Book Review

The Analysis of Complex Cases: A Review of Schwartz and Lacey's *Behaviorism, Science, and Human Nature*¹

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The goal of Schwartz and Lacey's book, *Behaviorism, Science, and Human Nature*, is laudable: present a fair summary of behavior theory, its assumptions, methods, and accomplishments and then carefully evaluate it to see if it can provide an accurate description of human nature. Their conclusion is that it cannot. They argue that the successes of behavior theory in the laboratory and in applied settings show only that the processes identified in behavior theory can operate, not that they do so generally. Perhaps, they suggest, processes unrecognized in behavior theory play a major role in the behavior of human and nonhuman animals in their normal environments. Perhaps something about the situations studied by behavior theorists suppress these other processes, causing them to go unnoticed. Indeed, they argue, there is strong evidence that this is so. Thus, behavior theory is seen to be inadequate. In its place Schwartz and Lacey favor an account of the sort used in everyday discourse—one based on the concepts of plans, goals, desires, intentions, and so forth—as providing a truer picture of human nature than the one provided by behavior theory.

We argue, in contrast, that Schwartz and Lacey have concocted a bizarre and weak version of behavior theory which they then, not surprisingly, find inadequate to account for much of human

behavior. In our view, this type of argument is always unfortunate, especially so when it is disguised as an authoritative, balanced textbook appropriate for undergraduate students.

In the following discussion we consider a variety of topics in the order that they appear in the book to support our general contention. Although we were able to consider only a portion of the topics that deserve comment, we hope our selection is sufficient to indicate the nature of the problem.

A cursory examination of *Behaviorism, Science and Human Nature* reveals an obvious similarity in its organization to *Science and Human Behavior* (Skinner, 1953). Each book may be divided into three major sections. The first section discusses the assumptions of a behavioral approach regarding human nature and methods for its study. The second section reviews the immediate products (laws, principles and empirical generalizations) that result from this study. The final section is concerned with the application of these findings to more remote and complex phenomena and an evaluation of this application.

Assumptions underlying behavior theory

There is a very succinct description of the properties of causal laws and their relationship to scientific understanding. Scientific accounts of human nature are contrasted with "everyday understanding" of human nature. The major distinction between scientific accounts and everyday accounts that is stressed is the tendency for everyday accounts to emphasize that, "Actions are oriented toward the future, toward their anticipated consequences" (p. 9). In

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understanding human action, "notions of purpose, belief, expectation and deliberation enter our reflections" (p. 9). This is to be contrasted with scientific understanding which identifies external initiating events whose occurrence is necessary and sufficient for the occurrence of the phenomenon to be understood. Behavior theory is presented as a branch of psychology "where commitment to a complete and thorough going scientific account of human nature is apparent" (p. 11).

One cannot find much to quarrel with in the necessarily limited discussion of these issues the authors provide. However, one senses that the authors, by innuendo and emphasis, may dispose the unsophisticated reader towards a negative view of the behavioral approach and its emphasis on scientific understanding. Two instances of this are discussed to document this possible biasing.

The authors do the reader a disservice to the extent that the reader is led to perceive the issue as a choice simply between types of understanding: everyday vs. scientific. For example, the authors contend that individuals seeking the advice of those with scientific knowledge may have unknowingly chosen to give up a piece of their everyday understanding. This characterization of the basis for seeking scientific knowledge tends to suggest an equipotentiality for the two types of knowledge. The desire for a scientific knowledge seems to be based on obtaining suggestions for effective courses of action that will remedy or enhance particular behaviors whereas such suggestions are not often provided by everyday understanding. By failing to emphasize the merits of scientific understanding, the authors do not provide the reader with the incentive necessary to consider critically the appropriateness of differing views and to endure the initial discomfort and difficulty in approaching human behavior from a scientific perspective. [It is instructive to contrast the Schwartz and Lacey discussion and Skinner's (1953) discussion of what is to be gained at the expense of losing "personal freedom."]

The "commitment to a complete and

thoroughgoing scientific account of human nature" which characterizes behavior theory is contrasted with a view in which "it is possible to pursue the understanding of some aspects of human nature scientifically without expecting that eventually all aspects of human nature will be so understood" (p. 11). However, it is not made clear to the reader that when one examines the range of psychological inquiry, there is no clearly agreed upon aspect of human nature which is not subject to scientific understanding. Collectively, psychologists pursue human nature as a scientifically approachable subject matter. The approach of behavior theory is consistent with the collective view. It is worth mentioning that those psychologists who take a more conservative view and approach only their domain from a scientific perspective do not do so based on any evidence that other areas are not so approachable.

Schwartz and Lacey identify two central features of the laws a behavior theory proposes. Those laws will relate environmental events to behavior. This characterization of the laws of a behavior theory would be acceptable if not for the manner in which the authors elaborated upon it. Their discussion of the emphasis on environmental events and behavior is more appropriate to earlier Watsonian formulations than it is to current radical behaviorism (Skinner, 1963). The reader might easily infer that behavior theory is incomplete and has little to say about the many interesting events "occurring within each of us." Behavior theory has dealt with the referential data in great detail. There is ample evidence of concern for "private events" in conceptual analysis (Skinner, 1963), language analysis (Day, 1969) and empirical analysis (Catania, 1975). Behavior theory has tried to argue that it represents a complete approach in that it can address all of the data of psychology. The discussion in *Behaviorism, Science, and Human Nature* does not adequately emphasize this and tends to suggest an incompleteness instead. If the behavioral approach lacks completeness, it should be recognized as

an omission of *concepts* common to other approaches rather than of any *body of data or events* which is demonstrably not amenable to behavioral analysis. Weist (1967) in discussing an earlier critique of "learning-theory" approaches (Breger & McGaugh, 1965) encountered the same pseudo-issue. He points to a failure to distinguish observation and inference. "It would seem advisable, therefore, not to form constructs about some phenomenon in terms of our own theory and then criticize other scientists for not dealing with our constructs" (Weist, 1967, p. 210).

The discussion of the environmental emphasis of behavior theory similarly provides a strong basis for misinterpretation of the behavioral approach. It is difficult to imagine that any behaviorist would object to an analysis of behavior based on relationships among events internal to the organism if the internal events referred to in such an analysis were comparable to the parts of a clock and were related to behavioral events as the parts of a clock are related to the behavior of the clock (the example used by Schwartz & Lacey). Behavior theory is in no way incompatible with a physiological or biochemical analysis of behavior, for example. Behavior theory with its environmental emphasis complements such analyses. The environmental emphasis is in part predicated on an attempt to offset "our customary inclination to hold people responsible for their action, and look *inside* them to their *wishes, desires, goals, intentions, and so on . . .*" (emphasis added p. 13), rather than to look for parts that correspond to the parts of a clock.

Discussion of several significant features of the environmental emphasis of behavior theory are conspicuous by their absence. It is generally accepted that no explanation of the functioning of a system is complete if the analysis stays within the system. Where do the springs, gears, and cogs of the clock come from? Will the clock work under water? What is the effect of winding the clock excessively? One may address such queries by examining the reconstituted composition of the clock or by looking at what has been designated

as environmental factors. Would an exclusively internal account suggest a course of action when the main spring is unwound? Behavior theory has a clearly expressed interest in the control of behavior, an activity which becomes possible as a result of scientific understanding. In principle it should be possible to intervene in either the external or internal milieu of the organism to alter its behavior. In practice we are severely limited by the skin of the organism. Unfortunately, the student is left unaware of the practical and conceptual basis for the environmental emphasis of behavior theory. There is no basic incompatibility between the environmental approach of the behaviorist and other approaches which appropriately look in the organism, those approaches that relate observed inner events to behavior rather than those dealing with the relations between behavior and inferred inner events.

One final comment is necessary with regard to the discussion of the environmental emphasis of the behavioral approach. There is some question as to whether the reader will appreciate, on the basis of the presentation in the text that it is not simply environmentalism that is in conflict with traditional modes of explaining behavior and notions of personal responsibility but any scientifically acceptable causal account. The locus of the antecedent events is unimportant. The environmental emphasis forces the reader to confront the issue of personal responsibility somewhat sooner than accounts which stress internal events. However, even a thoroughly cognitive account stressing internal events does not seem to hold much promise for eventual reconciliation with ordinary language explanations as the authors suggest, but we will leave this matter for others to comment on.

Any preparation used by a scientist for purposes of analysis involves the necessity of assumptions that enable the scientist to relate his or her findings to extra-experimental events. The methods of behavior theory are not unique in this regard. Several features of the methods of behavior theory and their justifying

assumptions earn the attention of the authors.

"If organisms were autonomous initiators of their behavior, or if a complex series of internal processes mediated between the occurrence of environmental events and an organism's behavior, the use of pigeons and rats to discover laws that govern people would be a questionable research strategy" (p. 38). The issue here is not unlike some discussed earlier. If organisms were autonomous initiators of their own behavior no science of behavior would be possible. Behavior theory shares with all other sciences the common assumption that its subject matter is amenable to an appropriate causal analysis. To be concerned about this assumption is to be concerned about the scientific endeavor, not simply behavior theory. The role of internal processes requires additional comment. The authors repeatedly refer to inner states and processes that if important determinants of behavior and if not considered in the causal analysis of behavior "will result in an inevitable incompleteness of causal analysis" (p. 39). There is no indication that a comprehensive and extended (back in time) analysis of environmental factors will be unable to handle the data that other approaches handle by appeal to (or, hopefully, examination of) internal states. Consider for the moment a college sophomore confronted with a problem-solving task. No doubt this behavior would be affected if the sophomore was running an extremely high fever. Such an internal state would clearly be a critical determinant. However, internal processes are no more autonomous than the behavior of the organism, and we would now consider the problem-solving behavior to be jointly determined by presentation of the problem and prior exposure to an infectious agent. (The effects of exposure to the infectious agent might demand an appeal to inner processes, for example, the students being "run down;" but this could also be related to environmental determinants.) Such an extended analysis seems to deal effectively with the data.

It was somewhat surprising to see the

behavioral view in which internal processes are unimportant contrasted with "cognitive psychology whose primary concern is to develop and test hypothesis about the cognitive abilities and internal processes that underlie human behavior" (p. 38). One wonders about the status of the efforts of the contributors to the recent volume *Cognitive processes in animal behavior* (Hulse, Fowler and Honig, 1978) in the scheme of Schwartz and Lacey. Many cognitive psychologists appear to be no more immediately concerned with human behavior than many behavior theorists.

According to Schwartz and Lacey, the critical environmental events that behavior theory focuses upon are rewards and punishments. "If you want to know why someone did something, do not ask. Analyze the person's immediate environment until you find the reward. If you want to change someone's actions, do not reason or persuade. Find the reward and eliminate it" (p. 16). Only an antiquated and narrow interpretation of behavior theory would advocate such an approach. While these factors are important determinants of behavior, it is patently false to suggest that behavior theory's explanations do not consider factors other than rewards and punishments. Two recent volumes reflect a variety of topics and factors that have been the subject of behavioral analysis (Honig, 1966; Honig & Staddon, 1977).

It is worth mentioning again that the reader is not presented with a dispassionate presentation of the assumptions of behavior theory of the type necessary for scholarly consideration. The authors' bias is evident again in the following passage.

While rewards and punishments are always an influence on human action, they are perhaps the least distinctively human influence. What we most care about, if we are to understand human action, is what people do despite the presence of obvious bribes and threats (p. 40).

The use of the terms bribes and threats is both connotatively and denotatively unfortunate if the authors wish to present an unbiased and correct view of behavior theory. There is a critical distinction

between the processes involved in controlling behavior with bribes and threats and with reinforcers and punishers. Unfortunately, the authors do not share this distinction with the readers. A more thorough presentation of behavior theory would reveal the need for a concept of stimulus control when talking about bribes and threats.

Schwartz and Lacey's discussion of the relationship of prediction and control to the more general goals of science should be of value to the reader. Again, it is unfortunate that there is a strong negative tone to the way in which it is related to behavior theory. Isolating lawful relationships that enable us to predict and control may have important practical implications. This is the stuff upon which technology is built. Despite the construction of a successful technology, there is no guarantee that the laws involved are adequate to explain more generally the subject matter of the science. Laws derived in the laboratory show only how behavior may be affected, not that these factors are necessarily affecting behavior generally. Behavior theory is clearly sensitive to this. The latter chapters of *Science and Human Behavior* represent one effort to show that these principles apply in situations other than those described by Schwartz and Lacey as involving "sterile boxes" and "a super controller who is secretly shaping our lives" (p. 42).

The content of behavior theory

The middle chapters (chapters 3-6) are supposed to provide an unbiased and "sympathetic" (p. 159) review of the content of behavior theory. Unfortunately, much of this material can only be interpreted as an effort to show the need for cognitive and lay concepts in analyses of operant and Pavlovian conditioning experiments. A few examples should suffice to document the point.

In discussing the "blocking effect" in Pavlovian conditioning, the authors conclude that,

We now know that conditioning involves more than simple temporal contiguity. Animals seem to evaluate *information* conveyed by various stimuli

and selectively to associate the most informative stimuli with the USs. For conditioning to occur, a potential CS must tell the animal something about the US which it would not otherwise know (p. 49).

Now most researchers probably would agree that the blocking phenomenon nicely demonstrates how a complete description of the events responsible for Pavlovian conditioning must include more than the mere statement that two stimuli were paired. But do the blocking results require us to invoke cognitive concepts like "evaluating information" and "knowing?" Might it not be possible to formulate alternative descriptions of these facts in terms of relations between environmental events (current and remote) and behavior? No effort is made to explore such possibilities and, consequently, to show by logic that the behavioral alternatives are fundamentally deficient. Instead, the discussion proceeds as if there were simply two mutually exclusive possibilities: (1) that mere pairing sufficiently describes the causal factors (equated with behavior theory) and (2) that an adequate description requires concepts like "knowing," "evaluating," and "information." Ruling out the first is then portrayed as providing sufficient evidence for the second. Students might be persuaded by such tactics, but they are not educated.

Consider another example. The authors discuss the interesting work on what is often described as laboratory analogs of economic choice. Certain functional relations, such as between rate of response and rate of reinforcement on variable-interval schedules, appear to differ depending on the duration of the experimental session. There may also be differences in the outcome of choice procedures depending on whether the alternative responses are reinforced by the same or by different kinds of reinforcers. Surely everyone would agree that these kinds of results will have to be incorporated into any adequate theory. The question is how this is to be done. Some researchers have been exploring the possibility that concepts from the field of economics (e.g., elasticity of demand, closed versus open economies and substitutability) might supplement

or even replace currently accepted behavioral concepts. Alternatively, it might be unnecessary to invoke new terms or concepts. We might, for example, be able to interpret some of the results as due to drive operations—operations that alter the effectiveness of events as reinforcers—along the lines suggested by Skinner (1938, chapter 9) and Herrnstein (1977), among others. Reputable behavior theorists can be found exploring both possibilities (cf. Rachlin, Ballalio, Kagal, & Green, 1981 and commentaries). But the reader of this book would get no sense that this is an area of active research and debate. Instead, the issue is presented simplistically as a choice between limited behavior theory and the more comprehensive economic approach, and it is clear where Schwartz and Lacey stand on this choice:

The rules by which animals allocate their resources in choosing among alternatives seem to be the same as the rules people use in making similar decisions. To the extent that economics provides us with a framework for understanding human choice, it may also underlie some principles of behavior theory since behavior theory seems to embody, in the laboratory, so many of the factors that are central to economic analysis (p. 122).

... the matching law is just a special case of the operation of far more general, economic principles. These principles involve the interplay of demand elasticity, income, price, and commodity substitutability, in open and closed systems. These principles capture all the phenomena explained by the matching law as well as a wide range of others that the matching law cannot explain (p. 123).

We may note in passing that there is considerable debate among behavior theorists as to whether or not the matching law describes a fundamental principle. The more important point, however, is that no mention is made of the possibility that an account based on the principles of behavior theory might be able to subsume the economic principles. The possibility is not rejected on the basis of reasoned argument, it simply is ignored.

In a third example, the authors assert the need for distinguishing an hedonic effect of reinforcers from an informational one.

Reinforcers have at least two distinguishable characteristics . . . Reinforcers have hedonic value:

Positive reinforcers are pleasurable and negative reinforcers are aversive. Virtually all research on operant behavior assumes that this hedonic property is the relevant one in determining the outcome of experiments. In addition, reinforcers are informative: They tell an organism whether it is on the right track or not (p. 69-70).

This distinction between informative and hedonic properties of reinforcers is not often made in research on operant behavior. The reason, in part, is that most research is done with animals in situations in which the hedonic properties of the consequences of responses are clearly relevant. It is hard to imagine why a rat would press a lever or a pigeon peck a key except if the consequences of these responses is pleasurable. In the case of humans, it is less clear. One might want to learn to play the piano or to swing a golf club properly. The reinforcers for these responses might be a well-played piece of music or a well-hit golf shot. The teacher's "good" in this case would be hedonically irrelevant, as would a piece of candy or a quarter. The relevant function of all these consequences would be the information they provided about progress toward the goal (p. 70).

Schwartz and Lacey do not describe the data that convince them of the necessity of making this distinction (although one can guess what the relevant data might be). This is unfortunate because the distinction is presented in a way that suggests a serious deficiency in behavior theory, as if behavior theorists have been oblivious to the kinds of everyday examples mentioned in the passages quoted above. One wonders why Schwartz and Lacey do not suggest how a behavioral account might be developed in terms of discriminative and conditioned reinforcing functions of stimulating events. (There is a large literature on the question of the similarities between unconditioned and conditioned reinforcers.) If such an interpretation is fundamentally flawed, the authors could perform a valuable service by showing how. But the compelling case was not made, and so we are left with an unjustified intrusion of cognitive and lay concepts into what is purported to be an unbiased summary of behavior theory. Once again, the authors have simply asserted the need for such concepts instead of demonstrating their need by logic and data.

What runs through these and other examples is an apparent insensitivity to what many would regard as a major goal and accomplishment of behavior theory,

namely the development of a precise, systematic, and parsimonious technical language for describing psychological phenomena. Consider, on the one hand, Michael's (1982) carefully reasoned, almost apologetic, justification for introducing a new technical term, Catania's (1969) thorough analysis of the vocabulary of reinforcement, and Himeline's (1980) thoughtful analysis of the implications of the behavioral descriptive language. Then consider, on the other hand, Schwartz and Lacey's indiscriminate mixture of behavioral, cognitive, and lay terms in descriptions of psychological phenomena, as exemplified in the passages quoted above and below. One has to be struck by the contrast.

What possible conclusion could a student reach about what behavior theory is and about how behavior theory differs from other approaches from statements like those already quoted and like the following? In a discussion of the shaping of lever pressing in a rat we read, "This time, while sniffing at the lever, the rat's nose inadvertently depresses it" (p. 71).

And continuing we read,

With continued experience, the rat's behavior changes. It learns that rearing is unnecessary to produce food, only depressing the lever with the nose is necessary. The rat seems to identify the single feature shared by each of its different activities with respect to the lever, and that is the downward deflection of the lever. Once this information is acquired, the efficiency of the rat's behavior increases dramatically (p. 72).

In a discussion of the so-called learned helplessness effect, we find that

... when organisms are exposed to procedures in which important events (like food or electric shock) are presented as independent of their behavior, they learn precisely that—that events are beyond their control. They generalize this learning to new situations, with the result that the development of effective responses in these new situations is retarded—even sometimes prevented altogether (p. 75).

In introducing the topic of discrimination, the authors describe a situation where keypecking is reinforced in the presence of a red keylight but not in the presence of a green keylight. Then an analogy is drawn between that situation and one where a child is learning to cross

streets in accordance with whether a traffic light is red or green. We find that,

The child being taught to cross the street already knows the difference between red and green, as does the pigeon. What must be learned is the difference in the *relation* between each of these stimuli and the consequences of crossing the street (p. 126).

From statements like these, how could a student possibly come to distinguish behavioral from nonbehavioral accounts? And how could a student come to appreciate the possibility of a behavioral account of complex cases when the simpler cases are described in lay and cognitive terms? Unless a student can do these things, it is unlikely that he or she would be prepared to distinguish serious attacks on behavior theory from specious ones.

Several topics essential to understanding behavior theory are noticeably absent. For example, the reader is not led to understand how the notion of the operant as emitted behavior freed the study of operant behavior from the confines of an artificial S-R framework. Nor is the reader shown the connection between the emitted nature of operant behavior and the use of response rate as a fundamental datum. Nor is sufficient attention given to the role of functional definitions of basic terms in behavior theory and to the distinction between functional and structural definitions. Nor is there sufficient discussion of the range of events that can function as reinforcers. Thus, the reader might falsely conclude that behavior theorists equate "reinforcer" with a few kinds of externally delivered events such as food and water.

In short, we believe that Schwartz and Lacey have provided a summary of behavior theory that is neither complete nor accurate. It might represent a good introduction to some of the facts and issues that have emerged from the study of operant and Pavlovian conditioning, but that is a different matter.

Extensions and evaluation of behavior theory

The final section of the book is intended as a critical examination of behavior theory. Chapter 7, entitled "Misbehavior

of Organisms," is concerned with the adequacy of behavior theory to provide a satisfactory account of the behavior of nonhuman animals; chapters 8 and 9 are concerned with extensions to analyses of human behavior. We will consider first the material in chapter 7.

Schwartz and Lacey open by emphasizing a point made earlier: demonstrating that operant and Pavlovian procedures influence a particular kind of behavior in the laboratory is not equivalent to demonstrating that operant and Pavlovian factors are important determiners of similar behavior in the animal's natural environment. Perhaps other kinds of control operate in the natural environment, but our laboratory preparations somehow suppress them so that their influence goes unnoticed. Or perhaps our preparations cause operant and Pavlovian factors to dominate in ways that are quite atypical. If so, our procedures might have given us a distorted picture of the importance of operant and Pavlovian principles. (One is reminded here of Kohler's charge (1925) that Thorndike failed to observe insightful solutions because his problems were so constructed as to preclude any solution other than trial-and-error.)

At one level it is hard to disagree with Schwartz and Lacey's concern. Most of what is known about operant and Pavlovian conditioning has come from only a few preparations, and so we cannot really know for certain how general and comprehensive our current behavior principles will turn out to be.

However, Schwartz and Lacey are not just reminding us of our limited data base. They are convinced that behavior theory is on the wrong track. They see as especially damaging the phenomena of autoshaping (especially the results of the omission procedure) and "misbehavior" such as described by the Brelands (Breland & Breland, 1961). In the autoshaping omission procedure, food delivery follows at the end of a period of key illumination provided the pigeon has not pecked the lighted key. Thus, there is an operant contingency: food follows periods of not-pecking the lighted key and never follows pecks to the lighted key.

Nevertheless, pigeons usually continue to peck on a substantial proportion of keylight trials. In a widely cited example of "misbehavior" in the racoon, the operant performance of depositing a token in a vending machine was followed by food reinforcement. Token-depositing initially increased in rate, but later declined due to the intrusion of token-washing. What is common to both examples is that some directed skeletal behavior emerges in the absence of explicit reinforcement and interferes with the operant performance that is explicitly reinforced. Operant control thus appears fragile even in these relatively controlled situations. Have we grounds, then, for believing that operant factors play a major role in the animal's natural environment?

How might behavior theorists respond to the challenge posed by these sorts of data? Schwartz and Lacey mention two possible approaches. One could either deny the relevance of such phenomena for understanding human behavior, or one could try to give an account of the phenomena in terms of behavior principles. The reasoning behind the first approach is this: the behavior of organisms is comprised of both instinctive (or phylogenetic) behavior and modifiable operant behavior. Most interesting behavior in humans is of the operant sort. Since the "misbehavior" phenomena represent intrusions of instinctive-like behavior, they are largely irrelevant to understanding human behavior.

As an example of the second approach, it might be possible to interpret the "misbehavior" phenomena as resulting from Pavlovian conditioning. It has long been recognized that Pavlovian associations are an inevitable consequence of operant contingencies. When a response is differentially reinforced, stimuli that are associated with the performance of the response will be differentially associated with the reinforcer. In the racoon example, the contingency between token-depositing and food reinforcement will ensure a differential association between stimulation from the token and food, a Pavlovian association. On the basis of familiar behavior principles, then, we

would expect the tokens to come to control Pavlovian conditioned responses. But what would be the nature of these conditioned responses? At one time or another behavior theorists have suggested that the conditioned response resembles the unconditioned response and that the conditioned response is likely to be one controlled by the autonomic nervous system. Clearly, these last principles relating to the form of the conditioned response will not help us understand the intrusion of skeletal behavior like token-washing. But suppose we were to liberalize the notion of the conditioned response to include directed skeletal behavior and behavior that does not necessarily mimic the unconditioned response. Then the token-washing behavior would seem understandable as an instance of Pavlovian conditioning. Should we liberalize the notion of the CR this way? Doing so would require some modification of a few principles. But for many researchers the data appear to require such modification (cf. Hearst & Jenkins, 1974; Williams, 1981). That Pavlovian procedures control skeletal behavior is no longer a radical proposal within behavior theory.

There might be other ways to interpret the "misbehavior" phenomena in terms of familiar behavior principles. Herrnstein (1977), for example, proposed that the "misbehavior" phenomena might be interpreted as due to changes in drive operations (cf. Skinner, 1938, Chapter 9). The effect of some drive operations might be fleeting; others might be more extended in time. Perhaps something about the association between token and food makes token-washing (or the sensory consequences of token-washing) highly reinforcing. If so, the directed skeletal behavior that emerges would be regarded as genuinely operant in nature. The particular reinforcer might seem odd, but the principle of control would be quite ordinary. That we know less than we should about the effects of drive operations other than those from relatively severe food or water deprivation hardly seems grounds for dismissing the approach.

Although Schwartz and Lacey ignore Herrnstein's proposal, they do consider

briefly the possibility of an account in terms of Pavlovian conditioning. But they are not optimistic about its likely success.

For if the racoon's behavior is to be explained in terms of the joint effects of Pavlovian and operant principles, this means that any situation offers the potential for such joint influences. And this possibility very much complicates the task of behavior theory (p. 171).

Of course, behavior theorists knew that Pavlovian conditioning was an inevitable consequence of operant procedures long before the demonstrations of the Brelands (Breland & Breland, 1961), and they were well aware that such combined effects made for complicated outcomes (cf. Skinner's, 1953, discussion of the effects of punishment procedures). Although we might hope for greater simplicity, it seems unwise to reject behavior theory on the grounds that its application will often be complicated unless a simpler alternative is available that will accomplish as much. What alternative do Schwartz and Lacey suggest? They propose what they call an ethological approach emphasizing the concept of the "genetic blueprint." The basic idea seems to be that natural selection has created a genotype, a blueprint for development that is designed to favor the development of a successful phenotype. It does this by placing constraints on the kinds of factors that can influence development and functioning. Learning is thus constrained by the kinds of events that can participate in learning, by when learning can occur, and by the circumstances in which learning can occur. These constraints presumably arose as the result of natural selection: animals that learned the wrong things were less likely to reproduce than their conspecifics whose learnings more appropriately matched the requirements of their ecology.

Although Schwartz and Lacey do not tell us exactly what following this approach would entail, one can infer the main implications. Presumably one would at least begin by constructing a detailed description of development in the animal's natural environment, paying particular attention to the actual function of each instance of learning. Since these

natural histories of learning would differ for different species, a number of species would have to be studied this way to have a reasonable chance of discovering principles of any significant generality.

It would be hard to fault a serious effort to expand our data base with descriptions of concrete adjustments of animals (as opposed to constructions of evolutionary "stories"), but we can remain skeptical that a wholesale switch from behavior theory to the "genetic blueprint" approach will set things straight. For one thing, there are serious conceptual and empirical difficulties with the "blueprint" concept (cf. Lehrman, 1970). For example, adaptive adjustments in novel environments are not comfortably handled by a "genetic blueprint" notion (cf. Kuo, 1967). Behavior theory, in contrast, by emphasizing general mechanisms of behavior modification can deal comfortably with such adjustments. More to the point, the "genetic blueprint" is a construct that is invoked to account for certain facts. Those same facts can be accounted for in other ways—for example, by reference to variables that operated in the evolutionary history.

Behavior theorists have distinguished questions about behavior whose answers lie mainly in the phylogenetic history of the species from questions whose answers lie mainly in the individual's history (e.g., Skinner, 1966b). Questions like, "Why can grain be used to reinforce the operant behavior of pigeons but not of rats?" "Why do dogs salivate to CSs while pigeons peck them?" and "Why can the stimulus property of equivalence come to control the operant behavior of human children but not of pigeons (Sidman, Rauzin, Lazar, Cunningham, Tailby, & Carrigan, 1982)?" are probably questions of the first sort. If one were interested in such issues, one probably would need to examine the animal's natural history. The kinds of principles that might emerge from such study could involve relations between aspects of an animal's ecology and forms of behavior (cf. Krebs & Davies, 1981). These kinds of principles, and the research programs that produce

them, would complement rather than contradict current behavior theory. (To suggest that differences in phylogenetic history might be important for observed differences in some characteristic is not at all equivalent to saying that genes are more important for the development of some characteristics than of others.)

The major issue, again, is whether there are principles of behavior modification of any significant generality. The kinds of data cited by Schwartz and Lacey do not really undermine our confidence that there are. The data might show that we are not yet in a position to predict details of the behavioral outcome of several interacting processes in novel settings, but that is not at all equivalent to showing that the principles lack generality.

The final two chapters of *Behaviorism, Science, and Human Nature* deal with applications of behavior theory and their evaluation as well as a more comprehensive discussion on the limits of behavior theory. These chapters present the author's major acknowledged attack on applications of behavior theory to human behavior. After presenting several illustrations of programs that presumably emanate from behavioral principles, the authors reexamine the specifics of these programs to determine more precisely their relevance to behavior theory. Although it is possible to disagree with several aspects of their analysis, they correctly point out that many reported instances of "application" have not been correctly attributed to events that correspond to the critical elements of behavior theory. Thus, these successful "applications" are not convincing evidence for the underlying conception of human nature held by behavior theory.

We agree that these inappropriate analyses must be replaced with more thorough analyses. However, it is not at all clear in so doing that we will, as the authors conclude, "identify other factors in these applied settings that may be critical to their success, but not amenable to an analysis in terms of behavior theory principles" (p. 216). We can find the same criticism of an inappropriate analysis and some constructive suggestions in a more

congenial context. Michael (1980) has pointed out that

... many human situations are often tossed off as examples of this or that simple concept, when in fact such analyses are quite superficial and easily spotted as such by critics of the behavioral approach (p. 14).

(We may wish to consider Michael's suggestions for education of behavior theorists such that they would be less likely to engage in such superficial analyses.) We feel that the authors have on the one hand appropriately responded in a critical way to superficial analyses of complex behavior. On the other hand, unfortunately, their own reanalyses are similarly superficial, leading them to conclude an incompatibility with behavior theory.

The situations regarded as most problematic for behavior theory—those that according to the authors require references to factors not amenable to behavior theory—may involve “planning, flexibility and intelligent variation,” as for example an activity such as “running a kitchen.”

When a patient earns tokens for successfully running a kitchen, what—if anything—can we say has been reinforced? What is the operant? What becomes more probable as a consequence of having been reinforced? (p. 220).

The authors comment further,

But given the flexible character of the processes of preparation, their variation from one day to the next—indeed, their deliberate, planned variations from one day to the next—the principles of behavior theory have little to tell us about them. At a very abstract level, we can say that just as food reinforces whatever activity of the rat produces the outcome of the lever being depressed, so the tokens reinforce whatever activity produces the outcome of edible food being available at the right time. The problem with this analysis is that it does not explain the variation in the processes of food preparation. And it is just this variation that is essential for the successful running of a kitchen (p. 221).

We agree with the authors that only a tenuous correspondence may exist between the variability in the lever press and the kitchen behavior of interest. The variability is much more functional in the kitchen behavior. What is difficult to understand is why the authors feel that the appropriateness of behavior theory rests on the basis of this rather superficial analysis, which seeks a simple, easily iden-

tified behavior whose repetition is easily noted. It would be easy to construct a behavioral situation with a rat subject involving chains and concurrent chains of discriminated operants with sequential and topographical contingencies and similarly be unable to find simple answers to such questions as, “What response becomes more probable?” and “What is the operant?” Unfortunately, *preparations that illustrate analytic concepts have been inappropriately viewed by critics as structural units*. The case with the human in the kitchen is exceedingly complex, but not obviously beyond analysis in terms compatible with behavior theory, although it clearly cannot be understood by any strong and direct reference to the functioning of the reinforcement of an isolated operant. Although the complexity of the situation requires a complex account, the effort seems worthwhile as an alternative to the account offered by the authors.

If we brought the explanatory tools provided by common sense to the token economy, to account for running a kitchen, we might say that the patient has the goal of obtaining tokens, and he does in the kitchen whatever he believes sufficient (and perhaps also necessary) to obtain the tokens. These beliefs correctly will vary in content from day to day in accordance with the sub-goals of the meal that he is setting out to prepare on a given day and how he plans to reach these subgoals (p. 222).

The final thrust of the text is to argue again that operant contingencies suppress other influences on behavior. While it is acknowledged that reinforcers and punishers may profoundly affect behavior, the question at issue is whether they are in fact the critical determinants of behavior in the natural setting. The authors' analysis of work leads them to conclude that factory work is clearly understandable by appeal to principles of behavior theory. However, despite the ubiquity of work, the authors suggest this positive assessment should not be construed as support for the notion that the principles are in evidence in natural settings. Rather, historical analysis leads the authors to conclude that the modern workplace, like the token economy, is a contrivance in which behavioral principles can function, not a natural setting in

which fundamental principles are revealed. Branch and Malagodi (1980) have responded critically to a more extensive presentation of this argument (Schwartz, Schuldenfrei, & Lacey, 1978), and we will not comment further on this matter.

Some comment is necessary, however, on two situations described by the authors as illustrative of the "suppression or superceding of non-operant influences on behavior by operant contingencies" (p. 249). The first of these deals with the observation that reinforcing certain activities of children may result in a lower frequency (compared to operant level) of occurrence of these behaviors when reinforcement is no longer provided (Lepper, Greene, & Nesbitt, 1973). As the authors point out, there is considerable controversy concerning the reliability of this effect and the conditions under which it will be observed. Let us assume that the effect is reliable, however. The authors would then conclude that we have, "... an example of how operant control may be achieved at the expense of other kinds of influence on behavior" (p. 247). The significance of this statement in the lengthy argument that the authors provide (which has not been presented here) concerns the conclusion that these other sources of control are more fundamental (more reflective of human nature) and not compatible with behavior theory. Unfortunately, there is not sufficient elaboration by the authors to clarify how this conclusion is arrived at. The nature of these other sources of control is completely unspecified, and it is difficult, therefore, to see why one might suspect they are incompatible with behavior theory.

The second situation describes the retarded discovery of a rule for a sequence of key presses by adult subjects if they have a history of prior reinforcement for a particular sequence of presses. In discussing these observations, the authors state,

It seems plausible that the rule discovery strategies of the students without pretraining were under nonoperant influence. And the operant influence introduced by the pretraining changed the nature of these strategies, in this case making them relatively ineffective (p. 249).

What is the basis for considering rule discovery strategies to be under nonoperant influence? Why is it not equally plausible to consider conflicting operant influences in the case of the pretrained subjects? In what sense can the authors reasonably regard these observations as laboratory demonstrations that operant contingencies in gaining control of behavior suppress other influences?

With regard to the implications of these observations for educational settings, the authors suggest that the use of operant contingencies, if they suppress other influences, interfere with the development of the ability to discover rules and with the development of inherent reward properties as part of the learning process itself. Granting for the moment that there are other influences that may be displaced by operant contingencies, one can only correctly conclude that the use of contingencies *as described* might be detrimental to the educational endeavors. But other operant contingencies might be quite productive. The possibility of using operant contingencies in an inappropriate manner should be recognized as such by critics, as well as those sympathetic to behavior theory. Even if such uses could be documented, they should not be construed as the basis for eliminating operant contingencies generally. It seems clear that whatever critical factors and principles eventually are revealed with respect to human behavior, there will be the opportunity for inappropriate use. In light of this, one wonders why the authors felt obliged to point this out specifically with regard to operant contingencies.

A major concern of the authors is with behavior that may be considered creative, unique, reflective of planning, goal directed, etc. as in the behavior of the scientist and the factory manager as opposed to the factory worker. Much of the behavior of this type is characterized by functional variability and is unique. It is not difficult to see that such behavior cannot be simple repetitions present because of past reinforcement. The authors, however, go much beyond this conclusion and assert that the concepts of behavior

theory are not well suited to deal with such behavior. Again, we feel that this conclusion reflects superficial analysis and little appreciation for the efforts that have been made to show how behavior theory can accommodate such behavior. Skinner has presented the form of such an analysis on several occasions. In *Science and Human Behavior* there is a lengthy discussion of "problem solving" (p. 246). In the Honig volume (1966) there is a discussion of the role of formal analysis as a guide to innovative behavior. In talking about the behavior of the factory manager in contrast to the factory worker, Schwartz and Lacey state:

The manager tells us that he is converting his factory from making large to making small cars because he has calculated the expenditure of conversion will more than be repaid from greater sales he expects with small cars. This is thoroughly intelligible, building explicitly upon such familiar notions as 'calculation' and 'expectancy', and implicitly upon 'goal'—though this is not a simple extrapolation of the past experience of the manager. It is a new act, a consequence of experience, well understood, but completely opaque to behavior theory (p. 251).

It is difficult to understand how the authors arrive at their conclusion. The conversion is obviously not the result of past reinforcement of this behavior, but does that mean that it is opaque to behavior theory? The behavior may be understood by demonstrating the discriminative function of the formal analysis. Skinner (1966a) provides several examples of such analyses and how they control behavior. Why are such analyses overlooked by critics? One possibility is that the behavioral analysis does not involve the concepts of the alternative analysis and may, therefore, be mistakenly regarded as inadequate. We must increase our efforts to make critics understand that behavior theory is to be evaluated by its adequacy in dealing with the data, rather than by the resemblance of its explanations to everyday conceptualizations.

Although we have focused on the positive assertions, there is an omission that is noteworthy in a text with the title that the current one carries. There is no mention of verbal behavior, language, or rule-governed behavior, nor do they ap-

pear in the index. It is difficult to imagine an adequate discussion of human nature that attempts to assess the adequacy of the behavioral approach that does not require reference to these topics.

Conclusion

We initially approached the book with positive expectations. What we had expected based on comments in the preface was a cogent and veridical presentation of behavior theory with a forceful attempt to apply its principles to a variety of settings, thus providing a basis for assessment of the approach characterized as behavior theory. Instead, we found that several critical aspects have been omitted, and features foreign to behavior theory were included as part of behavior theory. Analyses of complex events were dealt with superficially and regarded as reflective of inadequacies of the underlying approach, rather than of the superficial analyses. We, too, would find fault with behavior theory if the analyses offered by the authors were the best that could be provided. Fortunately, in view of the alternative accounts that the authors favor, this is not the case.

Overall, Schwartz and Lacey's criticism of behavior theory has a familiar ring (see, for example, Breger & McGaugh, 1965). Often the critic begins by describing behavior theory as if it were a static "thing" whose "essential features" can be summarized briefly and simply. The critical comments are then directed at the "essence" so described. But, of course, a school of thought like behavior theory is much too complex, subtle, dynamic, and internally disputatious to be so characterized. Consequently, the criticism appears to be directed at a simplistic caricature instead of at the reality (cf. Wiest, 1967).

To be truly effective, critical analyses will have to deal with the reality of behavior theory instead of with its caricature. Given the complexity of behavior theory, it is hard to believe that such criticism will be able to give a compelling answer to such global questions as, "Will behavior theory be able to provide a satisfactory account of human nature?"

A more realistic goal might be to identify problem areas, to indicate possible approaches toward a solution, and to describe implications of the possible solutions. As part of this analysis, one could ask how much behavior theory could change before it is no longer reasonable to call it behavior theory. For example, if one were to develop an account of choice behavior based on concepts and principles from economics, would such an account be considered as belonging to behavior theory? Suppose the economic concepts were rigorously defined in terms of confrontable environmental events and suppose explanations took the form of functional relations between measurable aspects of environmental events and measurable aspects of behavior, would it then be behavior theory? Although behavior theory is an approach to our subject matter, it is more than just an approach. It includes also a history, a language, and a tradition of accepted facts and principles. Whatever its merits as a theory, Herrnstein's matching law seems a less radical departure from early behavior theory than do the accounts in terms of economic concepts, even though both kinds of accounts might be formulated in terms of functional relations between confrontable events. This is probably so because it is relatively easy to see the methodological and conceptual connections between the matching law and earlier concerns within behavior theory (e.g., Skinner's concern with the reflex reserve). It is harder to trace the development of economic concepts back through the tradition of behavior theory. Thus, relationship, in an evolutionary or family-tree sense, seems to be part of how we judge whether or not something belongs to behavior theory.

Consider the following three statements that might be made about a rat that is pressing a lever in a Skinner box.

- (a) The rat is pressing the lever because past instances of lever pressing have been followed by reinforcement.
- (b) The rat is pressing the lever because there has been a positive correlation between the rate of lever pressing and the rate of reinforcement.
- (c) The rat is pressing the lever because it knows

that it has to press the lever in order to get the reinforcer.

Although the three statements are different, many would regard the first two as behavioral but the third one as not. That there is considerable current debate about which of the first two better describes the independent variable in operant conditioning makes clear that behavior theory can include a variety of fundamental principles—even incompatible ones. Yet there are constraints on what can and cannot be included, since the third statement would not be regarded as belonging to behavior theory. A critical analysis of behavior theory that clarified these kinds of issues about how behavior theory might evolve while still retaining its identity would be most useful. Superficial attacks on caricatures are unlikely to have any more impact now than they have ever had.

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