

SCIENCE AND HUMAN BEHAVIOR: A TUTORIAL IN BEHAVIOR ANALYSIS

JACK MICHAEL

WESTERN MICHIGAN UNIVERSITY, KALAMAZOO

B. F. Skinner's *Science and Human Behavior* (1953) became the main source of my understanding of behavior during my first semester as a college professor in 1955 at Kansas University. It has continued to exert a major influence throughout my career as the basis for a completely deterministic science of behavior, as a handbook to be consulted as a first step in dealing with any issue in behavior analysis, and as a tutorial in behavioral interpretive analysis—in the use of a small number of behavioral concepts and principles to understand behavior of all degrees of complexity. I describe four general interpretive orientations or maxims that are of broad significance for behavior analysis, and also two underappreciated major theoretical contributions.

Key words: *Science and Human Behavior*; B. F. Skinner, behavioral explanation, genetic determination, motivation, radical behaviorism

BACKGROUND INFORMATION

Graduate Training at UCLA

I first encountered *Science and Human Behavior* (Skinner, 1953) when I was just starting my first full-time teaching job. I had completed my Ph.D. in psychology at UCLA in spring of 1955 and been hired as an assistant professor in the Psychology Department at Kansas University starting that fall. My special interests during graduate training had been physiological psychology, statistics, philosophy of science, and learning theory. My only contact with Skinner was in an undergraduate course, using Hilgard's (1948) *Learning Theory*. Most of the psychology faculty at UCLA were strongly theory oriented and there was much discussion of Hull, Spence, Mowrer, Miller, and Tolman. I had an eclectic view of these different perspectives, and was convinced of the value and necessity of formal theory. I leaned toward a Hullian formulation—my dissertation (happily never published) was an attempt to provide a physiological validation of the Hullian multiplicative relation between drive and habit strength. Skinner's (1950) *Psychological Review* article "Are Theories of Learning Necessary?" was considered far too extreme in its opposition to theory, and although it was covered in graduate learning courses, its general theme

was clearly unpopular with faculty and students (including me).

There was one faculty member in the department who had a Skinnerian orientation, H. C. Gilhousen, and I had taken two undergraduate courses from him on comparative psychology, but he taught these courses from a very traditional perspective with hardly any reference to Skinner. His graduate research assistant, John Cullen, who was a close personal friend, had been much influenced by Skinner's (1938) *Behavior of Organisms* and attempted to enlighten me with respect to what he claimed was a most important development in psychology. He insisted that I borrow *Behavior of Organisms* and look it over carefully—it was the wave of the future. I borrowed it and returned it in a week or so completely unimpressed. I couldn't see what he saw in it. For me the wave of the future was more effective use of statistical inference, better understanding of a logical empiricist philosophy of science, steady improvements in Hull, Spence, and others' theory construction, and the theoretical use of recent discoveries in neurophysiology. John didn't attempt to argue and I don't remember our discussing the issue very much.

Professor Gilhousen, however, was responsible for my having a copy of *Science and Human Behavior* in my personal library. During my last year as an undergraduate student he offered a course titled "Motivation," and I enrolled in the course. After attending the first two lectures, I decided to drop the course because it did not appear that we would be considering anything that had not

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Correspondence concerning this article should be addressed to Jack Michael, Psychology Department, Western Michigan University, Kalamazoo, Michigan 49008 (e-mail: jack.michael@wmich.edu).

been covered in the two courses I had taken from him on comparative psychology. That the text assigned for the course was Skinner's *Science and Human Behavior* (*S&HB*) did not clue me that this course was going to be quite different. I had purchased the text for the course and did not sell it back, I think because I had nothing in my library by Skinner, but I do not remember reading it until my first semester as an assistant professor at Kansas University in 1955.

Kansas University, 1955–1957

I was hired at KU primarily to teach advanced statistics (mainly analysis of variance and experimental design), physiological psychology, and broader methodological or philosophical issues. In addition to advanced statistics and a graduate course titled "Methodology" (a sort of philosophy of science course), my teaching load for the fall semester of 1955 also included a junior level introductory course for students who were not psychology majors. An eclectic introductory text had been assigned for the course, and as we went through the text, I used my personal library to try to find lecture material that would make the text topics more relevant to the students' everyday activities and thus possibly more interesting. One evening in the 3rd or 4th week of the semester I was beginning to look for material to use in my lectures that would accompany the upcoming chapter on learning. I had several texts on learning theory from my UCLA courses, and *S&HB* was in the same part of the book shelf. A glance at the table of contents showed that the whole last half of the book was concerned with exactly the kind of topics that would be of interest to the students in this course—self-control, thinking, social behavior, government and law, religion, psychotherapy, designing a culture—and would presumably be analyzed in terms of the learning concepts presented in the first half. This was just what I needed for interesting lecture material.

I started reading that night and could hardly stop. I found that *S&HB* was much more than an introductory treatment for students with no psychology background. It was a highly consistent and completely behavioral interpretation of all aspects of human behavior. My graduate training at UCLA had emphasized differing theoretical perspectives and

subtle controversial issues. For me there had been no implication that a small number of concepts and principles, without further experimental research, could be the basis for a comprehensive understanding of human behavior and of great practical value for improving the human condition. This was clearly the message of *S&HB*, and my teaching efforts with that introductory course for non-majors had apparently sensitized me to the value of such an approach.

I continued to use *S&HB* for lecture material for the rest of the semester, and as a result, my lectures generated a good deal of useful and animated in-class and out-of-class discussion; considerable praise for my (Skinner's) analysis of everyday events and other forms of reinforcement for use of *S&HB*. And the repertoire that was developing from my study of the book and from my interaction with the class became increasingly my way of interacting with the KU faculty and graduate students. By the end of that first semester I was beginning to be known for my Skinnerian approach to everything. I taught the course two more semesters during which the traditional topics became primarily excuses for introducing the important components of a basic and applied science of behavior, as in *S&HB*.

The senior members of the KU Psychology Department at that time were cognitive psychologists of the Kurt Lewin variety (Fritz and Grace Heider, Martin Scheerer, Herbert Wright, Roger Barker, Eric and Beatrice Wright). My primary graduate teaching responsibility (advanced statistics) did not bring me into any conflict with this orientation. A significant portion of cognitive psychology, however, consisted in criticism of behaviorism. As my behavioral views became known to the graduate students (who were my age and with whom I socialized a good deal), they wanted me to address the flaws in behaviorism that the senior faculty were presenting in their graduate courses. Fortunately, the first three chapters of *S&HB* addressed many of these issues, and as a result, I found myself quite fluent in my frequent debates with the graduate students.

As it turned out, there were almost no faculty¹ or graduate students in the Psychology

¹ Edward Wike, who had come to KU with a Ph.D. from

Department at KU who knew anything at all about Skinner. Their criticisms of behaviorism were mostly directed at Watson, Thorndike, Hull or, more commonly, their own Lewinian interpretation of those views. I could often agree with some aspect of the criticism, but denied that it had any relevance to what I considered the modern descriptive behaviorism of B. F. Skinner. From me, the grad students received the message that their objections were either easily answered or were based on their own seriously flawed interpretation of behaviorism.

This interaction plus my teaching of a psychology of language course (based largely on a mimeographed version of Skinner's William James Lectures) in 1956 and again in 1957 brought me considerable attention—notoriety is a better term—and the departmental executive committee eventually decided that it would be better for all concerned if I obtained work at some other university. They were very considerate in urging me to look for another job but not to take an unsatisfactory one—in fact, to wait until I found a job that I would consider an improvement over the present one at KU. Several members of the department then began to contact colleagues at other universities as a way of helping me find such a job, and a nice position at the University of Houston was located, where I was subsequently hired, starting in summer of 1957.

University of Houston, 1957–1960

As at KU, my primary teaching responsibility was advanced statistics, and although the faculty were not in general agreement with Skinner's views, they welcomed my enthusiastic promotion of those views as a stimulus for intellectual discourse. It was an eclectic department, and several of the faculty (Richard Evans, Daniel Sheer, James McCary) enjoyed arguing with me and seemed intrigued with the practical implications of Skinner's behaviorism as I presented them based on

UCLA a few years before me, was very familiar with all varieties of behaviorism, especially the works of Hull and Skinner, but for various reasons kept a low theoretical profile. When he realized the extent of my interest in Skinner (that had not existed when I came to interview for the job) he gave me his copy of *Principles of Psychology*, the very effective 1950 introduction to Skinner's work by F. S. Keller and W. N. Schoenfeld.

S&HB and on Skinner's analysis of language in the William James Lectures and in *Verbal Behavior* (Skinner, 1957).

Interactions with my colleague Lee Meyerson and informal graduate seminars based on *S&HB* and *Verbal Behavior* (VB) resulted in strengthening my dependence on these two books as a basis for dealing with any aspect of behavior. When I had an opportunity to teach a junior level course on learning, however, I used Keller and Schoenfeld's (1950) *Principles of Psychology* as the text. I didn't think *S&HB* would be what the rest of the faculty would consider a learning text—no figures, no references to basic learning research, and so forth.

Arizona State University, 1960–1967

By 1959 Arthur Staats at Arizona State University and Hudson Jost, a physiological psychologist who had recently been hired as chair, were beginning to develop a strongly behavioral department and offered me a position. (Israel Goldiamond was the other hire that year.) I was not dissatisfied with Houston, but this was a better opportunity to further the behavioral position within psychology. By the time I moved to ASU in 1960, I was a dedicated and orthodox Skinnerian and continued to make intellectual and increasingly practical use of *S&HB* and *VB*. My colleagues there during the first several years were also very behavioral (Arthur Staats, Israel Goldiamond, Thom Verhave, Joel Greenspoon, Arthur Bachrach, Aaron Brownstein), but my reliance on the content of those two books was somewhat unusual even in that context, and I think I was considered a little too narrow in my theoretical focus. I taught an introductory behavior analysis course but, again, did not use *S&HB* as the text because by that time the Holland and Skinner (1961) programmed textbook *The Analysis of Behavior* had become available. At ASU, I became increasingly involved with Lee Meyerson's rehabilitation psychology program and made extensive use of the *S&HB* and *VB* repertoires in interaction with Lee and with the graduate students who were being supported on federal training grants that he had obtained.

Western Michigan University, 1967–2003

In 1966 Roger Ulrich had become chair of the WMU psychology department and was in

the process of strengthening its behavioral orientation. David Lyon, Paul Mountjoy, Neil Kent, Richard Malott, Douglas Anger, and Ron Hutchison were already there, and I accepted a position to begin in 1967. My teaching duties at WMU involved elementary statistics, an undergraduate course on verbal behavior (I used Skinner's *VB*), later an introductory course for psychology majors (using the Holland-Skinner programmed text, and various graduate courses, one with *VB* as the required text and others with *S&HB* as one of several required texts. While at WMU, I have written a number of conceptual papers, for example, on positive and negative reinforcement, the discriminative stimulus, establishing operations (Michael, 1975, 1982, 1993), and what I had absorbed from *S&HB* has figured prominently in such papers. I am quite confident that much of my writing in retirement will continue to have ties to Skinner's analysis in *S&HB*.

RETROSPECTIVE APPRECIATION

When I look at my own repertoire, I find many elements or components that can be easily traced to *S&HB*, and most of them are things I appreciate. This is not exactly a *retrospective* appreciation because I have treasured my familiarity with and use of *S&HB* often since that first night in 1955. This is a retrospective appreciation, however, in the sense that when I reread some part of the text I often find an analysis that seems deserving of new consideration. The book has affected me in several ways. It is a comprehensive description of a completely deterministic science of behavior, covering topics from unconditioned reflexes to the design of a culture. In this sense, it has functioned as a handbook that I have often consulted as a first step in dealing with some issue in behavior analysis. Somewhat more specifically, it has been a tutorial in behavioral interpretive analysis—in the use of a small number of behavioral concepts and principles to understand behavior of all degrees of complexity. It provides training in how to talk and how to think consistently about behavior and its controlling variables, not only with respect to the details of an analysis, but also in terms of some more general interpretive orientations or maxims. It also contains a number of important theoretical and conceptual contribu-

tions that are not found elsewhere; some of which have been especially important in my own teaching and writing. I start with four of the general interpretive orientations or maxims and then consider two theoretical contributions. These points are still useful to me over 40 years after I first encountered them.

Maxims

Any comprehensive account. At the beginning of Chapter 15 ("Self Control") a common objection is presented:

In emphasizing the controlling power of external variables, we have left the organism itself in a peculiarly helpless position. Its behavior appears to be simply a "repertoire"—a vocabulary of action, each item of which becomes more or less probable as the environment changes. (p. 228)

As an aside, and omitting "simply," this is a conveniently succinct and accurate statement of the behavioral position. The objection continues,

Yet to a considerable extent an individual does appear to shape his own destiny. He is often able to do something about the variables affecting him. Some degree of "self-determination" of conduct is usually recognized in the creative behavior of the artist and scientist, in the self-exploratory behavior of the writer, and in the self-discipline of the ascetic. . . . The individual "chooses" between alternative courses of action, "thinks through" a problem while isolated from the relevant environment, and guards his health or his position in society through the exercise of "self-control." (p. 228)

The beginning of the next paragraph then contains a brief statement of what I consider one of the most important "attitudes" generated by *S&HB*: "Any comprehensive account of human behavior must, of course, embrace the facts referred to in statements of this sort. But we can achieve this without abandoning our program" (p. 228).

The last two sentences can be a very useful guide for considering any statement about behavior irrespective of that statement's compatibility with the behavioral position. (The remainder of the chapter is a detailed analysis of behavior that appears to be evidence for self-control, but is ultimately to be understood in terms of the current environment and the environmental history of the individ-

ual.) A similar point is made in concluding a section on generalized reinforcement. “These observable facts must have a place in any theoretical or practical consideration” (p. 81); and in a statement about the impermanence of the effects of punishment, reference is made to Freud’s concept of repressed wishes followed by “as we shall see later, Freud’s observations can be brought into line with the present analysis” (p. 184).

Throughout the book explanations in terms of an inner agent are analyzed and rejected, but the observable facts on which they are based are seriously considered and “embraced” or “brought into line” with a behavioral analysis. Our approach is sometimes criticized as unscientific or narrow in terms of our failure to consider alternative viewpoints, but a ready willingness to consider the facts upon which they are based can mitigate this criticism to some extent. This maxim can also constitute an argument against “giving away” any topic related to behavior—assigning it to some other approach—talk therapy, individual differences, the perceptual constancies, imagery, for examples, all are grist for the behavioral mill.

If many variables are important. Current behavior may depend upon a history of reinforcement and relevant conditions of deprivation that may be unavailable at the time prediction is required. In Chapter 7 (“Operant Discrimination”), a scenario illustrating the difficulty and possible solutions is constructed around the task of predicting whether a guest will come to the table when the host says, “Won’t you come to dinner?” (p. 113). The analysis makes a major point and at the same time illustrates the sophistication that is possible from a behavioral perspective. (I recall the comment of one of the senior professors at Kansas University to the effect that behaviorism was possibly useful in understanding the behavior of young children and the developmentally disabled but hardly relevant to the normal adult.)

Coming to the table is the kind of behavior that has been reinforced with being able to eat, and we can probably assume such a history, but without relevant food deprivation the guest may decline on the grounds of not being hungry.

But even if the history of . . . deprivation is

satisfactory, the operant responses may be displaced by other behavior involving the same musculature. If our guest has been offended by undue delay in the preparation of the meal, for example, he may take revenge by creating a further delay—perhaps by asking to wash his hands and remaining out of the room a long time. (p. 113)

(This is exactly the kind of example that the Kansas cognitive psychologists would cite as proof that an understanding of human behavior without internal meanings, etc. was impossible.)

[Such] behavior has been acquired because it has been reinforced by its damaging effect upon other persons—because the guest has “learned how to annoy people.” Before we can predict that he will come to the table . . . we must have information about all relevant variables—not only those which increase the probability of the response but also those which increase the probability of competing responses. (p. 113)

Because this kind of information is usually unavailable it will be easier to assume that whether or not the guest comes to the table will depend upon whether or not he wants to. But this approach “is of neither theoretical nor practical value, for we still have to predict [his wants]. The inner explanation is no short cut to the information we need” (p. 113). And this is the punch line: “If many variables are important, many variables must be studied” (p. 113). This is a difficulty characteristic of all scientific prediction, certainly not unique to behavior analysis.

Don’t reverse the direction. “The control exerted by a discriminative stimulus is traditionally dealt with under the heading of attention. This concept reverses the direction of action by suggesting, not that the stimulus controls the behavior of an observer, but that the observer *attends* to the stimulus and thereby controls it” (p. 122). This undesirable verbal practice also occurs with respect to other terms related to stimulus control. The fact that the control by a stimulus may also be shown to some extent by novel stimuli is referred to as stimulus generalization, but the direction is often reversed in saying that the organism generalizes from the original to the novel stimulus, or in the case of metaphor, “*transfers* a description from one state of affairs to another which resembles it” (p. 133).

When an organism's response is reinforced in the presence of one stimulus and extinguished in the presence of another stimulus, the direction of control is often reversed by saying that the organism now discriminates between the two stimuli (p. 134). *Choosing* and *preferring* are more recent terms susceptible to this perversion, and many behavior analysts, sad but true, refer to the critical behavior of their experimental subjects as discriminating, preferring, and ultimately choosing one stimulus or one operandum over another. The identification and labeling of this undesirable verbal practice does not occur often in *S&HB* but often enough to sensitize the reader to the problem. Although stating the facts in the proper direction may sometimes be more cumbersome, it does not so easily support the notion of an inner process that precedes and causes the differential responding.

Possibly born that way. In everyday psychology and in the popular press, behavioral relations without an obvious environmental history are often attributed to inheritance—to the organism's genes. Many human characteristics are explained by the statement that the person was "born that way." As a general problem, this is dealt with in Chapter 3. ". . . the doctrine of 'being born that way' has little to do with demonstrated facts. It is usually an appeal to ignorance. 'Heredity' as the layman uses the term, is a fictional explanation of the behavior attributed to it" (p. 26). The uncontroversial behavioral antidote to this appeal is the identification of more subtle or pervasive features of the environment that may be relevant. Skinner does this in a number of places in *S&HB*, and similarly persuasive analyses occur in much other behavioral writing, both before and after Skinner's 1953 treatment.

In acquiring a behavioral approach, however, it is possible to adopt an anti-inheritance position so strong that any evidence of a genetic origin of some particular behavior is taken as a threat to behaviorism as a philosophy. And, in fact, anti-behavioral arguments do sometimes consist in citing some such evidence as though it contradicted a basic behavioral tenet. In this respect, *S&HB* develops in the reader a descriptive empirical attitude as contrasted with a commitment to the notion that all behavior is learned.

In a section on generalized reinforcement, the possibility is entertained that efficient manipulation of the physical environment may function as a form of generalized reinforcement because of having preceded many other forms of reinforcement. "We are automatically reinforced, apart from any particular deprivation, when we successfully control the physical world. This may explain our tendency to engage in skilled crafts, in artistic creation, and in such sports as bowling, billiards, and tennis" (p. 77). But then an alternative interpretation is considered. "It is possible, however, that some of the reinforcing effect of 'sensory feed-back' is unconditioned. . . . Any organism which is reinforced by its success in manipulating nature, regardless of the momentary consequences, will be in a favored position when important consequences follow" (p. 78).

A genetic origin for the pigeon's pecking response is considered possible in a section on shaping (p. 93), and later an imitative "reflex" is rejected, not on principle, but on lack of empirical evidence. "So far as we know imitative behavior does not arise because of any inherent reflex mechanism. . . . This would be an extremely complex mechanism and, in spite of a strong belief to the contrary, it seems not to exist." (p. 119). Another example involves a possible unlearned establishing operation in the area of emotion: "Just as food is reinforcing to a hungry organism, so damage inflicted upon another is reinforcing to an angry one" (p. 163). Although such unlearned relations involving food, water, sex, and others have not been controversial, the identification of "cries of pain and other evidences of damage" (p. 164) as possible unconditioned reinforcers established by the independent variable that produces anger is quite liberal in its entertainment of an unlearned provenance.

These and other such treatments develop a proper attitude of indifference with respect to genetic determination, an attitude that serves the field well as it tries to deal with individual differences in intelligence, artistic and athletic ability, sexual preference, susceptibility to alcoholism and so on. Other considerations aside, these are empirical issues that can not be easily resolved without a very sophisticated analysis of environmental effects, some of which become relevant very

early in the organism's lifetime and are effective on it for prolonged periods.

Important Conceptual or Theoretical Contributions

Not really stimulation. A major conceptual and terminological point is made in the first paragraph of Chapter 9 ("Deprivation and Satiation").

The discovery that part of the behavior of an organism was under the control of the environment led, as we have seen, to an unwarranted extension of the notion of the stimulus. Writers began to infer stimuli where none could be observed and to include various internal conditions in a 'total stimulating situation.' The principle of the stimulus was weakened by this extension and often abandoned in favor of other formulations of a less specific nature. It may be restored to usefulness in its proper sphere by distinguishing, as we have done, between the several functions of stimuli. We have now to note that *some effects of the environment are not usefully classified as stimulation at all* [italics added]. When we deprive an organism of food, for example, we may stimulate it, but this is incidental to the main effect. (p. 141)

The main effect is to increase the probability of relevant behavior (p. 142). And although food deprivation may produce hunger pangs as a form of stimulation, the implication here is that this stimulation is incidental to the effect of altering the probability of the type of behavior that has been reinforced with food. This proposition is clearly in opposition to the general behavioral notion that every response must have been produced by a stimulus—not, of course, Skinner's notion—and is still not widely appreciated.

Much of Chapter 9 is concerned with analyzing and eliminating from further consideration various alternative concepts that function as explanatory fictions, but what remains is a highly consistent theory of motivation. When combined with the concept of the emotional predisposition in the next chapter ("Emotion") and with the motivational aspects of aversive stimuli in the one after that ("Aversion, Avoidance, Anxiety"), the theory becomes comprehensive. From my perspective, the only improvement would consist in the existence of a common term for the motivational relations, distinguishing them from

the other effects of the various environmental events.

Radical behaviorism. The essence of the philosophical view known as radical behaviorism is contained in the first five pages of Chapter 17 ("Private Events in a Natural Science"). There appears the critical description of the four ways verbal behavior can be brought under the control of private stimuli and of the limitations of the resulting repertoire. This presentation is not much different from the same material found in the *Psychological Review* article (Skinner, 1945), or in *Verbal Behavior* (Skinner, 1957). Because of the function of *S&HB* as an introductory text for nonspecialists, however, the topic is set in a broader context; one that has always seemed to me of considerable, and possibly overlooked, importance. The radical-methodological distinction is based on the inclusion of private events in the first and their exclusion from the second, because they are not subject to public confirmation. Some scholars have, in a sense, *rejoiced* in the broadening of the behavioral approach so that feelings, consciousness, and states of mind could now be included, ostensibly because of the general importance of such events for our understanding of the individual's behavior and of the human condition in general. A quite different perspective, however, is possible from the section at the beginning of the chapter. After making the point that events taking place within the organism seem to have no special properties because of their limited accessibility, except for rendering a functional analysis more difficult, the question is asked how such variables should be treated.

These questions may not be of interest to all readers. The issue is an ancient one, which has occupied the attention of philosophers and others for more than two thousand years. It has never been satisfactorily resolved, and perhaps the present inclination on the part of educated laymen to avoid it represents simple extinction. Fortunately, the issue is seldom crucial in the practical control of human behavior. The reader whose interests are essentially practical and who may now prefer to move on to later chapters may do so without serious trouble. Nevertheless, the issue is important and must sometime be faced. Modern science has attempted to put forth an ordered and integrated conception of nature. Some of its most distinguished men have concerned

themselves with the broad implications of science with respect to the structure of the universe. The picture which emerges is almost always dualistic. The scientist humbly admits that he is describing only half of the universe, and he defers to another world—the world of the mind or consciousness—for which another mode of inquiry is assumed to be required. Such a point of view is by no means inevitable, but it is part of the cultural heritage from which science has emerged. It obviously stands in the way of a unified account of nature. The contribution which a science of behavior can make in suggesting an alternative point of view is perhaps one of its most important achievements. No discussion of the implications of science for an understanding of human behavior would be complete without at least a brief review of this contribution. (p. 258)

Seeing the verbal behavior controlled by private stimuli as no different in any important way from that controlled by public stimuli corrects a long standing misconception. Feelings, consciousness, and states of mind are simply verbal behavior, its relation to environmental and historical controlling variables, and its effect on other ongoing behavior. Furthermore, from an analysis of the way such behavior is developed, it is a very limited repertoire in terms of its accuracy and ultimately its value for the individual and for the social group. I think one could infer from Skinner's general approach (at least I so infer) that rather than feelings, it would be much better to get *in touch with* (acquire a more sophisticated understanding of) your current environment and your environmental history.

CONCLUSION

Many other aspects of *S&HB* could have been emphasized, and as I reread this material I am more impressed by all the things that have not been considered than by the few that have. In a talk I gave recently, I illustrated my confidence that “every page contained one or more gems” by turning randomly to a page, and finding a passage that became the basis for an extension of the talk into the lunch period. In our field there are very few books that have contributed as much to the development of the science of behavior.

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