounds that they have no implications for the prediction and control of behavior." On the contrary, I suggest a variety of roles for first-person reports, including the following: (1) They may function as discriminative stimuli controlling subsequent behavior, especially in rule-governed behavior; (2) they may serve as measures of generalization in the construction of psychological scales; (3) they may be used to infer hypotheses about covert behavioral events (Chapters 8 and 11). What behaviorists do wish to claim, along with psychoanalysts and cognitivists, is that first-person reports are not definitive observations about what is going on inside the reporter and that a psychology based primarily on what people say about themselves will be seriously compromised.

Nevertheless, first-person reports are responses, and, as such, must be accounted for by behaviorists. In Chapter 11 I discuss a variety of ways in which behaviorists attempt to do this. Included also is a consideration of expression of feeling. Because we are not yet blessed with a fully developed theory, these explanations are necessarily unconfirmed hypotheses. It is not unusual for scientists to try to extrapolate successful theories, in this way, to new areas in which the theory has not yet been tested. These extrapolations are necessarily in the form of possibilities, indicating how the theory *might* explain phenomena in the new domain. Such extrapolations are characteristic of good science and the search for truth. I find nothing sinister about them.

Obviously I was not sufficiently clear in Chapter 3 or **Hamlyn** would not have asked his third question (What then is behavior?): I try to show that for behaviorists, "behavior" can mean at least four things: (1) bodily movement (the distinction between physiological events and behavior does not exclude bodily movements such as eye blinking); (2) achievements, that is, effects on the external environment; (3) actions (*de jure* for purposive behaviorists, *de facto* for most of the rest); (4) anything that conforms to behavioral laws. To be sure, the terms "behavior," "stimulus," and "response" are problematic, because behaviorists do not all agree on their definitions and because some of the proposed definitions are inadequate. I therefore devote a good deal of my book (especially Chapters 3 and 6) to the reconstruction of these concepts and to showing their usefulness. Instead of addressing my efforts, Hamlyn merely refers to the traditional "many critics, both philosophical and psychological" who in the past have found fault with those concepts. This is not yet a criticism of *Reconstruction*.

My thanks to **Marshall** for his compliment. He is right that as a conceptual scheme, behaviorism is like an ideology. Although particular behaviorist theories are laden with testable empirical content and therefore refutable, conceptual schemes are not proven or disproven; they either win or lose. I note (p. 278) that contrary to behaviorist predictions, behaviorism is not winning, and I question which premises of the prediction are invalid.

The issue of control is a difficult one and is by no means limited to behaviorists. Teachers, preachers, judges, parents, and psychotherapists, among others, all try to change the behavior of others; for none of them is the moral issue of control a simple one. Behaviorists differ from the rest only because: (1) They are quite explicit about control and their interest in it; (2) they use the unfortunate word "control" with its negative connotations (see **Hineline**'s commentary) rather than "change," "influence," or "improve"; and (3) in some limited range, they are highly successful in controlling behavior. I share **Marshall**'s hope that we can progress on this moral and political issue to ensure that all who exercise authority, control, or influence over others will do so only for the common good.

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