Neo-Skinnerian Psychology: A Non-Radical Behaviorism

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Radical Behaviorism makes the implausible claim that "the appeal to mind explains nothing at all" (Skinner 1971, p. 186). Clearly, such a claim (if accepted) would lend strong support to the Skinnerian research program, if only because it would eliminate the major competition. But what support remains when such a claim is not accepted? This paper shall argue that the Skinnerian research program need not depend upon the supposition that there is something scientifically illicit or vacuous about the explanations offered by mentalistic psychology. Distinguishing Radical Behaviorism from a position that grows out of Skinner's writings from 1938 through 1950, it asserts that the latter position provides a conception of the behavior analytic program that is compatible with the basic claims of cognitive and folk psychology.

We may give a rough definition of this position by following Cummins (1983) in distinguishing between two types of scientific theory. On the one hand, there are transition theories, which "explain changes of state in a system as effects of previous causes" (p. 1);and on the other, there are property theories, which "explain the properties of a system not in the sense in which this means 'Why did S acquire P?' or 'What caused S to acquire P?' but, rather, 'What is it for S to instantiate P?', or 'In virtue of what does S have P?'" (pp. 14-15). In general, transition theories seek to establish a set of independent variables which control a given dependent variable, and to characterize the relationship between the former and the latter mathematically. Property theories, on the other hand, attempt to identify and describe the mechanisms which underlie such relationships. Transition theories explain by subsumption under causal laws (p. 5); property theories explain by analysis, showing that something having certain components organized in a certain way is "bound to have the target property" (p. 17). Transition theories add to our understanding of what causes a given event to occur; property theories add to our understanding of how these causes bring about their effects.

If we apply this distinction between transition and property theories to psychology, then most psychological theories would fall under the heading of property theories. They focus upon properties (in the form of dispositions) as their explananda, and they account for these properties by analyzing them "into a number of less problematic dispositions such that programmed manifestation of these analyzing dispositions amounts to a manifestation of the analyzed disposition" (p. 28). This pattern of explanation, which Cummins (unfortunately) calls "functional analysis,"[1] is the

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dominant one in contemporary cognitive psychology. By contrast, the body of knowledge known as 'behavior analysis' is a collection of transition theories that attempt to identify environmental variables which bring about changes in the pattern of responding and to characterize this relationship mathematically.

Although behavior analysis is a collection of transition theories, Radical Behaviorism rests upon Skinner's flawed attempts to provide a functional analysis of behavior. (Here, as elsewhere, I use 'functional analysis' in Cummins' sense, not Skinner's–see note 1). By disassociating these functional analyses from the behavior analytic program, we take the first step in the definition of "Neo-Skinnerianism"–a position implicitly adopted by many active members of the Skinnerian research program.[2] Neo-Skinnerianism acknowledges the appropriateness of mentalism for the functional analysis of behavior. It holds, however, that to arrive at transition theories of behavior, mentalism is not only unnecessary, but less than optimal.

Usually, we think of Skinner as limiting himself to the compilation of cumulative records of schedules of reinforcement. The main point of his most important experimental analysis of operant behavior, however, is to give a *functional analysis* of the patterns described in cumulative records (Ferster & Skinner 1957). The thesis of this book is that schedule effects are due to the strengthening of responses which occur just prior to the delivery of reinforcement. On the basis of this hypothesis (and it is an hypothesis in anybody's book–including Skinner's), Ferster and Skinner attempt to account for such well known phenomena as the fixed-interval 'scallop,' the fixed-ratio 'stairstep,' and other characteristic patterns found in cumulative records of the different types of schedule. The crucial feature of this hypothesis is that it attempts to give a functional analysis of such dispositions while positing no information processing on the part of the organism.[3]

I believe it is fair to say that the settled opinion among behavior analysts is that the Ferster/Skinner hypothesis has serious problems–many would say fatal problems. Indeed, the typical Neo-Skinnerian has concluded that if schedule effects are to be given a functional analysis, we must posit information processing mechanisms that bridge the temporal gap from environmental cause to behavioral effect. Such theories are now routinely published in *The Journal of the Experimental Analysis of Behavior*, the first, and still most important, Skinnerian journal. This means that one important source of support for Radical Behaviorism has lost significant ground even among Skinnerians.

There is, however, a less direct route by which Skinner has attempted to offer a functional analysis of operant behavior. Beginning in 1953, Skinner has emphasized the analogy between operant conditioning and natural selection. On the whole, this is a sound analogy which can add to our understanding of the unique conceptual features of behavior analysis. Skinner, however, gives the analogy a characteristically radical twist by asserting that just as natural selection operates upon essentially random genetic variation, so likewise operant conditioning operates upon essentially random behavioral variation (Staddon & Simmelhag 1971). The idea that behavioral variation is random is of a piece with the idea that reinforcement operates on the basis of contiguity–i.e., it is another attempt to give a functional analysis of behavioral dispositions without positing information processing.

This is a recurrent theme of Skinner's speculations about the mechanisms that account for behavioral dispositions. In effect, Skinner tries to prove that underlying processes are irrelevant to *subsumptive* accounts of behavior by proving that they are irrelevant to *functional* accounts as well. In this he has failed totally.[4] It has long been clear that a functional analysis of linguistic behavior will require complex information processing mechanisms (Chomsky 1957), and it now begins to appear that even the analysis of schedule effects may benefit from such assumptions (Mackintosh 1974).

What remains defensible, however, is Skinner's more fundamental suggestion that we may not need a theory of underlying processes for the purpose of pursuing a transition theory of behavior (Skinner 1950). This indeed is the central thesis of Neo-Skinnerianism, which like many neo-isms, is an attempt to acknowledge the valid points of a rival tradition, while defending the core assertions of one's own.

Having separated the defensible from the indefensible, one can now make a plausible case on behalf of the potential of the Skinnerian program to contribute to our understanding of human behavior. The key question becomes whether we get more powerful causal generalizations by keeping to the environmental/behavioral level (as Skinnerians advise), or whether we do better to include underlying variables in our transition laws.

Before examining this question, however, let us pause to ask what the purpose of arriving at such laws might be. Skinner often speaks as if the purpose of behavioral psychology is simply to maximize prediction and control (Skinner 1953). Neo-Skinnerians such as Staddon (1983), however, tend to side with an earlier Skinner, who minimizes the extent to which prediction and control can be attained outside the experimental chamber, and who emphasizes the goal of understanding the behavior of the whole organism (Skinner 1938).

Providing an account of the behavior of the whole organism is something that cognitive psychology has not been notably successful at doing. Instead, it tends to divide the organism into subsystems which operate in a semi-autonomous manner, and to postpone indefinitely the integration of these subsystems into a whole organism. The framework provided by behavior analysis, on the other hand, affords (even in its early stages) an understanding of the behavior of the whole organism. If, for example, a recurrent pattern of modulations in the frequency of some response is attributed to the schedule by which a reinforcer is delivered, this says something about the net effect of the functioning of the entire organism. There is a level of understanding here which is not necessarily equivalent to an understanding of the operation of the various subsystems which make up the organism.

A similar kind of understanding is provided by the matching law and its many competitors, all of which imply that over an extended period, an organism will maintain a certain objectively definable relationship with its environment. The question of whether this relationship can be defined as optimizing the value of a certain parameter, or whether some other mathematical function is involved, is a hotly debated issue among behavior analysts. Whatever the outcome of this debate may be, the theories emerging from this controversy provide a profound (although perhaps mistaken) interpretation of what behaving organisms (including human beings) are up to. These theories are the major preoccupation of the Neo-Skinnerians.

Although it may begin to look like Neo-Skinnerianism is simply a return to Proto-Skinnerianism, there is at least one important respect in which it departs from both early and late Skinner. Its conception of the dependent variable is decisively 'molar'–i.e., it takes behavioral laws to be about the pattern of responding over a relatively extended period of time, and not about individual responses (Rachlin 1976). This view parallels an analogous feature of the theory of natural selection, according to which it never accounts for the properties of individual organisms, but only for the prevalence of a property within a population (Sober 1984). Skinner has consistently held that behavior theory is capable of providing a probabilistic explanation of individual responses. By contrast, on the molar view, behavioral laws are not about individual responses but are about a population of responses that occur over an extended period of time. For example, on the molar interpretation, behavior analysis is responsible for providing a subsumptive account of the proportion of responses devoted to a variable-interval 30-

second schedule when it runs concurrently with a variable-interval 15-second schedule (according to the matching law, there will tend to be a 1 to 2 ratio), but not for providing an account of why any particular response occurs. Operant theory, on this conception, is about what happens in the long run when an organism is exposed to a certain pattern of contingencies–i.e., when it is exposed to 'information' (in the Shannon/Weaver sense) about sources of reinforcement.

This shift in the conception of the dependent variable is crucial to the defense of behavior analysis against the following influential line of criticism, which attempts to show that the scope of behavior analytic generalizations is severely limited in the domain of human behavior.

> Consider any given response to a stimulus. Common sense (i.e., folk psychology) tells us that if the subject had had a sufficiently different set of beliefs or desires, then the same stimulus would have resulted in a different response. This implies that either behavioral generalizations must mention such underlying variables, or else their domain must be limited to restricted environments (Skinner boxes, prisons, factories, elementary classrooms, etc.) where the range of psychologically possible beliefs and desires is highly constrained.[5]

Whatever the strength of this argument against Skinner's interpretation of behavior theory may be, it does not mount a decisive objection to behavior theory on the Neo-Skinnerian interpretation.

If the organism is exposed to a certain type of contingency over an extended period of time, then (on the Neo-Skinnerian interpretation) behavior will adjust to that contingency over an extended period of time. Both the dependent and independent variables spread out across time. If certain beliefs would interfere with such long-term adjustments, then presumably such beliefs will tend not to arise, or if they do arise, their effect upon behavior will tend to be neutralized by other adjustments within the organism. What these adjustments might be, need not be specified by behavior analysis. Folk psychology implies that there are indefinitely many such potential adjustments, for the effect of any belief upon a given response can be neutralized by an incredible variety of belief/value combinations. There may also be adjustments at non-cognitive levels which contribute to the ability of the organim to do the sorts of things operant theory says it does. Neo-Skinnerians do not claim to know how these adjustments occur (even though, as noted above, Skinner himself does). They do not even claim to know whether there is one or many mechanisms responsible for such adjustments. They simply specify what will happen over the relatively long run if the organism is functioning properly.

An implicit assumption is that behaving organisms have been selected to adjust their pattern of responding in ways that bring about certain objectively definable environmental consequences. If this is not so, then behavior analysis is unlikely to be of broad applicability. On the other hand, if it is so, or to the extent that it is so, the behavior analytic program may be capable of giving a rather complete account of a certain type of dependent variable. For example, S.E.G. Lea (1981) has suggested that the reason schedules of reinforcement have proven to be such powerful independent variables is that rats, pigeons, and other experimental animals have been selected to solve the foraging problem of maximizing caloric intake, and schedules of reinforcement replicate this problem.

Some psychologists think human beings are an exception, that our complex behavioral dispositions do not ultimately serve the function of maintaining a certain relationship with the environment. I do not know if this is so, and I suspect no one else

does either. I hope, however, to have shown that we should not dismiss the possibility that human beings are, in this important respect, similar to rats and pigeons. If we are, this would have implications for our conception of human nature.

During the eighteenth century there was a lively debate between figures such as William Godwin and Antoine-Nicholas de Condorcet on the one hand, and Adam Smith and Edmund Burke on the other, about the extent to which human behavior is constrained by the availability of rewards and punishments. This was not a debate about the mechanisms underlying human dispositions, but about the dispositions themselves. The growth of cognitive science has been so dramatic, and is so recent, that we may be in danger of forgetting that many profound questions about human nature are simply about our dispositions, and not about the mechanisms that underlie them. Although such questions have practical implications, they are theoretical in the sense that only a theory can address them adequately. The major importance of Neo-Skinnerianism is that it provides the most promising strategy by which the Skinnerian program may do so.

Notes

[1]Cummins' choice of terminology is especially unfortunate for those of us who wish to use his concepts to explicate Skinnerian psychology. For Skinner uses "functional analysis" and "functional account" to refer to exactly the opposite type of explanation from that which Cummins uses these terms to refer to. This is to say, when Skinner speaks of a functional analysis, he means an account which subsumes an event under a transition law. Obviously, the potential for confusion in the use of "functional analysis" is high. Since my audience is more likely to be familiar with Cummins' usage than with Skinner's, I have chosen to adopt Cummins'.

[2]So far as I know, the term "Neo-Skinnerian" was first used in Dennett (1979), and has not been used since. I do not know whom he was thinking of when he used it, but the context of his usage leads me to believe that his intended meaning is consistent with my own. If I were to name the Neo-Skinnerians, I would start with R. Herrnstein and J.E.R. Staddon, and go on to list (in alphabetical order) G. Ainslie, J. Hinson, P.R. Killeen, J. Malone, Jr., H. Rachlin, C. Shimp, P.A. de Villiers, B. Williams, and many others, including the majority of those who work within the matching law tradition.

[3]As his defenders often point out, Skinner is not associationistic when engaged in behavior analysis. When he turns to the task of giving a functional analysis of behavioral dispositions, however, he reverts to a rather old-fashioned form of associationism.

[4]Dennett (1979) makes a related point in his explanation of why the homunculi of cognitive psychology are not problematical in the manner Skinner thinks. Once again, the point is that mentalism is apparently inevitable when we pursue functional analyses of complex behavioral dispositions.

[5]The most prominent example of this argument appears in Schwartz & Lacey (1982). The basic strategy of the argument, however, (although aimed at a different target) can be found in the refutations of philosophical behaviorism given by Geach and Chisholm in the late 1950's. This pattern of argument is now part of the common property of all analytical philosophers, and in this sense, Schwartz and Lacey have

stated in a very clear and well motivated manner an objection to behavior analysis that, in a less precise form, has surely occurred to many analytical philosophers.

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