Noel W. Smith, Editor
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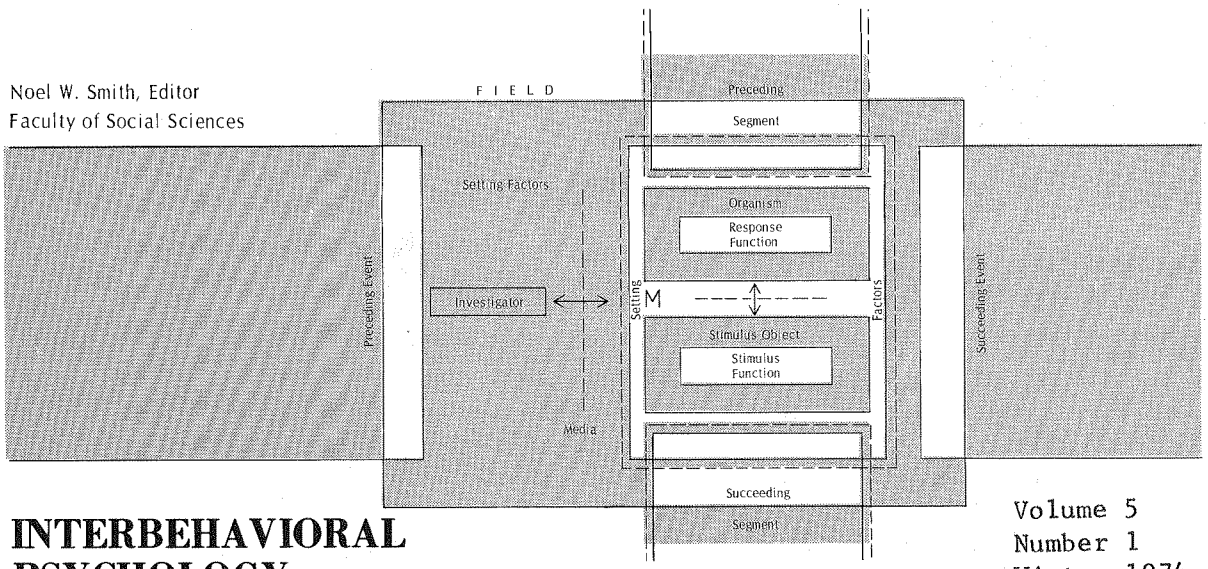

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.... If you start with the assumption that whatever the world is made of must be inherently inert, you then have to go ahead and guess that it changes only as force is applied to it. Here you are, saddled with two distinct constructs; objects, and the force that makes them move. As long as you are a materialist--and nearly everybody is in spite of what he says--there is not much you can do except think in terms of primary objects, such as the atoms of Democritus, being pushed around by secondary forces. Apply this basic thinking to physiology, and you have the notion of a body being actuated by energy; apply it to psychology, and you come up with the notion of a person either being propelled by "motives" in spite of himself, or stuck tight in his fundament. . . Suppose we began by assuming that the fundamental thing about life is that it goes on. It isn't that something makes it go on; the going on is the thing itself. It isn't that motives make man come alert and do things; his alertness is an aspect of his very being. Talking about activating motives is simply redundant talky-talk, for once you've got a human being on your hands, you already have alertness and movement, and sometimes a lot more of it than you know what to make of. There is another habit of thinking that Western Man more or less fell into fortuitously. As long as he was assuming that human beings are propelled by motives, it seemed reasonable to imagine also that the motives give direction to the movement; if they push, they must push in some direction. Now if we could only find out what is pushing, we could predict where everybody is going, as well as how soon he would get there. So for two thousand years we have been looking for the thing that is doing the pushing, and often trying to define it by the directions it pushes. We haven't found it yet; naturally, we haven't found it, but during the centuries we have built up a tremendous lexicon of push and pull terms. Even our language has fallen heir to the design of our quest, and we have committed ourselves to a grammar of motives that controls our speech and channels our thinking about human behavior. Now we can scarcely say anything about what a person had done, or is about to do, without using a language form that implies that he has been pushed into it. We are even inclined to think that way about our own behavior, and when we do, it usually means we are in trouble.

--George Kelley in Nebraska Symposium on Motivation, 1962.

THE AGORA

The quotation from George Kelley is one of those admirable statements where the author's own works do not follow his best precepts.

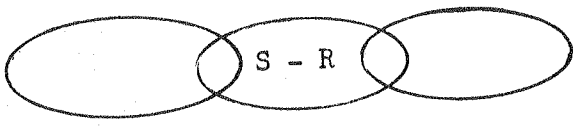
Two recent articles have appeared that suggest the importance of considering the field or context in psychological situations. James R. Averill writes in the Psychological Bulletin, 1973, 80, 286-303 under the title "Personal Control Over Aversive Stimuli and Its Relation to Stress" that conditions of stress and the extent to which the individual can control it "depend upon such factors as the nature of the response and the context in which it is embedded and not just upon its effectiveness in preventing or mitigating the impact of a potentially harmful stimulus." They also depend "upon the meaning of the control response for the individual; and what lends a response meaning is largely the context in which it is embedded." Donald N. Bersoff in "Silk Purses Into Sows' Ears: The Decline of Psychological Testing and a Suggestion for its Redemption," American Psychologist, 1973, 28, 892-899, recommends that the usual artificial test arrangement be replaced by "psychosituation assessment" where "the aim is to 'contextualize' behavior and discover what Fischer called the when/when-nots of specific behavior. In the classroom, both the child's behavior and that of the teacher are carefully assessed before any changes in the instructional environments are made. The child and his teacher 'co-constitute' this instructional environment, interacting to evoke behavior in the other. Any assessment procedure that isolates the target person from the significant others who participate in his behavior can be considered, at the very least, incomplete, and, at the most, unethical."

In a chapter on "Hypnotism and Surgical Pain" by J. F. Chaves & T. X. Barber in a new book by Barber, Spanos, and Chaves ('HYPNOSIS,' DIRECTED IMAGINING AND HUMAN CAPABILITIES, N.Y.: Pergamon Press, in press, chapter 8) the authors stress that surgical pain is primarily limited to outer tissues and that with a local skin anesthetic along with low anxiety, positive expectations of minimal pain, and active imagining of insensitivity, surgical pain can be tolerated by some individuals. These conditions are those used in hypnotic surgery and are essentially the conditions of acupuncture surgery as well. The same authors consider acupuncture surgery in more detail in Medfield Foundation Report #128: "Acupuncture Analgesia: A Six-Factor Theory." These factors are "(a) the patients who are accepted for surgery with acupuncture strongly believe in its efficiency and are not fearful or anxious; (b) with few exceptions, narcotic analgesics, local anesthetics, and sedatives were also used, singly or in combination, during surgery with acupuncture; ...(c) the pain normally associated with many surgical procedures is less than is generally assumed...(d) the patients are typically exposed to special preparation and indoctrination for several days prior to surgery, (e) the acupuncture needles distract the patients from the pain of surgery, and (f) suggestions for pain relief are present in the acupuncture situation." They describe the Chinese Yin-Yang meridian theory and observe that some Chinese surgeons disregard the traditional meridian locations and put the needles anywhere. They find the neural gate theory equally unsatisfactory. This theory fails because (1) the gate is a hypothetical entity having no observable referent, (2) some of the acupuncture locations such as ear or head could not close the spinal gate and would require postulation of still additional gates, (3) it cannot account for failures, (4) it cannot account for the necessity of belief in the efficacy of the needles, (5) it cannot account for the requirement of localized anesthetics. By contrast, Ronald Melzack ("How Acupuncture Can Block

Pain," Impact of Science on Society, 1973, 65-75) argues the traditionalist approach. He supports the gate theory as well as hypothesizing neural arrangements that might give support to the Yin-Yang theory. He rejects hypnosis by assuming the traditional trance notion and other discredited concepts about it. Ironically, he describes the importance of low anxiety and the probable role of suggestion in a manner that approaches the account given by Chaves and Barber. The view that hypnosis is a mysterious and paranormal state rather than "DIRECTED IMAGINING" is a tenacious one.

Among Bob Lundin's several books that incorporate an interbehavioral approach is the new second edition of PERSONALITY: A BEHAVIORAL ANALYSIS, Macmillan Co., 1974.

The author of the feature article in this issue is a graduate student at State University of New York at Plattsburgh. In the next issue we will also carry an article on motivation by a graduate student.



B. F. Skinner on Motivation: a Critique

Cynthia J. LaShier

B. F. Skinner never really said what he thought "drive" was. In 1938 he allotted "drive" a whole chapter of heavy reading in experimental procedures and charts, typical of The Behavior of Organisms. In 1953 the chapter heading "Drive" did not appear. Rather, it had become "Deprivation and Satiation," which was much more appropriate to Skinner's operational approach in both The Behavior of Organisms (1938) and Science and Human Behavior (1953). The earlier work is valuable as experimental report and as a basic statement of Skinner's position, the latter as a readable digest of Skinner's system. In 1938 Skinner's concern was statement and method; in 1953 it was interrelationship.

Drive in 1938

Skinner's main purpose in 1938 was to demonstrate the usefulness of the response as the "proper study of organism-kind" as the behaviorist saw it. From the vantage point of the response-observer, Skinner began his discourse on the nature of drive with the following inferential statement:

> "The problem of drive arises because much of the behavior of an organism shows an apparent variability. A rat does not always respond to food placed before it, and a factor called its 'hunger' is invoked by way of explanation. The rat is said to eat only when it is hungry. It is because eating is not inevitable that we are led to hypothesize an internal state to which we may assign the variability... As in any case of variability in reflex strength, the problem here is to find the variable or variables of which the strength is a function and to express the relationship in a set of laws." (1938, pp. 341-342.)

The inference is that response variability indicates the existence of some cause of it. The use of the word "cause" was anathema to Skinner, but the best he could do with "drive" at that time was to hypothesize an equivalent to an intervening variable, which he preferred to call an "internal state." Although the behaviorist strenuously objects (and Skinner not the least of them) to intervening, immeasurable unobservables, Skinner did not seem at all uncomfortable with "drive." The proof of its existence is, as he said, reflected in response variability. In any case, "internal state" is a vague term for a behaviorist to be using. His ultimate purpose was to include it, thereby legitimizing it, in a set of behavioral laws.

Step one toward legitimization was the use of operational definition. Drive level was a function of deprivation, expressed in number of hours. The drives Skinner investigated were hunger and thirst; his subjects were rats. But he did not call his studies investigations into "hunger drives" or "thirst drives" because he refused to speak of kinds of classifications of drives. All-important were the effects of deprivations on the organisms's response patterns; naming the "drives" was not only irrelevant, but meaningless.

Skinner's "reflex strength" was close kin to Hull's "habit strength" in that it was a result of conditioning, i.e., it is the learned component of performance. The other component, also parallel to Hull's, was drive. In his two chapters on drive in The Behavior of Organisms, Skinner presents experimental evidence to sup-

port his multiplicative theory of performance, and comes to just about the same conclusion as Hull. What he did was to observe the effect of different levels of drive (deprivation hours) on instrumental learning, or operant behavior. The rat, depending on how "hungry" (with apologies to Dr. Skinner) he was, was to press the lever or perform some other task in order to receive a reward, or reinforcement. The reinforcer was what, in ordinary operant conditioning, more or less assured that learning would take place. Learning was measured in "reflex strength," assuming the reinforcer really did increase the probability of a given response. To measure reflex strength or "learning," Skinner used the ingenious method of resistance to extinction. The more responses to "complete" extinction, the more the reflex strength.

It was probably Skinner's definition of "positive reinforcer" that helped lead him to a rapid conclusion on the nature of drive. If the reader recalls, a "positive" reinforcer is anything following a response which increases the probability of that response's recurrence the next time the same or similar stimulus conditions are present. If a positive reinforcer is really reinforcing, then it follows that a food-deprived rat should show no significantly different level of reflex strength from any other non-deprived rat, IF drive has no effect. It was no surprise to anyone that Skinner found that, indeed, drive did have a tremendous effect on performance. However, he also found that number of responses to extinction did not significantly differ from deprived to non-deprived groups. He concluded, logically enough, that drive had no effect on learning. Hull came to the same conclusions.

Drive in 1953

By 1953 Skinner's attempt to pinpoint the nature of drive yielded both more precise and more vague phraseology. As was noted earlier, "drive" had now become "deprivation and satiation," indicating his distaste for an all-inclusive concept. Of "drive," he said, "The term is simply a convenient way of referring to the effects of deprivation and satiation and of other operations which alter the probability of behavior in more or less the same way." (1953, p. 144).

Further, drive was no longer a "state." It was "... a verbal device with which we account for a state of strength, and it cannot answer experimental questions" (1953, p. 144) [emphasis mine]. It was now something (but not an entity) which helped account for a "state," that state being learning. One gets the impression that Skinner at this point had abandoned hopes of compromising drive and reinforcement and had decided to concentrate wholly on reinforcement as essential to both learning and performance. He seems to cling to drive as one answer to the response variability problem and, more specifically, as an answer to the behavior of individuals and their responses at any given time. In fact, he seems to have come around to the point Hull did when Hull came upon his "oscillation" idea. It is an explanatory concept of the last-ditch-effort type which is used in a mild state of conceptual desperation. In any case, Skinner's frustration with the concept is obvious. He says, "No concept can eliminate an actual diversity of data" (1953, p. 144), and he admits (see above) that "it cannot answer experimental questions."

No, drive was neither physiological nor psychic state, nor was it a stimulus. Nor could it be simply a state of strength. No, he said, "The prossibility remains that the strength of the behavior is due to other kinds of variables not in the field of motivation." (1953, p.146). Drive is not the only source of behavioral variability.

Having failed to solve the problem, Skinner resorts again to deprivation-as-drive, which is inferred from "unexplained changes in probability." Unfortunately,

that which is inferred from these changes is also used to account for them. Such a flaw in reasoning is an error of circularity and was not detected by Skinner, who went on to address the question of categorization of drives in the following manner: "Our question thus becomes: How many kinds of behavior vary in strength independently of each other?" (1953, p. 149)

Though lacking the formality of Clark Hull, Skinner approached the interaction of learning and drive in a Hullian way--by using the reinforcing properties of stimuli which are drive-related. He would never have dared to call it drive-reduction or even drive-stimulus reduction and avoided at all costs the mention of "goal." Other than these omissions of what Skinner believed to be meaningless terminology, his theory is very near Hull's and not so far from Tolman's. An example given in the text proceeds as follows:

> "The behavior of going to a restaurant is composed of a sequence of responses, early members of which (for example, going along a certain street) are reinforced by the appearance of discriminative stimuli which control later responses (the appearance of the restaurant, which we then enter). The whole sequence is ultimately reinforced by food, and the probability varies with food deprivation. We increase the chances that someone will go to a restaurant, or even walk along a particular street, by making him hungry." (1953, p. 150).

Skinner's emphasis here is on the probability of a given response, which leaves us no alternative but to classify him under "Pure Behaviorists." The influence of Hull, however, is noticeable in Skinner's new inclusion of behavior-reinforcing stimuli. And, though the implication of goal-direction is plain, in deference to the "pure behaviorist" we shall not belabor the point. He concludes, as in 1938, that "Behavior which has been strengthened by a conditioned reinforcer varies with the deprivation appropriate to the primary reinforcer." (1953, p. 150)

Skinner launches into a discussion of "generalized reinforcers:" attention, approval, affection, and domination. He says these cannot be drives because they do not show results of the appropriate operations of deprivation and satiation. Here again we see the difference between the behaviorist and most motivation theorists. Whereas the latter would describe attention and approval in terms of needs and secondary drives attained by reinforcement, Skinner maintains that they themselves are reinforcers.

Emotion, Negative Reinforcement, and Punishment

Emotion: 1953

Skinner attacks the problem of "emotion" in much the same way as he did drive--by looking for variables of which emotions are functions.

> "We define an emotion--insofar as we wish to do so--as a particular state of strength or weakness in one or more responses induced by any one of a class of operations." (1953, p. 166)

> "The behavior observed during an emotion is not to be confused with the emotion as a hypothetical 'state'." (1953, p. 168)

As with drive, the proper subject matter in the study of emotion is (1) the behavior and (2) the manipulable conditions for it. Also, as with drive, its "state" is neither physiological nor psychic, nor is it a cause.

The relationship of emotion to drive is cousin-like. Behaviorally, an "extreme deprivation probably acts as an emotional operation." (1953, p. 165)

Working with everyday words like "anger" and "sadness," Skinner discards them as colloquial at best, but fails himself in his attempt to discover any really reliable scientific definition of them. In this author's opinion, the fact that he worked with them at all indicates that this is not the purely behavioristic Skinner of 1938.

Negative Reinforcement: Avoidance

The confusion of negative reinforcement with punishment is well-documented. In Skinner's negative reinforcement, withdrawal of the aversive stimulus increases the probability of that response's recurrence under the same conditions.

Until 1953 Skinner managed to avoid any notion of drive-reduction. Even then, he would have managed to avoid it altogether, were it not for his chapter on avoidance learning. The following quotation suggests that, even enlisting the aid of conditioned negative reinforcement, he just couldn't get around it any other way.

> "In avoidance the conditioned and unconditioned aversive stimuli are separated by an appreciable interval of time... A rapidly approaching object precedes painful contact. The sputter of the fuse precedes the explosion of the firecracker... When stimuli occur in this order, the first stimulus becomes a conditioned negative reinforcer, and henceforth any action which reduces it is strengthened through operant conditioning." (1953, p. 176)

The key to the diminution of the conditioned stimulus is the diminution of "anxiety," another concept Skinner has trouble putting his finger on. It is "... a violent emotional reaction which is characteristic of all stimuli leading to avoidance behavior." (1953, p. 178) Anxiety is an aversive emotion from which the organism attempts to escape and is, therefore, a necessary component of avoidance behavior. Without anxiety, the organism would probably not learn an avoidance problem.

The point to be stressed here is Skinner's involvement with immeasurable, unobservable concepts inferrable only by their effects on behavior. None of these is operationally defined for any reliably accurate detection, nor are any characterized in enough detail to differentiate among them. Skinner is an inductive theorist, but he seems to be engaging in sloppy practice. Common to these aforementioned concepts is their dependence on the "controlling environment" for stimulus cues by which the behaviors (the concepts) are supposedly characterized.

Punishment

Punishment is described as "withdrawing a positive reinforcer or presenting a negative." (1953, p. 185) The possible effects of punishment are three: (1) it elicits responses incompatible with the punished behavior and suppresses it, (2) it supplies its own aversive stimuli on later occasions, and thus interferes with the punished behavioral act, and (3) most importantly, "If a given response is followed by an aversive stimulus... any behavior which reduces this conditioned aversive stimulation will be reinforced." (1953, p. 188, 189)

Note again the use of stimulus reduction as reinforcement. No mention is made of drive; only of aversive stimuli. There is no goal stimulus involved, but there are hints of persisting stimulus traces.

It is often mentioned that Skinner's position on punishment is similar to Thorndike's in that punishment temporarily suppresses the response but does not permanently weaken it. This position has before and since received much experimental support.

Drive in 1966

In his Preface to the seventh printing of The Behavior of Organisms in 1966 Skinner acknowledged that his interpretation of "drive" as a third variable (first and second are stimulus and response) was in error. Comparing his own usage of the concept as a means of reference to environmental variables with Tolman's usage of it as a full-fledged intervening, cognition-filled variable, Skinner criticized both Tolman's and Hull's preoccupation with internal states (see p. xi).

His "drive" of 1938, the reader will recall, consisted of carefully calculated deprivation and satiation. By 1953, "drive" was an amorphous concept that often was related to and included emotion, anxiety, and punishment. Consequently, the status of "drive" as an operationally defined variable had become rather cloudy. Skinner had, though, for many years stuck to his conclusion that some third variable must necessarily be inferred whenever behavior (i.e., response) varied under externally constant conditions. Now in 1966 Skinner admitted, of the concept "drive" that "J. R. Kantor eventually convinced me of its dangers" (p. x). All mediating concepts of S-R theories, he was to argue, suffer from such dangers, one of the most lethal of which is that they can "serve no other function than to account for failure to relate the objective terminal events in a meaningful way" (p. xii). One might state the problem more directly: mediating concepts are simply not operationally definable either (1) in terms of stimulus and/or response or (2) in their own right.

At this juncture Skinner disassociated his Behavior of Organisms from the S-R tradition, ostensibly because he believed stimulus, response, and reinforcer were quite sufficient to "account for attending, remembering, learning, forgetting, generalizing, abstracting, and many other so-called cognitive processes" (p. xii). He considers S + R + reinforcer inclusive of all relevant input and output, so that "there is no need to appeal to an inner apparatus, whether mental, physiological, or conceptual" (p. xii). Deprivation and satiation are relegated to a sort of peasantlike relationship of fealty to the "reinforcer" element of the basic triad.

Less ostensibly, but far more basically, Skinner pounds home what one is bound to read between the lines of his Preface of 1966: "...Nor are mediating concepts observable." For Skinner, input and output ought to be real data. Obviously, what is unobservable cannot be real data.

What we see here is a full circular evolution of an interpretation of "drive," a return to the old spawning ground. It was asif the old operational definition of 1938 had taken a swing at its perpetrator "to bring him back to his senses."

Perhaps the greatest difference between Skinner's attitude toward operationally defined "drive" in 1938 and that in 1966 was that in 1938 Skinner intended to use situation-specific operational definitions of drive (i.e., degree of satiation and hours of deprivation) as a means to an end, that end being a scientific grasp of the real roots of drive-in-general and a common meaning of the term, which might still permit classification of types of drives. By 1966 Skinner had realized that there was to be no common real root nor a drive-in-general and that his original approach in its simplest form had been a correct one. The true sceintist was to define variables concretely, specifically, and operationally. This is the only way for the behaviorist to approach accuracy in prediction, measurement, and experimental control. Once again, Skinner had become the purist.

Summary

Skinner's position on reinforcement, his methods, his operational definitions have become litany to the contemporary behaviorist. Operational definitions provide their own inherent methodology and are self-perpetuating. Their usefulness lies in their purpose: meaningful observations and measurements. Skinner's system in 1938 was based on observation and measurement and his chapters on drive are methodological and observational reports. Drive in 1938 was operationally defined as a function of deprivation. By 1953 Skinner seems to have beencaught up in the quest of his contemporaries for the ultimate word on drive, emotion, avoidance, and punishment. He was best prepared to handle drive, but his method and means of attack on the other three problems (with the possible exception of punishment) were non-behavioristic. Thevariables of which such concepts were supposedly functions were vaguely defined and observable only by inference and induction. Perhaps the "Behaviorist's behaviorist" felt obligated to make a stab at these concepts, for the sake of Behaviorism, in answer to chiding from less "scientific" thinkers. If so, the challenge was, in this author's humble opinion, poorly met.

The other, more plausible interpretation of Skinner's approach to motivational theory is that he intended merely to represent the pervasiveness of the role of reinforcement and that ideas expressed in his motivation chapters are a means to that end. Correspondingly, they were not to be taken as "'My Ultimate Word on Motivation' by B. F. Skinner." The Preface to the 1966 printing of The Behavior of Organisms seems to confirm this. It is a reaffirmation of the original Skinnerian purpose: observability and predictability in the simplest, most elemental form possible. With that reaffirmation and an alleged mending of his ways, Skinner abandoned further pursuit of "drive" altogether, and returned to a behavioristic purism of observable, measurable, controllable stimulus, response and reinforcer.

Mediating variables are presumed by some to have suffered great losses in his wake. "Rescue"operations are in effect, however, under the supervision of those parties.

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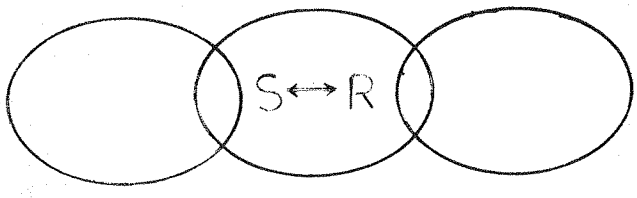

It is widely held that a stimulus event, for example, a touch or vocalization or some combination, is often treated as if it carries the same meaning regardless of context. We maintain that this is not necessarily true, and that it is necessary to study the contextual or situational setting as well as behavioral events themselves. Thus, a stimulus event may change its meaning for both infant and caregiver, depending on the situation. A simple example may suffice. The infant cries--in one case it has just been fed, in the other it is feeding time. The caregiver, through contextual cues, realizes that in the former case the infant is in need of a burp, while in the latter the infant is hungry. The behavior of the infant is the same, but the meaning of the behavior is quite different. Interestingly, comparable examples of a caregiver's behavior are not as easily found. For example, a caregiver may pick up an infant because she thinks the infant wants to be held, whereas another time the caregiver picks up the infant because she wants to hold it. At issue then is the meaning of behavior. One way this can be explored is to observe given behaviors in given contextual situations. Parenthetically, it might be mentioned that context may be very important for a developing organism. The infant may utilize behavior-context situations to learn meaning.

--Michael Lewis & Leonard A. Rosenblum: THE EFFECT OF THE INFANT ON ITS CAREGIVER, Wiley, 1974, p. xxii.