*BETWEEN ORGANISM AND ENVIRONMENT.
A REVIEW OF B. F. SKINNER'S ABOUT BEHAVIORISM¹*

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The reviewer of a book is under the control of the text he examines, and of his own past history. While the text is common to all readers, the dispositions consequent to the reviewer's personal history are unquestionably unique. The histories of those who review books by Skinner seem to lead to one or the other of two distinct patterns of response—the frankly commending (MacCorquodale, 1969; Day, 1972) and the unforgivingly displeased (Chomsky, 1959; 1971). From the outset, this reviewer confesses to the former reaction. Still, there are points to Skinner's newest book that evoke comments beyond reiteration and routine praise.

To readers familiar with Skinner's previous work, there will be few surprises in *About Behaviorism*. Repeatedly, one covertly remarks, "That is from 'The operational analysis of psychological terms'", or "He says that better in 'Behaviorism at fifty'". The bibliography begins with references to Skinner's previous work on 15 selected topics. *About Behaviorism* is a summary statement of the philosophy of a science of behavior that has appeared before, distributed through many volumes. Now we will have it in a concise and accessible form. Of Skinner's previous books, this one resembles *Science and Human Behavior* (1953) most of all. Like that earlier book, it has a beguiling simplicity. It is again an error to read rapidly, however—the simplicity is only sentence-structure deep. Changes have occurred over the 21 yr separating the books. The autonomic-respondent invariance has been abandoned, or at least highly qualified. That is ironic, as Miller now reports an inability to replicate the experiments that

must have influenced the change in Skinner's position (Miller and Dworkin, 1974). Rule-governed behavior, which was mentioned hardly at all in *Science and Human Behavior*, now is discussed extensively. The rules in question are explicit, not like the curious rules of Chomsky that are supposed to be, as Quine (1970) said, "heeded inarticulately". Structuralism is addressed in several places with indirect references to Levi-Strauss, Piaget, and Chomsky. Readers will note many more evolutionary changes, but nothing radically new.

As with *Science and Human Behavior*, the new book is directed toward a college audience, and an inexpensive college edition has been published concurrently with the hardcover edition. While it is probably not appropriate to use the book as an independent text, as *Science and Human Behavior* was intended, it can serve a valuable role in conjunction with a text in the science of behavior. An introductory course in the analysis of behavior using *About Behaviorism*, Rachlin's *Introduction to Modern Behaviorism* (1970), and Patterson's *Families* (1971) is appealing to the reviewer. Others will undoubtedly find different uses for it in their own curricula.

While the book is ostensibly a response to criticism, that seems to be an artful device, rather than the basis of the text. The criticisms are stated in the introduction and their inadequacies are explained in the fourteenth chapter. The criticisms are typical of what appeared in the numerous reviews following publication of *Beyond Freedom and Dignity* (Skinner, 1971). Charges of scientism, dehumanization, and authoritarianism are characteristic. Criticisms stemming from a philosophy of science perspective are not dealt with. Between the introduction and the fourteenth chapter of the book, the philosophy of radical behaviorism is developed without a hint of apology. The book is not a defense. Skinner seems to be saying:

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"Read this carefully and the criticisms dispose of themselves." When the criticisms are returned to in the end, they have not merely been answered, they have been overpowered.

The significance of *About Behaviorism* lies not in its response to criticism, but in the positive statement it provides concerning the philosophical underpinnings of the experimental analysis of behavior. That analysis is "a rapidly advancing branch of biology" (p. 231) concerned with "the relation between organism and environment" (p. xii). It may be divided into two parts, one concerned with the evolutionary environment of the species, and one with the lifetime environment of the individual. The former concern is central to ethology and the latter to the experimental analysis of behavior, although they are clearly interdependent. By the philosophy of this science, Skinner means at least three things. The foremost task of the philosophy of a science must be a clarification of "the nature and purpose of a scientific analysis" (p. xiii), but Skinner also includes in the philosophy a "critique of traditional theories" (p. xiii) and a behavioral "interpretation" (p. 19) of problems extending beyond the limits of prediction and control. Because *About Behaviorism* has been written for an audience more general than experimentalists, the text has been organized around the latter two concerns. Views on the nature and purpose of a scientific analysis are found throughout the book, and have been developed in chapters with titles as diverse as "Perceiving", "Thinking", "Knowing", and "Causes and Reasons". I believe that Skinner's account of the nature and purpose of a scientific analysis of behavior may be summarized by two major points: the analysis is causal or functional rather than structural, and it is made at a consistent level, rather than at multiple levels referring to different kinds of event or entity. The most unique product of the convergence of these two principles comes about in the analysis of private events. These three points, then, will be the concern of the remainder of this review.

FUNCTION RATHER THAN STRUCTURE

Skinner's analysis of behavior is so inclusive and internally consistent that it is difficult to determine the proper point of entry. A scien-

tific analysis is itself behavior, and a clarification of the nature and purpose of that analysis is further behavior. Each in turn is subject to additional analysis. Science began, according to Skinner, when people determined laws that enabled them "to behave effectively" (p. x). The early planter prying a stone from his field with a pole learned through natural contingencies that when the pole was held near the end it levered better than when it was held near the fulcrum. Scientific laws "describe contingencies which prevail in the environment quite apart from any deliberate human action" (p. 124), as the contingency of the lever holds independent of anyone prying up a stone. Natural science is thus a sort of behaviorism of the inanimate, where natural contingencies are described by analyzing the behavior of objects as a function of their circumstances. When early man's contingency-shaped behavior led him to devise the law of the lever, the law could be applied in "situations where contingency-shaped behavior was unlikely or impossible" (p. 124). A water wheel will do more work if its size is increased along the spokes instead of along the axle. Much effort was saved when man was able to solve that and similar problems by reference to general principles, instead of relying on the repeated shaping of his behavior by natural contingencies. Men who devised such laws were more likely to survive due to further contingencies of environmental origin.

The nature of man's interaction with his environment is a functional one, and it follows that the most useful things man has learned of his environment have been of a functional nature. When man approaches his own behavior as a subject for scientific analysis, he starts in the same way, and presumably with the same concerns. The analysis of behavior is thus functional, directed at a description of the contingencies of behavior. The organism, a product of an evolutionary history (unanalyzed for the moment), exists within an environment. In the most interesting case, the organism acts and the environment responds in such a way that the probability of the organism's repeated action is altered. The probability is altered because the organism has been altered; in an important sense it is a different organism from what it was before the episode. When the organism acts again, it is said that its new behavior is a function of its past history, of the

prior episode, and of others like it. A functional relation has been identified when a change in one variable (independent) results in a change in a second variable (dependent), regardless of temporal separation. That a temporal gap intervenes between the historical causes and the behavior functionally related is no reason for alarm. Behavioral functions simply work that way.

Much criticism has resulted from a misunderstanding of the nature of functional relations in the analysis of behavior, and further clarification in *About Behaviorism* would have helped to counter these misconceptions. When responses occur serially, for instance, it is usually a mistake to consider them chained, in the sense of prior responses causing subsequent ones. Intrasubject responses are more typically dependent than independent variables in a functional analysis, even when occurring in orderly sequences. Thus, the rapid responding seen on a fixed-ratio schedule is more accurately attributed to causal variables affecting the probability of all like responses (*i.e.*, schedule indifference to interresponse times, a fixed behavioral requirement, and the response-probability-increasing effect of reinforcement—the first two of environmental and the third of phylogenetic origin) than to a special kind of relation between responses. Perhaps there has been an evolution regarding this point since 1938, when a law of chaining appeared (Skinner, 1938). The traditional account of chaining, where prior responses are discriminative and following ones reinforcing, is at best an empirical hypothesis to be evaluated on every occasion. While it may be accurate in some places and not so in others, it was *never* proposed as the account of verbal behavior (Skinner, 1957). The recent assertion that Skinner views a sentence as a "behavior chain, each element of which provides a conditioned stimulus for the production of the succeeding element" (Fodor, Bever, and Garrett, 1974, p. 25) is remarkable for its lack of scholarship.

When dealing with the evolutionary history of the species, Skinner again leaves temporal gaps, rather than mediating them with instinctual mentalisms or physiological hypotheses. These gaps span not moments, days or years, but millennia. The initiate to behavioral analysis may have great difficulty attributing causal efficacy to some inchoate environmental

selection occurring millions of years ago when he wonders, for instance, why birds migrate. It is easy enough to see how contingencies of survival maintain current migratory behavior, but the earth's geographical stability apparent to the casual observer does not seem to provide the shaping contingencies necessary to have established extended migratory repertoires. Earth scientists who have arranged their environments in a way to be responsive to the historical geographical record have found that apparent stability to be an illusion, however, when considered over time periods of sufficient magnitude to shape behaviors through contingencies of survival. Plate tectonics, an example of the kind of theory Skinner finds both inescapable and acceptable, provide a natural shaping mechanism for movement over geographical distances, and Skinner has suggested how it applies to the migration of North Atlantic eels and of the Brazilian green turtle (Skinner, 1969, p. 217). The formation of the Atlantic over the past 150 million years has proceeded at a rate increasing a migratory response requirement by a few meters at most in the lifetime of any individual. Bird migration may be related to historical climate changes rather than geographical change, or to a combination of both. The possibilities for interaction between the earth sciences and the analysis of species-typical behavior is intriguing, and that interaction will no doubt increase in the years to come.

Skinner does not suggest that the evolutionary environment operates on current behaviors across time like a manipulating hand. A reconstruction of evolutionary history tells why the observed behaviors are now seen, but these behaviors are controlled in additional ways by the current environment of each individual. Thus, different species of migrating birds may respond to celestial stimuli or to the magnetosphere of the earth. That the controlling relation has not been acquired in the manner of an operant discrimination does not exempt it from an experimental analysis, and in fact the analysis is going on. Attribution of the behavior to a "migratory instinct" will only muddle things.

The contributions of phylogeny to behavior are frequently recondite, and yield more slowly than ontogenetic contributions to experimental analysis. Phylogenetic effects sometimes appear unexpectedly, where the analysis had

seemed complete, and stimulate further investigation (e.g., the autoshaping phenomenon). The analysis of many complex patterns of human behavior has just begun. Skinner cautions not to ignore possible phylogenetic contributions to areas as diverse as aggression (p. 36), mothering (p. 197), and verbal behavior, where such contributions must have evolved "for other reasons" (p. 217). He repeatedly suggests that what may be inherited are not preprogrammed bits of behavior but dispositions to be reinforced in certain ways. The imprinted duckling does not innately follow, but instead *any* behavior that results in greater proximity to the mother is reinforced (p. 41). The typical environment of the duckling has confounded that principle with following for millions of years, but an experimental analysis has isolated the actual controlling variables. These suggestions should be of heuristic value to many students of animal behavior.

Neither control by the evolutionary nor the lifetime environment requires invention of a causal nexus, association, mediator, memory store, or central representation to fill time until behavior occurs. Physiology will someday tell us "what is happening inside the behaving organism" (p. 215), whether it behaves as a consequence of its species history or of its individual history. By acknowledging processes and structures that intervene between earlier events in species and individual histories and current behavior, Skinner relieves the uneasiness of those whose notion of causality is excessively Humean. Of course there is a contiguity, somewhere, but it is outside the domain of a science of behavior. Skinner's acknowledgment of the relation between physiology and behavior emphasizes the unity of science, however, in spite of the conceptual independence of the experimental analysis of behavior.

A functional account of behavior is desirable because it allows prediction and control, which are the essence of effective action. The structural account, on the other hand, is said to be ahistorical and topographical. Prediction is possible only in the weak sense that "what people have often done they are likely to do again" (p. 11). Unique behaviors may also be predicted through extrapolation from structural "universals", but specifics concerning when or where are not possible. When time

is invoked as a weak functional variable, developmentalism results. Time, however, is to be endured rather than controlled, and little progress results from the basic developmental model. Compared to a functional account, what we see is a "systematic neglect of useful information" (p. 12).

The structuralist Noam Chomsky has been quoted: "But how does a person who has acquired systems of knowledge and belief then proceed to use them in his daily life? About this we are entirely in the dark . . ." (Fodor *et al.*, 1974). The "systems of knowledge" are structural rules capable of transforming "Aaron hit a homer" to "A homer was hit by Aaron", and the like. Indeed, the rules do have nothing to say concerning the occasions when "Aaron hit a homer" might be uttered. Chomsky recognizes the limitation on his own theory but regrettably fails to see that everyone else is not so much in the dark. Still, the linguist says much about verbal behavior that may be of interest to the behaviorist (e.g., Catania, 1972), and it will no doubt be wise for the behaviorist to keep a watchful eye on developments in structuralist theory.

The interaction between linguistics and developmentalism over the last 15 yr has resulted in an enormous body of research, recently summarized by Brown (1973). For a decade, utterances were recorded in long, undistinguished lists. Accumulations including entries such as *all broke*, *boot off*, *Papa away*, and *beep beep* then were analyzed to determine underlying structural principles. It was not until Lois Bloom's dissertation (1970) that attention was directed at the nonlinguistic context of the child's utterances. The observation that "Mommy sock" was uttered both when mother was putting on Kathryn's sock and when mother's stocking was in sight made it clear that a single structural representation of the utterance was not possible. However, the message of a controlling environment has still not been apprehended, and thus a new wave of semantic structural analysis begins to supplant the fading syntactic structural analysis in developmental psycholinguistics.

A CONSISTENT LEVEL

Some of the most incisive writing in *About Behaviorism* concerns the usefulness of speaking about an event in terms of other events of

the same type, and the uselessness of much talk going off in other directions. It is tempting to describe the level of analysis as observational, but reference is made to unobservables when talking about an evolutionary environment, while physiological observations are avoided. The level of discourse is just the one that most directly addresses the interaction between organism and environment. There is no room in the analysis for the reductive language of physiology or the seductive language of mentalism. Most frequently, Skinner develops his case through a critique of and alternative to mentalism, and he does so in two ways. He analyzes the controlling variables in ordinary language making reference to mentalistic terms, and he repeatedly demonstrates the weakness of scientifically presented mentalisms.

When a person says something like, "I want to go" (p. 52), he does not describe an inner entity called a "want". Instead, he is describing his probability of going, or better, the utterance is just a function of events correlated with his probability of going. The mentalistic aspect of the utterance is entirely gratuitous. Offering a translation, Skinner suggests, "I feel now as I have felt in the past when I have gone". Thus, private stimulation partially controls the kernel tact "go", and "I want" is a descriptive autoclitic according to the analysis of verbal behavior presented elsewhere (Skinner, 1957). Many more analyses of ordinary language mentalisms occur in *About Behaviorism*, and frequently they are pithy and aphoristic: ". . . when we say, 'I have an idea; let's try the rear door; it may be unlocked,' what is 'had' is the behavior of trying the rear door" (p. 53). Many of the pleasures of the book are provided in this way.

Skinner has called a quantitative science of behavior "a sort of thermodynamics of the nervous system" (Skinner, 1938, p. 432), and in *About Behaviorism* he describes cognitive psychology in the same way (p. 217). In both the experimental analysis of behavior and cognitive psychology, general laws descriptive of actions of the nervous system are established without direct or indirect reference to the nervous system. Neither behaviorism nor cognitive psychology make the mistake of physiologising in the absence of physiological data, but radical behaviorism eschews any model beyond quantification of observed rela-

tionships, whereas the essence of the cognitive approach is just that model building. Skinner has said that behavioral laws can never be disproved by physiology; in fact, these laws lay a program before physiology (Skinner, 1938, p. 432). At the same time, cognitive psychology does a disservice to physiology by using metaphorical terms that prove illusory when pursued at the physiological level (p. 211).

Skinner repeatedly criticizes the terms of a cognitive approach, and he does so in a way consistent with his critique of ordinary language mentalisms. Mentalistic terms say no more *about behavior* than can be said through a careful statement at the observational level—usually they say much less—and their surplus meaning is invariably misleading. The metaphor of "storage" seems to rankle more than others. Skinner suggests that the human penchant for copying things physically has led to a metaphorical extension of the practice in cognitive theory. Mental copies must be put somewhere, and thus are stored and later retrieved in ways reminiscent of practices employed in libraries, warehouses, postal systems, and so on (pp. 108-9). Skinner's alternative is that the performance of behavior requires neither the prior storage of that behavior nor of the contingencies responsible for it. Instead, the individual as a behaving system was changed when behavior was acquired, and the changed system is all that is "possessed" (p. 137). How the system was changed is a matter for physiologists to discern; that the system is changed states a fact but not a useful one for the behavioral analysis. The contingencies themselves must be analyzed, and there is no way of short-circuiting the analysis through the elaboration of a mental metaphor. Thus, Skinner's analysis cuts away metaphor, leaving one with the observations on which the metaphor is constructed.

These metaphors are not only unwarranted but dangerous because, like a structural account, they lead to the systematic neglect of useful information. Theorizing turns one away from the personal and genetic environment before contributions from those sources have been effectively analyzed. Skinner provides many examples. He discusses Kohler's classic banana-fetching observations, which contributed to the insight doctrine, and concludes with a number of conjectures on what the chimpanzee had to do, and what, by impli-

cation, Kohler failed to observe. "To solve such a problem a chimpanzee must have learned at least the following: to stop reaching for a banana out of reach; to stop reaching with short sticks; to discriminate between long and short sticks, as by using long sticks to rake in bananas successfully; to pick up two sticks in separate hands; and to thrust sticks into holes. With this preparation, it is not impossible that in that rare (but poorly authenticated) instance the chimpanzee stuck one stick into the hole at the end of another and used the resulting long stick to rake in the banana" (p. 113). A similar critique is provided for the acquisition of an arithmetical concept (p. 107), and for the development of verbal behavior (p. 100). In each case, the cost of ignoring a consistent emphasis on prediction and control is exemplified.

It is not a novel observation that the theoretical terms of cognitive psychology, or any theoretical area of science, are metaphorical. That is widely acknowledged throughout science, and philosophers of science are virtually unanimous in assigning a crucial role to metaphorical terms in scientific theory. But according to Skinner, not only is metaphor a danger, theories are constructed by scientists "about things which they have not yet adequately observed or analyzed" (p. x). When philosophers have dealt with Skinner's approach, their comments have been predictably negative. For example, Maxwell (1962) told the story of a scientist Jones who invented the theory of crobes to explain the transmission of infectious disease. From the theory followed the useful deduction that disinfectants could be used to destroy crobes before infection, reducing the incidence of disease. Crobes were not observable in principle because the microscope had not yet been invented, and much controversy occurred among philosophers who were concerned about the status of the theoretical term "crobe". It was said that a psychologist of the time called Pelter would have much preferred "all of the observation laws relating to disease contraction, transmission, *etc.*, without introducing superfluous links (the crobes) into the causal chain" (Maxwell, 1962, p. 6). Maxwell's implication was, of course, that that would have been ridiculous, and needed no further comment.

The story does not mesh well with Skinner's reading of medical history. He points out that,

until recently, medicine has often been characterized by cruel and harmful practices perpetrated on the ill and infirm. The physician has been in the best position to make relevant observations on the variables leading to illness, and to discover practices with curative effect. Yet, blood letting and similar treatments were not under the control of the contingencies of illness and recovery; rather "*they were based on theories of what was going on inside the body of a person who was ill*" (p. x). The behavior of physicians was not significantly under the control of the natural contingencies of infection and disease, but under the control of traditional texts and lore. A careful functional analysis of disease processes was not often in evidence until the nineteenth century and progress was made in medical science when a functional analysis of disease began, not with the origin of theory.

The processes through which "obscure properties of nature" come to control human behavior are not to be discovered through hypothetical arguments, but through a careful analysis of historical cases (p. 106). Semmelweis' work on childbed fever may be reviewed here to advantage, because it parallels Maxwell's story of the crobes but leads to quite a different conclusion (see Hempel, 1966). Semmelweis did not set about his task by inventing possible theoretical causes of infection but by carefully evaluating all the current theories of the cause of childbed fever, such as crowding in the wards, rough examination by medical students, access of priests to the sick-room, delivery position, and so on. His method was to observe carefully the relation between the putative causes and actual incidence of childbed fever. The theories were systematically rejected. When an acquaintance died after a scalpel cut inflicted during an autopsy, Semmelweis reasoned that "cadaveric matter" could have been responsible. If he had experienced scalpel cuts without ill effect when the scalpel had not recently been used on a cadaver, then it is plausible to reconstruct his reasoning as nothing more than a functional analysis. He generalized to the case of childbed fever, where the unwashed hands of delivering physicians, rather than a scalpel, might contact tissue susceptible to infection. If foreign matter on the hands was responsible for childbed fever, then removing it by washing with a solution of chlorinated lime should reduce the

incidence of childbed fever. Semmelweis' investigation of this variable had good effect. It might be argued that cadaveric matter was logically equivalent to Maxwell's crobes, but it is Semmelweis' behavior in devising a preventive measure that should not be overlooked. The functional relations between antecedent events and disease came to control his behavior because Semmelweis conducted his research in a way so that appropriate control was probable, and it is unlikely that the history of medicine will disclose major contributors who did not conduct themselves in a similar way.

The philosopher's or logician's reconstruction of science is usually inaccurate as a functional analysis (p. 236), although it may serve in some ways as a structural account. Philosophers of science have in most cases dismissed the context of discovery, the behavioral process by which scientists establish their claim to having learned something about nature, as irrelevant to their concern for a logical analysis of scientific knowledge. In the behavioral account of scientific discovery, the scientist exposes himself to a variety of contingencies, both natural and verbal, in ways designed to make his eventual verbal behavior more precise. If scientific laws are the description of those contingencies prevailing in the environment, then the contingencies described in the theory of crobes do amount to the "observation laws relating to disease contraction, transmission, *etc.*" The term "crobe" is not directly related to the natural contingencies but to additional verbal contingencies in the scientist's environment and past history. If invention of the microscope shows crobes to have "reality status", the behavioral analysis will not be refuted, although it will be altered to take account of the new controlling variables. When crobes enter into natural contingencies affecting the scientist directly, they may explain the mechanism of the observation laws relating to disease. But they do not alter those laws or explain them away, just as a future knowledge of physiology will explain the mechanism of, but will not explain away, behavioral contingencies. The role that metaphorical terms may serve in a structural account of science, then, does not necessarily indicate their place in a functional analysis of scientific behavior.

It should be noted that a science pursuing a functional analysis by reference to environment alone will inevitably encounter cases

where a grain of behavior may be specified and changes in the behavior observed, but (current) environmental causes cannot be determined. In 1938, Skinner identified the level of a behavioral analysis as that "marked by the orderliness of dynamic changes" (Skinner, 1938, p. 40), and by implication there are various ways of specifying behavior not marked by such orderliness. To pursue a simple example, the upward and downward wing movements of flying insects indicate a possible level of analysis where changes are readily observed. Physiological investigations have shown the mechanism controlling these wing movements to involve both proprioceptive feedback loops and a central oscillator alternating outputs to the elevator and depressor muscle groups on a reciprocal basis (Wilson, 1964). The behaviorist might propose a reflexive chain $Rdep \rightarrow (s) \rightarrow Rel \rightarrow (s) \rightarrow Rdep \rightarrow$ to account for the wing movements, but such a hypothesis requires physiological rather than behavioral analysis for validation due to the internal location of proprioceptors. The function of the central oscillator is even more resistant to behavioral analysis. A behavioral account may be effective with the molar response class called flying, although the rate of wing flapping, but not the individual wing movements, may also be subject to an experimental analysis. The behaviorist may describe the molecular response changes noncausally, and must be satisfied with that. He may offer an interpretation in terms of an evolutionary history, but in the example of wing movements it is doubtful if much can be gained by it. It has been suggested that complex processes studied by the cognitive psychologist, such as language, also are unmarked by the kind of orderliness Skinner demands, and require an inner account.

Can the cognitive psychologist do any better at devising a causal account for this difficult class of behaviors, or for behaviors marked by a high degree of complexity in the orderliness of their dynamic changes? On formal grounds, the answer is no. As long as the cognitive psychologist deals with nonphysiological variables, his disposition to theorize does not give access to independent variables unavailable to the radical behaviorist. Any test of a psychological theory inevitably begins and ends with interchange between environment and subject, and no empirical theory can differentially predict in the absence of differential controlling

variables. On behavioral grounds, the answer is not so clear. The nagging problem of metaphor in science is its existence there at all. The contingencies maintaining it may be entirely irrelevant to the processes whereby properties of nature come to control the scientist's verbal behavior—theories, after all, *are* fun. On the other hand, the genetic endowment of humankind and the constancies of human experience may make it as unavoidable a component of the scientist's verbal behavior as the pigeon's peck in the autoshaping experiment, irrational as both may seem. Even though metaphor and theory may be dangerous and misleading at times, the effort to avoid it may have a further cost. It has been suggested that complex operant and verbal behavior is "*better understood* in terms of the operation of rules, principles, strategies, and the like" than in terms of atheoretical analysis, even though that analysis might be accomplished (Estes, 1971, p. 23; emphasis added). Whether Estes or Skinner is correct will not be settled by argument. Contingencies of survival apply to scientific behavior as well as to other cultural practices, and the contingencies will provide the important test.

PRIVATE EVENTS

The necessity for an analysis of private events is apparent with the first request for a causal account: "Why did I do that? Because I felt like it". The analysis may be worked out by application of the two principles already discussed. Though initially private events do not seem to have anything to do with the relation between organism and environment, only to the extent that they do are we aware of them as a subject matter and are we able to offer an analysis of them at all. Thus, private events must be behavioral events if they are to have any relevance to psychology.

Private events fall into the principle categories of events in a behavioral analysis, that is, stimuli and responses. Private stimulation is not problematic as it is unremarkable that stimulation arises from within the skin as well as from without. In fact, Skinner points out that contingencies of survival have resulted in the evolution of two nervous systems specifically sensitive to that type of stimulation. Private stimulation can in principle be dis-

criminative, reinforcing, or punishing on various occasions, depending on its role in contingencies to which an organism may be exposed. However, the natural contingencies involving the interoceptive and proprioceptive nervous systems are defective, and not likely to have precise effects. Reference to private stimulation in the control of behavior is invariably an interpretation, rather than the product of an experimental analysis, because the behaviorist does not have direct access to the controlling variables, but a more important conceptual problem arises in the case where we have "awareness" or "consciousness" of private stimulation. The self-knowledge involved will be discussed shortly.

Private responses fall in several categories, among them covert behavior such as thinking, perceptual responses to the exteroceptive nervous system, and feelings to the interoceptive and proprioceptive nervous systems. Covert behavior is frequently described as differing from overt behavior only in scale, but it would be as much an error to define the covert operant through muscular topography as to do so for the overt operant. While Watson made the mistake of asserting that the vocal cords think, many contemporary psychologists would assert that the brain thinks. Skinner rejects both ideas and proposes that in a behavioral account it is the *person* who thinks, feels, sees, and knows. A strong argument for a response interpretation of perceptual phenomena is made by demonstrating the implausibility of the copy theory, which has been pursued in various guises from ancient Greece to contemporary cognitive theory. If we recall a piece of familiar music (p. 82), it is not that the notes in their temporal organization are brought out of some storage system within the brain to be played again before an inner ear of unknown dimensions, but that we again do some of the things we did in the presence of the music. It is both incautious and unnecessary to assert that what we do when recalling the music is muscular, nor is it necessary to assert that the brain or the nerves exhibit the behavior. So far as we know, a disembodied brain would be as unable to recall a phrase of music as a denervated body. Instead, it is the person as a whole to whom the behavior is attributed.

While the assertion that perceptual phenomena are behaviors begins as an interpretation, various kinds of evidence can be adduced

for it. A literature going back several decades has shown perception to be subject to deprivation effects, conditioning, and so on, all response-like characteristics. Rapid eye movements seem to confirm that dreaming is a response process rather than a sensory one (p. 85), and even stronger evidence comes from recent experiments by Jouvet (1973). During REM sleep, there appears to be an active suppression of electromyographic activity in the muscles of the neck, although the limbs may twitch spasmodically. Jouvet has discovered a nucleus that seems responsible for the muscular inhibition, and when it is lesioned in cats, entry into REM sleep is accompanied by sleep-walking episodes that involved aggressive encounters with nonexistent opponents.

The distinction between "knowing how" and "knowing that" is a crucial one in a behavioral analysis. To say that a child knows how to walk means no more than to say that he walks on appropriate occasions (p. 137). How the child comes to know that he knows, know that he feels, and know that he sees presents a new kind of problem. Skinner asserts that none of these kinds of knowledge is possible without the action of a reinforcing community. The traditional epistemologies of rationalism and empiricism are built on the privacy of thought and perception. In Skinner's account, the transaction between organism and environment is the foundation for knowledge of private events. The private events may come to control behavior only in the way that public events come to do so; that is, they must enter into contingencies of reinforcement. The arrangement of such contingencies is relatively straightforward for public variables, but becomes less precise when the condition to be discriminated is the private stimulation called "hunger", or "love". As in other cases, the argument is mostly one of interpretation, but there are forms of evidence that can be brought to bear, and one of Skinner's favorites is etymology. He repeatedly shows that the vocabulary of private events is either reference to the original conditions or is a metaphorical extension from environmental variables. Thus, pains are "sharp" or "dull" (p. 23), or we speak of being "tense" or "depressed" without meaning that we are literally "stretched" or "weighted down" (p. 24). The numerous examples are compelling, and one wonders if a critic might be able to produce

counter-examples that would lead to a different conclusion. But one doubts if it could be done.

As the behavioral account demands a functional analysis, clarifying the causal role of private events is of paramount importance. That role follows as a direct consequence of more basic commitments of behaviorism, yet little has generated more adverse reaction among critics. Causal variables must be independent variables, accessible to manipulation and control. Private events just happen to occur in the wrong place to fill that role, and so they are treated as dependent variables, or more frequently as "by-products" or as "collateral" to the control of overt behaviors. The reasoning might be called methodological, but should not be confused with methodological behaviorism, which has quite different characteristics (pp. 13-6). When private events control behavior, which they must do in order to account for self-descriptive repertoires, they are not shown to do so through an experimental analysis but through an interpretation based on the logical requirements of the situation and what is known directly about environmental variables. Even here, however, the interpretation is a shorthand for the past history in which the individual, through the action of environmental contingencies, acquired the self-descriptive repertoire.

Private events are not suitable candidates for filling temporal gaps between causal variables and behavior. What is felt as hunger is a condition of a part of one's physiology, and it is the physiology that is causally related to the behavior of eating. However, the feeling is in no way co-extensive with the mediation between environmental causes and behavior—"all that a person comes to know . . . is just more stimuli and responses. He does not make contact with the vast nervous system that mediates his behavior. He does not because he has no nerves going to the right places" (p. 216). There is just no way to fill in causally between environment and behavior and maintain a consistent level of analysis. Private events are not related to the nervous system in a way allowing them to perform that function.

In short, Skinner's analysis of private events is what one ends up saying when giving a functional account at a consistent level of analysis. There is no particular ontological message involved, and Skinner does not claim to have laid bare reality once and for all. The

truth of a statement is a function of the variables controlling the verbal behavior of the individual whose statements are claimed to be true (p. 136), and another set of considerations under the control of another class of variables may lead to other statements. What Skinner has said about private events is as accurate as possible within the framework of radical behaviorism, but as the science of behavior progresses, the controlling variables may be altered and new verbal behavior concerning private events may possibly become appropriate. All one can do at present is to see if the behavioral analysis of private events is useful or obstructive in those situations in which reference to private events is being made. If the behavioral account accurately describes the natural contingencies regarding those events, then the outcome will be favorable.

The claim that Skinner's position concerning private events is nothing but the epiphenomenalist response to the mind-body problem fails (Mowrer, 1972) because it does not take account of the variables controlling Skinner's analysis. It is true that in some ways, the topography of Skinner's verbal behavior resembles epiphenomenalism; on other occasions it resembles identity theory. To assert that Skinner is an epiphenomenalist or an identity theorist is to assert that his verbal behavior is under the control of the same variables as those controlling the philosophical positions just mentioned, and that assertion is a typical mistake of structuralism. It is probably for similar reasons that Skinner has never concerned himself much with philosophical critiques of his position. I am sure he would propose to any philosopher about to undertake an analysis of radical behaviorism that he first spend a year or more in an operant laboratory. Without that experience, the philosopher's critique will be of limited value to the behaviorist, as it will be controlled by variables largely extraneous to the experimental analysis of behavior.

CONCLUSION

Several recent commentators have raised the existential issue of "being" a behaviorist. The first person singular is no more in evidence in this book than it has been in earlier ones, but the book is filled with description of the behaviorist's conception of himself and of his

species. It does not begin with the inner man; that pernicious strategy has been struck down. We are acquainted with the lay vocabulary of private events and self determination long before we begin to acquire a technical repertoire appropriate to the same matters, but for the behaviorist that is the only sense in which the inner life has precedence over a history of environmental interaction. The existential basis behaviorism provides is a product of renewed shaping of self-descriptive repertoires in the light of a scientific account. The shaping process seems to be haphazard in the training of behaviorists, and it is likely that we all have self-descriptive repertoires more and less the product of a technical analysis on different occasions. That is probably harmless most of the time. No doubt it makes little difference if one reads a poem and responds to it naively, or in a way conditioned by the remnants of a college literature course, or by a more technical analysis of verbal behavior. But the behaviorist is failing to be consistent if he rejects the contention that his naive response is any less a product of a past history, unanalyzed though it may be. A feeling or sense of free will does not claim a meaningful precedence in an understanding of behavior either. One is poignantly aware of the fact that the contentious freshman proclaiming "I do exactly what I want to do" does not experience his life in quite the same way as we might when engaged in behaviors of similar topography. Skinner has not written a chapter titled "On Being a Behaviorist" perhaps because its content would be difficult for the nonbehaviorist to understand, but also because one "becomes" a behaviorist not through that sort of thing but through circumstances relating organism to environment. Skinner's new book provides many of those circumstances in verbal form, and it is words enough.

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