The Continuing Fascination with the Inside Story: A Review of *Behavior and Its Causes* by T. L. Smith

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“What happened to the Skinnerians?” In the preface to *Behavior and Its Causes*, Terry Smith tells us that his book is a response to this question, asked in 1980 by the philosopher Dudley Shapere. In answering the question, Smith was surprised to discover that during the 1960s operant psychology had actually grown, contrary to the conventional wisdom among nonbehaviorists that it had virtually disappeared. Furthermore, it has grown at an accelerated rate.¹ Smith starts with the working assumption that operant psychology is unlikely to have grown as it did unless it had something important, and essentially correct, to say about behavior. But before going further, a quibble: Shapere referred to “The Skinnerians,” and Smith refers to “The Skinnerian program” (p. xi). The behavior-analytic program is, however, more than the corpus of Skinner’s work, as foundational and important as that work was and remains. I recommend substituting “behavior-analytic program” for “Skinnerian program.”

The book’s introduction is followed by four parts: “Understanding the [behavior-analytic] Program of Research,” “Circumventing Standard Criticisms of the Program,” “Weighing the Strengths and Weaknesses of

Radical Behaviorism,” and “Disentangling the Program from Radical Behaviorism.” This listing alone should suffice to whet one’s appetite.

Smith’s primary audience is philosophers and cognitive psychologists; operant psychologists are his secondary audience. Because of this, Smith devotes almost all of Part 1 to material that is familiar to operant psychologists (behavior analysts).

It is refreshing to read and talk with (as I have) a philosopher like Smith who has been truly interested in learning about behavior analysis. Also, unlike almost all philosophers and cognitive psychologists, Smith argues that operant psychology does have significant value.

Chapter 2 includes a sophisticated analysis of stimulus–response (S-R) psychology. Actually, all of chapter 2 is a rather sophisticated analysis of the difference between S-R psychology and the operant program. For some behavior analysts it may not be the elementary material that Smith assumes it to be. This characterization is also true of chapter 3, which is concerned with the functional nature of behavioral categories. It may not be elementary material because behavior analysts vary in terms of their formal (and informal) education in behavior analysis and radical behaviorism.

In chapter 4, Smith argues that behavior analysis is *not* incompatible with cognitive psychology. His assertion is that cognitive psychology tells us, or more accurately, issues a promissory note that it will tell us, what sorts of processes underlie behavioral phenomena. The question that occurs to the reader is why cognitive psychology, rather than physiology, is the

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¹ For a report on the relative strengths of the cognitive, behavioral, and psychoanalytic programs, see Friman, Allen, Kerwin, and Larzelere (1993) and responses by Robins and Craik (1994) and Salzinger (1994).

place to turn. Smith goes on to say that the turn to cognitivism should be accompanied by a rejection of radical behaviorism (not to be confused with behavior analysis). This is an issue to which I shall return.

Smith makes an important point when considering the criticism that the law of effect (better, the principle of reinforcement) is tautological. In the process of answering that criticism, he points out that the content of operant psychology (behavior analysis) lies not in the principle of reinforcement, but in the quantitative analysis of the principle of reinforcement:

although the property of functioning as a reinforcer cannot be verified independently of the occurrence of the response that is being reinforced . . . physical aspects of reinforcement (e.g., the length of delay of delivery of reinforcement) can be verified and measured independently of certain physical aspects of the operant response (e.g., the rate of operant responding). (p. 79)

It is these physical properties that are the independent and dependent variables of behavior analysis.

In chapter 5, Smith presents the folk psychology critique of behavior analysis. He says that among philosophers this argument has been very influential. Basically, the argument is that even if every particular behavior (effect) has a certain distal environmental cause, there is no guarantee that a regularity connects the distal environmental cause to its behavioral effect. What is often required to be taken into account is an intervening process. Instead of the distal-environmental-cause-to-distal-behavioral effect, "we may get only a regularity that says a certain distal [environmental] cause will produce a certain distal [behavioral] effect if the intervening process has certain properties" (p. 87).

Furthermore, in folk psychology, many of these intervening processes are intentional in nature, comprising persons' beliefs and desires. So in order to predict and control and understand complex human behavior, we must ordinarily take into account be-

liefs and desires. Changes in beliefs and desires "can cancel, reverse, or modify almost any environment-to-behavior tendency in any number of ways" (p. 95).

As Smith notes, others have also made this argument. The important question for behavior analysts is whether it can be satisfactorily answered, and in chapter 6 Smith outlines a rebuttal. It starts with folk psychology's dependence on intentional explanations that rely on such nonphysical causes as beliefs and desires. Folk psychology is then faced with the as-yet-unsolved problem of the relation between physical and nonphysical ontological spheres (see Baum & Heath, 1992, for more discussion of intentional explanations). Furthermore, Smith points out that the advocates of folk psychology have misunderstood behavior analysis, and when these misunderstandings have been removed, little is left. Smith describes these misunderstandings and shows how, once they are corrected, behavior analysis remains unscathed. One misunderstanding has to do with the dependent variable. Contrary to the critique, behavior analysts usually are *not* concerned with the occurrence of *one* response; rather, they are usually concerned with patterns of responding over time. Critics are also sometimes unaware that the independent variable also extends over time. Smith says that this is important, "because the tendency of folk psychology is to look at a particular response to a given stimulus and note that if the organism were to change a certain belief, then the same stimulus would result in a different response" (p. 104). But behavior analysts predict behavioral patterns based on the organism's exposure to the contingency (independent variable) for some duration of time.

Misunderstanding also stems from the failure of the advocates of folk psychology to distinguish between the functional categories behavior analysts use to describe the environment/organism system and the variables that are

the causes and effects connected by behavioral principles. As Smith says, an operant response is not itself a dependent variable, and a reinforcing stimulus or discriminative stimulus is not itself an independent variable. Rather, these are the descriptive categories that define the behavioral entities whose measurable properties are the variables.

Some critics of behavior analysis, according to Smith, also seem to be completely unaware that functional concepts play any role in behavioral generalizations. Dennett (1978) and Taylor (1964), for example, have asserted that behavioral generalizations can be refuted by the case in which an organism misinterprets its environment. Smith points out that this line of criticism completely ignores the role of functional concepts in behavior analysis. Functional concepts insure that the organism is maintaining a certain type of contact with the environment. But when it is not in such contact, the principles do not apply. The criticism that behavior analysis fails to adequately account for the behavior of normal adult humans in complex everyday settings is also addressed. Here Smith acknowledges that behavioral principles have limited predictive and control capacity. This limitation should not, however, be taken to imply that the principles are not valid in complex situations.

In chapter 7, Smith maintains that contemporary sophisticated philosophical critiques of behavior analysis question whether it is truly behavioral. The criticism is that behavior-analytic and mentalistic concepts are somehow logically connected. Smith attempts to distance behavior analysis from mentalism. There are (at least) three ways to do this: (a) Show that behavioral categories are not a subspecies of mentalistic categories. (b) Show that not only are behavioral categories not a subspecies of mental categories, they are not even logically connected with them. (c) Show that behavioral categories serve a different scientific pur-

pose from that served by mentalistic categories.

With respect to (c), the advantage accruing to behavioral categories over mentalistic categories is that behavioral properties can be measured independently of one another, but mentalistic properties cannot. And in Smith's view, "This independence of behavioral quantities from one another is one important benefit of doing psychology the behavioral way" (p. 123). It is an important benefit because it enables behavior analysts to more easily establish quantitative relations experimentally, and then to refine their understanding of them.

Smith also discusses (a) and (b), above. He analyzes examples that bolster the suggestion that behavioral categories are not subspecies of mentalistic categories. The difference between the categories is that mentalistic categories, involving beliefs and desires, have propositional content. For example, Bob believes that Neptune is the third planet from the sun and desires to graduate from college. Behavioral categories, in contrast, describe functional relations between the organism and the environment, and such descriptions do not have propositional content. Rather than saying that "The rat desires its lever presses to be followed by pellets," the behavior analyst says, "The rat's lever presses are reinforced by access to pellets." Smith shows that behavioral categories are not logically connected to mentalistic categories because behavioral concepts are functional. There is no logical implication that if a certain mental state changes, then so does the function of certain aspects of the behavioral system.

Part 3 is concerned with the strengths and weaknesses of radical behaviorism. In chapter 8 ("What Is Radical Behaviorism?"), Smith says that radical behaviorism does not imply mind-body identity, although it eliminates the mental, so radical behaviorism is not dualistic. According to Smith, the key to understanding Skin-

ner's view is in Skinner's analysis of terms:

When he says that subjective states are states of our bodies, he means only that these are states of our bodies in the same sense that stimuli and responses are states of our bodies—i.e., states that are functionally defined. This means they will not be individuated as brain states—on the basis of their physiological properties—any more than a generic stimulus or response would be. (p. 142)

It is not Smith's purpose to clarify his own ontological position here, but his advocacy of intentionality and its attendant categories of beliefs and desires implies some view other than monistic materialism.

Most of this chapter is devoted to clearing up some of the many misunderstandings about radical behaviorism. An extremely important point is made in the process of doing so. Smith quotes Skinner as asserting that, "The issue between behaviorists and mentalists is 'primarily empirical rather than logical' (Skinner, 1972, p. 555) and must be resolved by the experimental analysis of behavior" (p. 140). This, it seems to me, describes an extremely important divide between most philosophers and behavior analysts. Philosophers are interested in, and give great weight to, argumentation (in a nonpejorative sense), whereas behavior analysts are interested in, and give great weight to, empirical evidence. Not that behavior analysts disregard argumentation or that philosophers dismiss evidence, but the relative weights given to argument and evidence by both groups are sufficiently different to demarcate a qualitative difference between them. For example, Smith says that Skinner "deftly pushes the debate out of the philosopher's study and into the psychologist's laboratory. For despite the fact that Skinner rarely draws the connection explicitly, the principal support for radical behaviorism comes from his own [and others'] program of research" (pp. 147–148).

Chapter 9 concerns "The Scientific Case For Radical Behaviorism." Again the question is whether a full account

of behavior requires reference to mental states and processes, as cognitive psychologists maintain. Smith says that it is here that the scientific case for mentalism is strongest. Taking schedule effects as an impressive example, Smith argues that Skinner's goal was to account for these effects without having to appeal to mental processes. The ability to do this would constitute a strong challenge to cognitive psychology. Unfortunately, the chapter concludes on a pessimistic note. According to Smith, the earlier optimism was unwarranted; little progress has been made. There is said to be some debate among behavior analysts about the potential usefulness of references to underlying mentalistic states and processes. Smith draws an important conclusion:

radical behaviorism, insofar as it prescribes specific research priorities, has reached a dead end. This goes far beyond the typical situation in which a scientific theory faces difficult unsolved problems. The effect on behavior analysts has been a sense of profound frustration. (p. 168)

Smith presents two solutions. One is to turn to cognitive theory. The other is to shift the focus of research by using schedules as independent variables that can be synthesized into larger wholes or can interact with other variables. But in either case, according to Smith, it is clear that radical behaviorism can no longer count on empirical support from behavior analysis. The upshot, Smith says, is that the defense of radical behaviorism is no longer closely tied to the behavior-analytic program of research.

In chapter 10, Smith says that by the 1980s Skinner's analogy between natural selection and operant conditioning had become his principal source of support for radical behaviorism. This was an important move, says Smith, because it implies a strategy for the development of radical behaviorism. The analogy provides a means of revising the content of radical behaviorism, thereby making it possible for the philosophy itself to evolve. Smith concludes that the analogy has two prin-

cial uses: (a) to justify radical behaviorism and (b) to revise it. These may enter into a dialectical process "that goes back and forth between the two, searching for an equilibrium point at which an empirically well-supported theory of learning justifies a philosophically productive conception of radical behaviorism" (p. 175).

The analogy with natural selection is said by Smith to serve the function formerly served by the theory of schedule effects, so that appeals to mental (cognitive) processes are unnecessary. But Smith argues that for conditioning to be analogous to natural selection, conditioning must encompass three key features, and that it is very difficult to defend those features empirically. The features are (a) small and undifferentiated variations, (b) nondirectionality of variation, and (c) blind reinforcement. The problem with the analogy, according to Smith, is that there are many experimental results that are inconsistent with the selectionist interpretation of conditioning. Each of the three features has been successfully (in Smith's view) attacked. Smith concludes that radical behaviorism is either less consistent, or less profound, than it first seems, and that indeed it may be both.

In Part 4, Smith is interested in "disentangling" operant psychology from radical behaviorism; indeed, of having operant psychology transcend radical behaviorism. This is because operant psychology, in Smith's view, can no longer persuasively claim to account for behavioral regularities by reducing them to more basic behavioral principles. Rather, there must be a recognition of the necessity of referring to underlying cognitive processes. Smith cites evidence (e.g., Lowe, 1979) that the fixed-interval scallop disappears from human behavior at about the age of 4 or 5 years as seriously undermining the view that contingencies of reinforcement are ubiquitous features of behavior—nonhuman *and* human. The acceptance of the cognitive view is the acceptance of the notion that there are

more factors that must be taken into account when explaining behavior (particularly human behavior) than are encompassed by behavior analysis. It is because of these additional factors that a human subject will not necessarily generate the same results in the same controlled setting that a nonhuman does.

In arguing for the cognitive turn, Smith, following Cummins (1983), distinguishes between two types of scientific theory. Transition theories explain changes in a system as effects of causes. Property theories explain changes by arguing that causes have their effects through certain processes instantiated in certain systems, including humans. Smith goes on to say that for any learned pattern of behavior, two quite different types of explanation are possible: a causal account (i.e., transition theory) of why the pattern developed, and a reductive account (i.e., property theory) that argues the pattern to be the result of a more basic process. This division is exemplified by the attempts of Chomsky (1980) and Skinner (1957) to account for the ability to distinguish between grammatical and ungrammatical English sentences. Skinner's transition theory attempts to identify what causes a child to acquire this ability. Chomsky's property theory, on the other hand, asks what process underlies the child's acquisition of this ability. There is a division of labor. In this respect, the two accounts are complementary, not competitive; they attempt to answer different questions.

In chapter 11, Smith says that "Contemporary behavior analysts . . . view aspects of human learning as different in kind from animal learning, and see a need to explore qualitatively different functional relations than one finds in animal research" (p. 200). The different relations that Smith cites are instructional stimuli and rule-governed behavior, and those involving establishing stimuli, as well as equivalence classes. It seems to me that we have here the constituents of a promissory

note in competition with those of folk psychology and cognitive psychology. Smith, however, does not see it that way. In his view, the recent growth of the behavior-analytic program has been limited, in contrast to cognitive psychology, which has made rapid progress in accounting for complex learned behavior on the basis of underlying cognitive processes. Behavior analysis, on the other hand, has not, indeed cannot, do this.

Earlier research on schedules of reinforcement generated a collection of facts not easily related to one another. But with the matching law, operant psychology entered the realm of theory. "For the first time one can compare with some precision the relative contribution of two or three different factors to *intentional* behavior" (p. 205, emphasis added). In Smith's view, the theory of operant psychology must be cognitive, encompassing intentional idioms, and this is being recognized by behavior analysts. He maintains, for example, that "Many behavioral psychologists now concede that reference to cognitive mechanisms is necessary to provide explanations of behavioral regularities" (p. 215). But Smith's conviction about this is stronger than the evidence. For instance, I searched the key words of all the empirical, theoretical, and review articles in eight issues of the *Journal of the Experimental Analysis of Behavior (JEAB)* published in 1993 and 1994. Of the 85 articles, only seven included one or more cognitive terms in the listing of key words, hardly indicative of a floodtide of cognitivism. Importantly, when a putatively cognitive term (e.g., "remembering") was used, it was described in a behavior-analytic, not cognitive, framework. An important caveat is provided by Higa and Staddon (1993) in their study of "transitive inference" in pigeons (note the quotation marks in the title of their article):

we argue that by considering the task [i.e., of transitive inference] as a measure of inference based on the assumed ordering of stimuli (without providing a mechanism for how the ranking

develops), rather than as just another kind of choice or preference task, there is a danger of minimizing the importance of procedural details. There is much less danger of minimizing the importance of things like training schedule, how preference is to be measured . . . and what effect the particular training history might be expected to have on the simple tendency to peck on the . . . different stimuli, if we regard the task as just another kind of choice procedure (p. 267).

Higa and Staddon conclude their article by again referring to the necessity of knowing the organism's relevant history: "We should study the historical effects of training and carefully examine variables such as the presentation order of stimulus pairs and reinforcement contingencies, as well as the role of nonreinforced responses" (p. 291). Nothing cognitive here. Other terms in the *JEAB* articles that at first glance may seem to refer to cognitive processes do not do so when considered in the context of the articles in which they appear.

In chapter 12 ("Operant Psychology Without Behaviorism"), Smith argues that for any given learned pattern of behavior, there are two quite different types of explanation. One type of explanation provides a causal account of why the pattern arose. The other, reductive, type of explanation argues that the pattern is the result of a more basic process. These are the transition and property theories discussed above. So we seem to have a division of labor, with operant psychology providing the first type of explanation and cognitive psychology the second. Operant psychology and cognitive psychology are thus complementary.

They are also, however, in competition with one another. Both operant psychology and cognitive psychology attempt to provide subsumptive explanations of everyday human behavior. Smith believes that operant psychology has two advantages in this competition, which gives it a virtually insurmountable edge in accounting for ordinary human behavior.

One of operant psychology's advantages is epistemological. This derives from the fact that behavioral units have

quantitative properties that can be measured directly. "The quantitative aspects of cognitive entities, on the other hand, can be measured only indirectly by making certain assumptions about the quantitative values of other psychological states" (p. 217). Such circularity poses real difficulties for cognitive psychology.

The other advantage accruing to operant psychology, according to Smith, is logical, and in his view, this is the more important one. The advantage is that behavioral concepts are capable of defining regularities that satisfy the principle of compositionality. This means that "when more than one behavioral cause exerts influence over the same variable, it is (at least theoretically) possible to calculate the net effect" (p. 218). The primary example of this is the matching law. The law tells us, for example, the degree to which a delay of reinforcement for one response will tend to shift responding to alternative responses. The law also tells us the degree to which an increase in relative frequency of reinforcement for a response will have the opposite effect.

The causal tendencies imparted by changes in delay and frequency of reinforcement maintain their validity when they interact, and so they can be composed into a net result that tells us how much of an increase in frequency of reinforcement will be necessary to offset a given increase in delay of reinforcement. (p. 218)

It is not likely, however, that cognitive principles can be composed this way. Unless one has a complete description of a person's system of beliefs and desires, prediction is unreliable and explanation is incomplete. But a complete description of any person's system is probably impossible.

In the last two sections of the book's last chapter, Smith briefly considers freedom and responsibility, self-control, and ethics. His thesis here is that radical behaviorism, unlike behavior analysis, has not been helpful. Contra Skinner, radical behaviorism does not resolve the issues of free will and responsibility because the philosophical

problem is not centered on the choice between naturalism (e.g., radical behaviorism) and antinaturalism. "Rather, it is to solve a series of puzzles about the implications of naturalism for ethics. So it is odd to find Skinner asserting that since he has discovered that behavior has natural causes, it follows that we should abandon the ideals of freedom and responsibility" (p. 226). Smith also faults Skinner for failing to give serious consideration to compatibilism, the doctrine that freedom and responsibility are compatible with naturalism. Self-control and ethical behavior can, Smith says, be accounted for and developed by behavior analysis without any need to refer to the tenets of radical behaviorism. This part of the book is not clearly tied to what has preceded it. Indeed, it could probably have been omitted.

I take issue with Smith on two of his fundamental theses: (a) that behavior analysis must incorporate cognitive idioms, and (b) that radical behaviorism is independent from behavior analysis (operant psychology) and that radical behaviorism impedes the development of behavior analysis and thus should be dispensed with. No persuasive evidence is presented that behavior analysts must adduce cognitive processes in order to account for behavior, including complex human behavior. Labeling such behavior with cognitive terms and appealing to unobserved, hypothesized processes have not been shown to advance cognitive psychology vis-à-vis behavior analysis. There is also the question of just what are cognitive processes. Smith and other philosophers of an intentional bent write about beliefs and desires. Most cognitive psychologists, however, *do not* refer to beliefs and intentions. It is not made clear by Smith just how beliefs and desires and other cognitive processes are to be conceptualized. It seems to me that either they are physiological or they are not. If they are physiological, then their conceptualization and explication should be in physiological terms. If they are not physi-

ological, they must nevertheless be explicated in some terms. But what terms? These issues are not discussed.

In one sense I agree with Smith that radical behaviorism is independent from behavior analysis. No doubt, one can find behavior analysts who do not subscribe to radical behaviorism. One could, for example, be an applied behavior analyst working with clients and being concerned solely with bringing about behavior change. At the same time, however, I am not persuaded that Smith has made the case that radical behaviorism has impeded the development of behavior analysis. As Smith points out, the evaluation of behavior analysis is not done in a vacuum; it is a comparative process. And in the comparison with present-day alternatives, behavior analysis holds up well. Behavior analysis is where it is in part because of radical behaviorism.

In conclusion, Smith has advanced one of the better arguments against the radical behavioral program. I remain, however, unconvinced that behavior analysts need to make the cognitive turn. It is still at least an open question whether the cognitive or behavioral program will cash out its promissory note. I bet on the behavioral program. And amid much uncertainty, there is one thing that can be accurately predicted—the conversation will continue.

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