The Behavior Analyst 1991, 14, 1-13 No. 1 (Spring)

Humble Behaviorism

Allen Neuringer
Reed College

Key words: behavioral subject-matter, behavioral methods, behavioral theory, personal characteristics of behavioral scientists, humility, science

If behaviorists were more humble, their effectiveness as scientists would increase. An explicitly humble behaviorism could reduce the threat of behavioral research perceived by many in our society; meliorate fights among sub-disciplines of psychology, so that adversaries might work together toward common goals, and encourage researchers to identify and admit their own ignorance and error, thereby motivating research. This hypothesis is not meant as criticism specifically of behaviorists, for some of my behavioral colleagues are among the most humble of people, others not, as is the case in all fields. Nor is behavioral science, or science generally, unusually arrogant. Science depends upon the continual checking of one scientist's work by others. Most scientists therefore must be somewhat humble, for often their research is shown to be insufficient and their conclusions challenged. At the same time, positions can be strongly held, and activities involved in testing and disproving can be aggressive and destructive. Arrogance and humility in science coexist. The present hypothesis, however, is that humility will prove to be functional.

This paper discusses, under four overlapping categories, both areas where behaviorists' positions are humble and those where greater humility might be nurtured. The categories are: (1) subject-matter of contemporary behavioral research, (2) methods used in such research, (3) theoretical positions held by contemporary behaviorists, and (4) personal characteristics of the behavioral scientist. My aim is not to exhaust the topic but to motivate consideration of humility as one goal for behavioral researchers. "Humility" is used broadly to imply tentativeness of theoretical and methodological positions, willingness to consider alternative views, support for diversity, openness to criticism—in brief, a scientific stance that all knowledge is provisional and that one's most deeply held positions must continually be reconsidered.

SUBJECT MATTER

Behavioral Classes and Sub-Classes

Behaviorists study behaviors of organisms. The plural, behavior*s*, connotes differences among behavioral classes, such as unconditioned, respondent, and operant (Hearst, 1975; Hineline, 1986). Generalizations about one class may not hold true for another. Healthy debate continues as to how best to cut the behavioral pie, for example, does respondent behavior differ from operant and, if so, how?

The plural also implies that instances within a given behavioral class may further be divided into functional sub-classes. For example, Shettleworth (1975) showed that frequencies of some operant-like behaviors, such as grooming, may be unaffected by food presented contin-

I dedicate this paper to the memory of B. F. Skinner, my teacher and friend. He may be responsible for many of my motivations, but not my errors.

A version of the paper was presented to the Association for Behavior Analysis, Philadelphia, Pa., in May, 1988.

Martha Neuringer, Meg Wilson, Sigrid Glenn and four anonymous reviewers provided very helpful critical comments. Preparation of this manuscript was partly supported by NSF grant #BNS-8707992.

Reprints may be obtained from the author, Psychology Department, Reed College, Portland, Oregon 97202.

gent upon the behaviors; whereas, frequencies of other operants, such as lever-pressing, may readily be increased by contingent food. As another example, Garcia, Hankins, and Rusniak (1974) showed that some behaviors are controlled by events long delayed, up to hours, when illness follows the drinking of a novel solution; whereas, other behaviors are unaffected by events delayed more than a few seconds (however, see Lattal & Gleeson, 1990). Biological preparedness describes such within-class differences. Readers probably accept the implied distinctions, whether or not they use that term.

Overt and Covert

Yet behaviorists are criticized for reducing all meaningful psychological phenomena to a single class, that of behavior. Why, we are asked, do we ignore mind, psyche, and the contents of consciousness? In fact, the criticism has two horns. First, we are accused of including too much under "behavior." Some behaviorists view thought, perception, imagination, and emotion as behavioral phenomena. The rationale for so doing is that each of these involves an actively changing organism under the control of prior and consequent environmental events. An active-responder model generates explanations of "mental" phenomena analogous to those for motoric behaviors. Together with most of our contemporaries in other fields of psychology, we contrast our view of perception as activity with older views of perception as passive reception.

According to critics, however, by classifying both mental and physical under the single rubric of "behavior," we weaken the term. Meaning is based on discrimination of differences: If everything is "x," then "x" is meaningless. Our burden is to distinguish the class of events called "behavior" from other classes, and to show why it is functional to hypothesize similarities between overt (or "physical") and covert (or "mental") "behaviors." At the same time, we must consider the possibility that differences among these phenomena are sufficient that they not be lumped together. For example, some behaviorists maintain that imagining is a covert response to an unspecified stimulus; we imagine the absent box just as we see the present box. Seeing and imagining are both responses, the difference being the presence or absence of a specifiable current *external* stimulus. Others who study imagery argue, however, that seeing and imagining may be sufficiently different to require placement of the two into different sub-classes. Necker cubes reverse when looked at but do not reverse when imagined (see Chambers & Reisberg, 1985). Similarly, thinking, a covert activity, and speaking, an overt one, may be sufficiently different in terms of their dynamics and causes to require differential treatment and terminology. Behaviorists presumably would accept important differences in the classes of overt and covert, if these were demonstrated, just as they presently accept different classes within the overt domain. Until the evidence is in, humble agnosticism may be more functional than adherence to any position.

The second horn of the criticism is that some behaviorists appear to relegate the causal role of covert behaviors to second-class status. Radical behaviorists (a form of behaviorism identified with B. F. Skinner and colleagues) accept and write about such phenomena as feelings and thoughts, but often imply that they are less important for understanding overt behaviors than are the initiating environmental events. What is the empirical evidence, however, for maintaining that feelings, thoughts, and images have little or no independent causal role in the generation of overt behaviors, or that they play only a "collateral" role? These "mental" events are often closer in time to overt behavior than the initiating environmental events and thus may serve as better predictors. When I attempt to predict *my own* overt behaviors, covert states often provide helpful information: Do I have a bad headache? Have I been ruminating about a particular situation? Am I anticipating a friend's arrival? Am I depressed? Covert and overt behaviors may be of the same class or they may differ; covert behaviors may play redundant or collateral roles in the generation of overt behaviors, or they may play significant and independent roles. These are hypotheses to be tested.

The distinctions between covert and overt stimuli are also problematic and must be carefully drawn (Skinner, 1953). The lever press may be taken as an instance of an overt stimulus; different people can observe it and therefore it serves as a discriminative stimulus for saying, "That was a lever press." Lever presses are said to be "objective." But the quintessential private phenomenon, pain, may also provide "objective" data. Imagine twelve people in a room and one suddenly says "Ow, I have a tooth ache." Presumably, she is tacting a covert event. But now image a dentist's drill with twelve identical tentacles, each containing a drill. A dentist-ex-machina says "pain" and simultaneously puts the drill to a tooth in each of the twelve individuals' mouths. The drill is removed and the dentist says "no more pain." In this situation, pain is as objective (or, if you prefer, subjective) as the lever-press. Skinner has noted, correctly I believe, that a more helpful distinction is between events that occur within the skin of an organism versus without. That can be rephrased: one can be said to respond to an "objective" stimulus if the response is consistent with those emitted by other observers. One way to affect such consistency is for a number of individuals to receive discrimination training until all are emitting analogous responses. Such discrimination training may be possible with respect to "having an intention or goal," "being worried," and "feeling depressed," as well as other so-called "private states," and therefore discriminative responses to these "subjective" states would be useful in an experimental analysis of behavior.

There is another problem, however. Often the behavior analyst is not in a position to experience the conditions leading to the purported emotion, thought, rumination, feeling, or the like. Can behaviorists utilize verbal reports, in the case of human subjects, or other discriminative responses, in the case of non-human subjects, concerning subjective states? An analogous situation might be the following: a scientist and subject both have learned to respond with the word "red" to a red light and "green" to a green one. One of the two lights is randomly illuminated, the order being controlled by a process not under direct control of the scientist. The scientist asks the subject, "What color is that?" and when the subject answers, the scientist looks at the color to confirm whether the answer was correct or not. Now imagine a screen placed between scientist and subject. Again, one of the two lights, out of sight of the scientist but next to the subject, is randomly selected for illumination. The subject emits a verbal response, "red" or "green." The scientist need not devalue such data because she or he cannot also see the color.

Concurrent Behavior Systems

Also implied by the behavior*s* of organisms is that the behavior of an individual may be viewed as a combination of many different, concurrently acting behavior sub-systems. I improvise on the piano, keep the beat with my foot, hum an antiphonal melody, read the newspaper, and make funny faces—all of these occurring concurrently. Other examples abound: we walk, talk, hear, see, and scratch ourselves simultaneously. Each sub-behavior may serve as part of the controlling environment for other sub-behaviors—serve as discriminative cues and reinforcers, or mediate other functions. Thoughts may influence actions, feelings influence thoughts, one type of action influence other concurrently emitted actions, and so forth. For example, concurrent physical movement has been reported to increase the frequency of "good ideas" (Neuringer, 1981).

Cognitive behavior therapy is another example: Thoughts or self-statements influence overt acts. Imagine yourself drinking wine and then vomiting; you may be less likely to actually drink wine (Cautela, 1967). Say to yourself, "I can answer these test questions, relax, take your time, don't freak out," and you may then be more likely to complete the exam successfully (Meichenbaum, 1977). "Contingency thinking" may also influence overt behaviors. If I want to decrease candy eating, I say to myself at the point of temptation, "If I eat a candy-bar now, that will increase the probability that I eat another one tomorrow. Do I want to do that to myself?" To increase my physical activity, I say, "If I don't exercise today, that will cause me not to exercise tomorrow and the next day—do I want that to happen?" Thinking such thoughts appears to influence my overt behaviors. A variant of contingency thinking is simply to ask myself, "If I do such and such behavior, then what might the consequences be?" Prior to turning on the television, rising from my desk while writing a paper, or answering a question sarcastically, I sometimes inquire briefly as to the possible consequences of the act. The covert query, "If this behavior—then what consequence?" again seems to influence my behaviors. Without controlled study, I do not know whether contingency thinking is, in fact, effective or whether its effects are due to an interruption of the behavioral stream, or to some other confounding variable. Contingency thinking is offered as a hypothesis. Note, however, that hypotheses concerning covert events can readily be stated in a way that may lead to independent testing: if the reader engages in contingency thinking, then, according to the present hypothesis, probabilities of overt behaviors will change. That hypothesis can be tested and the results reported to the community (see, for another example, Glenn & Hughes, 1978).

Verbal Behavior

The behaviorist acknowledges differences between verbal and non-verbal behaviors, but at the same time, hypothesizes that language has much in common with other instrumental activities (Skinner, 1957). One attribute that sets language apart, according to many in the non-behavioral community, is that appropriate instances are generated without ever having been previously experienced or reinforced. It is indeed intriguing, as psycholinguists point out, that children learn correct usages (or, at some stages, systematically incorrect usages) if there is no direct reinforcement (see, however, Moerk, 1983). With psycholinguists, developmental psychologists, and cognitive psychologists, behaviorists are awed by and applaud the successes of children in learning language. But behaviorists note that other common instrumental acts, such as lever pressing and keypecking, demonstrate possibly analogous response induction and stimulus generalization. Rats trained on one type of operandum will respond appropriately to others. Pigeons trained to respond to projections of slides with people in them will respond appropriately to pictures of people they have never before seen (Herrnstein, 1984). In an extension of this work, after pigeons were trained to respond on one key to music by Bach and another key to music by Stravinsky, they generalized in ways analogous to people: they responded on the Bach key when probe pieces by Telemann or Buxtehude were introduced, and responded on the Stravinsky key when music by Eliot Carter was played (Porter & Neuringer, 1984). We applaud these behavioral accomplishments of pigeons as well as the linguistic accomplishments of children.

Another aspect of language that, according to non-behaviorists, sets it apart, and therefore sets humans apart from other species, is the infinite variability of sentences, the fact that novel sentences are continually generated and understood, sentences that may never before have been uttered or heard. How is that possible, it is asked, of conditioned behavior? Here, too, the behaviorist attempts to show how. There is variability in all operant behavior. Rats are reinforced for pressing levers, but no two responses are identical. We learn to walk in a particular way, but each of us can now walk in unpredictable fashion. Animals, such as porpoises, have been reinforced explicitly for generating novel behaviors (Pryor, Haag, & O'Reilly, 1969). Thus, a defining attribute of all instrumental behavior may be its great potential variability and consequent generativity.

Scientific Language and Translation

The behaviorists' interest in language extends to their own scientific language, and serious attempts are made to emit language that is based on empirical relationships, and to stay close to the observed data (Hineline, 1980). There has arisen, however, a correlated tendency to maintain that "our" language is better than others, for example, the language of philosophers, Freudian psychologists, and cognitive psychologists, to name a few. Language as instrumental behavior is effective or not, and its effectiveness depends on the linguistic community. Are the psychodynamic and cognitive languages effective? Workers within those verbal communities answer yes. We emit our particular scientific language for a variety of reasons, but we must subject these reasons to the same scrutiny that we do other subjects of our science.

Rather than arguing about the best language, all would profit from the more difficult but productive activity of translation. Skinner (1974) has, on many occasions, attempted to translate the language of mind into behavioral terminology. Many others have also attempted translations across disciplines (Davey, 1983; Rescorla, 1985; Seligman, 1975). Grosch and Neuringer (1981) attempted to translate between a behavioral analysis of self-control in pigeons and psychodynamic and cognitive interpretations of self-control in children. In the research with children (Mischel & Ebbesen, 1970), the experimenter left the child alone in a room. If the child rang a bell, the experimenter returned immediately and provided the child with a less preferred reward (e.g., marshmallows); if the child did not ring the bell and waited until the experimenter returned on his own, then a more preferred reward was given (e.g., pretzels). The same procedure was used to explore a number of conditions, for example, Mischel, Ebbesen, and Zeiss (1972) asked one group of children to think about a "fun" event during the wait period, and a second group to think about a "sad" event. The "fun" thinking children waited significantly longer than the "sad" thinkers. Grosch and Neuringer (1981) trained pigeons under analogous conditions where pecking a response key caused immediate presentation of less-preferred food, whereas waiting caused eventual access to a more-preferred food. An overhead light, the "think fun" stimulus, had previously been correlated with freely presented food; a different overhead stimulus, "think sad," had been correlated with timeout. For pigeons, as well as children, presenting the "fun" stimulus led to significantly longer self-control than did the "sad" stimulus. The results could be interpreted in terms either of thinking processes or conditioned reinforcers and punishers.

Translation brings differing points of view and modes of research to bear upon a given phenomenon and helps to broaden the audience. I ask my students not to talk or write in a particular way, but instead to communicate, and that often requires that they use a language grounded in observations of behaviors and events.

METHODS

Data and Theory

Behaviorists emphasize data because they distrust the selectivity and creativity of current interpretations and theories, including their own. This skepticism results in publication of precise details of procedures and results, a humble practice that hopefully will be appreciated by those who follow. Although data drive much behavioral research, behaviorists are criticized for being constrained by their commitment to theories, for example, concerning reinforcement and environmental contingencies. The objection is not to theory, but to premature theorizing, or overgeneralizing. Theories provide ideas for research, but it is easy to be seduced by their potential power, as have some of our colleagues in other fields of psychology, as well as some of us. For example, if "undermatching" and "overmatching" are as common as "matching" under concurrent schedules of reinforcement, does the theory of matching have explanatory power, or does it constrain how we think about the phenomenon of "choice"? Similarly, what is the predictive power of a mathematical description that contains many "free" parameters? In the long run, the humble approach of gathering data may best lead to attainable goals, at least at the early stages of a given science. In place of verbal or mathematical theory, one could indicate circumscribed hypotheses—if food is presented within *t* seconds of a keypeck in pigeons, then . . . , or, as Charlie Perkins (personal communication, January, 1990) has suggested, instructional rules: do this in order to get such and such an effect in a given situation.

Measurement Operations

The scientists' own discriminations define what they study. Sometimes we can specify with precision the physical characteristics of what is to us a discriminative stimulus, such as a lever press, and therefore automate its measurement. Other times, however, there may be intersubjective agreement without our being able to specify the precise stimulus. The preferential looking method provides one example from the research laboratory. Human (Teller, Morse, Borton, & Regal, 1974) and monkey (Lee & Boothe, 1981) infant sensory capacities are evaluated by showing the infants two cards on a grey background, one card being an identical grey color as the background, the other consisting of black and white stripes. Infants have a tendency to look toward novel and patterned stimuli, and thus look at the striped, rather than the solid gray, card. Stripe frequency (or stripe width) is varied systematically, thereby enabling specification of visual acuity. When the stripe widths are sufficiently small, the striped card is perceived (by you or me, and therefore presumably by the infants) as a grey, identical to the background, and consequently there is no preferential looking. Importantly for the present point, human observers collect the data by judging whether the infant looked at the left or right card. It would be possible to collect "objective" data concerning the motion of the infant's eyes and head, but a global estimation by a human observer appears to be the better means of gathering the data: the noise of the infant's movements, and the lack of explicit definition of when a "look" occurred, makes it difficult to define the exact stimulus—but there is high reliability between observers judging the direction of the look.

Other examples abound of instances in which reliable human discriminations can be made although the exact bases of the discriminations are not known. Absence of knowledge concerning effective stimulus control does not preclude intersubjective agreement. If such agreement exists, then phenomena such as awareness, attention, desire, interest, contentment, or striving may readily be accessible to behavioral analysis: An observer's response, human or non-human, serves to operationally define the dependent variable.

Methodological Consistency and Basic Questions

What basic behavioral researchers study is guided by available behavioral methodology, and as a result, they tend to choose questions which can be studied in the operant chamber. One reason for so circumscribing the field is that we are unable to read all of the psychological literature, be conversant with all methods, or teach all of psychology in our courses. But selection based on methodological consistency—look only at journals which emphasize *N*-of-1 research, or in which rates of responding in operant chambers are the dependent variable—may be less effective than selection based upon the questions. If your goal is to create a pure behavior analytic science, then possibly ignore work on human preference judgments. On the other hand, if your goal is to explain choice behavior, then attempt to relate Kahneman and Tversky's findings (e.g., Kahneman, Slovic, & Tversky, 1982) to those from concurrent schedules of reinforcement, as is being attempted by Rachlin (1989). Method-oriented research may have the inadvertent consequence of inverting the processes of science: Questions are asked because they can be studied, rather than methods chosen to answer questions.

Where important questions come from is not dictated by scientific method or any other formal means. The questions have a variety of sources (see Beveridge, 1950). As suggested above, a major source has been laboratory research. Another is society, that is, the perception of societal problems. Development of behavioral techniques to deal with such issues has been one result (Greene, Winett, Van Houten, Geller, & Iwata, 1987). A third source is our own behaviors and problems, a source that has led me to self-experimentation, a field with a long history both in medicine and in psychology (Altman, 1987; Franklin & Sutherland, 1984; Neuringer, 1981, 1984). My students have self-experimented on questions concerning sleep, fear of speaking, sex, self-control, addiction, study performance, memory, social interactions, circadian rhythms and the like.

As with all research, a self experiment may lead to questions appropriate for study using different methods, for example, "other" experimentation, using other people or non-human animals. Skinner's description of stimulus control over his professional work (Skinner, 1987) led to similar attempts by others. Farley Mowat's (1963) description of multiple short sleep periods led students to try the same. And studies of my own ability to generate random numbers has led to the experimental analyses of operant variability in rats, pigeons, and other people (Cohen, Neuringer, & Rhodes, 1990; Neuringer, 1986; Page & Neuringer, 1985).

Individual Designs and Scientific Play

Behaviorists emphasize data from *individual* organisms, partly because group averages may not predict an individual's behavior, and partly because each step in the research evolves from the individual subject's performance under preceding conditions. Experimental manipulations are often motivated by the research process itself. This feedback-loop, as documented in Ferster & Skinner (1957) among others, can be characterized as scientific play—and the term is used in its best sense. No more important advice can be given to a young scientist than to learn to play with his or her subject-matter. Play around and, depending upon what you learn, play some more.

One result of scientific play is to increase the probability of serendipitous findings. Skinner discovered control by reinforcement schedules, James Olds discovered reinforcing intracranial brain stimulation, Flemming discovered penicillin, Roentgen discovered x-rays, Galvani discovered reflex action due to electrical stimulation—the list of "accidental" discoveries is long. While the discoverer must prepare him or herself for such chance findings, often the discovery process is neither pre-planned nor rational (see Root-Bernstein, 1988).

Scientific play also helps to generate intuitions, that is, a sense of subject that is neither rational nor verbal. Einstein described the discovery process as involving kinaesthetic body feels; he first felt the solution to a difficult problem and only then worked at translating those feelings into equations or words (Ghiselin, 1952, pp. 43–44). Poincaré hypothesized that his discoveries involved an initial period of hard work followed by a non-conscious, intuitive process of selection based on the relative aesthetics of the ideas (Ghiselin, 1952, pp. 33–42). Science may be the art of generating verifiable intuitions.

Whether scientific play should be carefully documented is a question of heuristics—is it helpful to fill journal pages with descriptions of play? Do the end results benefit from descriptions of initial processes? Documentation (and formal analyses of obtained results) at early stages of play-research may be inhibiting and non-functional. For one reason, it slows the process. For another, it inhibits unplanned, "non-rational" changes in procedure. Notes to oneself might suffice.

Group Designs

The demonstrated worth of single-subject research (Barlow & Hersen, 1984) does not imply, however, that group designs are unhelpful. Group designs are necessary to study gender differences, effects of prenatal malnutrition, irreversible diseases, or other developmental or inherited characteristics. The point here is a general one: Because X is valid or functional does not imply that Y or Z are invalid or non-functional. J. S. Mill captured this point when he wrote, "[The] . . . danger is not so much of embracing falsehood for truth, as of mistaking part of the truth for the whole" (cited in Boakes, 1984, p. 174). Single-subject and group designs complement one another, each leading to predictions to be tested within the other's domain. One problem with single subject research is the unknown representativeness of the particular subject. The only way to find out is by testing other subjects, leading to a group design. A related problem is the unknown contribution of the history of a given subject prior to the experiment. The ABA design permits assessment of the effectiveness of a particular experimental manipulation, but its effectiveness may vary with prior history. Furthermore, although feedback-loop research has, indeed, been correlated with Skinner's single-subject studies, group researchers can engage in analogous forms of scientific play. It may be more difficult to treat a group as an individual, and to manipulate procedures based on, say, the average performance of the group, but it is possible and might help the process of group study.

Induction from Animals to People

Behaviorists hypothesize that human actions may be modeled by animals in operant chambers. The emphasis here is on the last word in the phrase, "behaviors of organisms." People are animals who share much with our animal cousins. However, we are tentative about inducing from the animal laboratory to the human sphere, tentative because we are as ready to document differences among animals—including human—as homologies and analogies. Thus, we need continually to go back and forth between animal model and human application in order to validate the model. The study of the behavioral effects of drugs (Carlton, 1983) and of uncontrollable aversive events (Seligman, 1975) provide important examples of interactions between animal and human levels.

Empathy

"Objectivity" is associated with behavioral studies. But objectification of behavior should be the standard only so far as it is functional. If attempting to "put yourself in the place of a rat" helps us to understand, predict, and control the rat's behavior, then we should do so. Practitioners in biology, chemistry, and physics do that sort of thing: They imagine themselves as bacteria, organic molecules, or atomic particles (Judson, 1980). Empathy may turn out to be as functional in the experimental analysis of behavior as it is in human social interactions.

Empathy may play another functional role. Occam's razor and Lloyd-Morgan's canon have served psychologists well: Do not assert of another organism some capacity more intricate or complex than is necessitated by the immediate data. This point of view may be referred to as the simplicity hypothesis, that is, assume that an organism's capacities and behaviors are as "simple" as required by the evidence at hand. There is, however, an alternative point of view which may also be functional: Within the limits of the evidence available, hypothesize that another organism can do what you, yourself, can do. This can be referred to as the similarity hypothesis. Both simplicity and similarity hypotheses must lead to experimental tests, but they work in opposite directions. Simplicity works from the bottom up through a series of proofs or demonstrations of function; similarity leads to a series of attempts at disproof, a series of demonstrations that the organism fails to perform under a set of tasks and demands. Eventually, both similarity and simplicity strategies should lead to the same identification of function, one getting there from top down, the other from bottom up (see Porter & Neuringer, 1984). Which hypothesis to use is not a question of right and wrong, but of scientific heuristics.

THEORETICAL ISSUES

External Versus Endogenous Determinants

Behaviorism is associated with the philosophical position of determinism: Behaviors are hypothesized to be functionally related to events, with those events external to the behaving organism most helpful in predicting and controlling behavior. Of course, genetic determinants are important, on the one hand, and external events may initiate a long sequence of neural responses or covert occurrences, on the other, but to gain control over behavior, behaviorists emphasize control by external stimuli.

Determinism is thought to be a functional position. If scientists assume that all behaviors are controlled by external variables, then current ignorance will spur them to try to identify those variables. The opposite point of view, we are told, leads to giving up the search. But entertain for a moment two alternatives: first, that endogenous mechanisms—those which may function relatively independently of external environmental stimuli—exert important behavioral control, for example, timing oscillators responsible for circadian rhythms and other timing functions (e.g., Meck, Church, & Gibbon, 1985; Roberts, 1981). Here, knowledge of environmental events would not suffice to predict or control behavior. Second, and more problematic, is the possibility of endogenous random generators, that is, influences internal to the organism which contribute randomly to behavioral output. In physics, chemistry, and biology, the possibility of truly random events is accepted. Although some physicists argue that hidden variables account for the apparent randomness of quantum phenomena, others hypothesize a completely random or unpredictable underlying nature. A scientist can posit causal relationships while entertaining the possibility of endogenous control, random influences, or both.

This last assertion is not as far from the radical behaviorist's position as might appear. Behaviorists describe relationships between environment and responses in terms of response probabilities. Especially with respect to operant behaviors, where we talk of response emission—as opposed to elicitation—we acknowledge an underlying uncertainty or variability of response. One can reasonably *hypothesize* that the source of such random variation lies within the organism. As indicated above, pigeons and rats respond quite adequately when variable sequences of responses are reinforced (Page & Neuringer, 1985) and humans learn to generate number sequences which are indistinguishable, according to a subset of statistical tests, from that of a computer-based random generator (Neuringer, 1986).

There are two major classes of explanation for such random-like behavior, chaotic and stochastic. A chaotic basis of random behavior (see Gleick, 1987) is consistent with philosophical determinism, but, at the same time, provides trouble for those whose goal is to relate all behavior to environmental causes. Many readers are familiar with computer-based random numbers generated from simple, deterministic equations, whose outputs meet many of the statistical requirements of randomness. There are two aspects of such random outputs: First, the output indeed appears to be random according to statistical evaluations. Second, if you know the generating equation and the position in the sequence, you can predict with certainty the next instance. Assuming that animals and people do, in fact, contain an internal mechanism for generating highly variable outputs, the mechanism employed may be a chaotic one, that is, based on an underlying deterministic process. The output would be random-*like,* but, with complete knowledge, every instance could be predicted.

There remains, however, a problem for the determinist-behaviorist whose goal is to predict and control. It may be impossible to learn the underlying generating mechanism from the output of a chaotic generator. That is, if you already know the chaotic mechanism, you may be able to predict instances, but it may be impossible to discover what that mechanism is. There simply are too many (possibly infinite) alternative ways to generate chaotic outputs. Thus, we could be secure in the belief that every instance of behavior is determined, in part by the workings of a chaotic generator, but unable to discover its characteristics and therefore unable to predict instances.

The second class of possible explanations involves a stochastic, or "truly random" generator. As an analogy, imagine the atomic emissions of radioactive particles. According to physicists, there is no way—empirically or theoretically—to predict the exact time of emission of the next particle. One can readily describe the general function, but not the instance. If we have such an internal random generating mechanism, and it somehow influences which among a set of responses is to be emitted, then, empirically and theoretically, it will be impossible to predict instances (see Peirce, 1923).

That aspects of behavior are stochastic, or probabilistic, is consistent with much of radical behavioral writings. That the stochasticity may imply an inability to predict and control goes, however, against a major goal of the science—as indicated by Zuriff (1985), among others. Control and prediction may be possible in some cases, but understanding may be the realistic goal in others (see Boakes, 1984, for analogous positions held by early functionalists). By understanding is meant the ability to specify the relative contributions of endogenous versus external influences or predict the circumstances in which unpredictable behaviors are likely to occur—for instance, when a prey is chased by a predator. If prediction is used in its broadest sense, then science always involves some aspect of prediction, but if it is intended to imply instances, then many behavioral events may be unpredictable. That conclusion need not inhibit a humble science of behavior.

Reinforcement

Operants are reinforced; they are selected by their consequences. However, in any particular instance, reinforcers must be identified, not assumed. As has been pointed out by many researchers, the intermittent presentation of food or water to a food- or water-deprived animal may elicit patterns of behavior which appear to be operant (Neuringer, 1970; Skinner, 1948; Staddon & Simmelhag, 1971). Instrumental acts may evolve behaviorally in ways analogous to the selection of neutral phenotypes (Kimura, 1983). Such behaviors are not reinforced; they occur for other reasons. There is another way in which we could be more tentative with respect to the role of reinforcement. By speaking of the reinforcement of operant responses, we imply a uniformity which may or may not exist. Until the data compel generalizations, it is safer to speak descriptively: The animal's lever press was followed by food pellets, rather than the animal's operant behavior was reinforced.

PERSONAL CHARACTERISTICS

Relations with Other Psychological Disciplines

Important research is performed in other areas of psychology: physiological, cognitive, perceptual, developmental, and social, to name a few. We hinder our contributions to a science of behavior by not taking that research seriously. For example, research on event-related evoked brain potentials, reaction time, psychological changes occurring in infants when they begin to explore their environments, and the consequences of being in a group versus being alone are a few of the areas that may assist us. Our attention to other research areas increases the probability that others will attend to our work: Interest is presently being shown by ecologists and economists to ecologically-relevant (Fantino & Abarca, 1985) and economically-relevant (Rachlin, Battalio, Kagel, & Green, 1981) operant studies. A humble stance with regard to other disciplines—asking for help in solving our problems—may, in the long run, serve all better than a continuation of the "you're wrong/I'm right" battles.

Constructive Criticism

As suggested at the outset, science depends upon continual checking and criticizing. However, criticism can be more or less aversive. Yerkes and Dodson (1908) suggested that the more complex the behavior, the less aversive a punishing stimulus need be to effect desired changes (see, also, Balaban, Rhodes, & Neuringer, in press). Doing science is complex behavior par excellence, and therefore mutual support, especially when we are being critical, is helpful in changing behaviors. Criticize the research or theory, don't punish the individual, and criticize constructively (Lyons, 1976). One form of constructive criticism is: When you do or say "A," that results in "B," the consequence of which is "C"; rather than "You're wrong," or "That position is harmful." Along with constructive criticism, the humble behaviorist accepts and reinforces small accomplishments.

Behavioral Ethics

Humble behaviorists attempt to substitute if-then contingency statements for the more easily uttered "rights" and "wrongs," and "oughts" and "shoulds." In place of "You ought not use introspective reports," we try to substitute, "If you use introspective reports then. . . ." This is a scientific ethic—one suggested by Reichenbach (1951) and others. Statements of the prescriptive form—Do not speed—contain either implicit or explicit contingencies. The behaviorist's goal is to specify the contingencies, through research. Given insufficient knowledge concerning the contingency, a humble behavioral scientist is silent, admits ignorance, or specifies what he, she, or others do in the situation.

CONCLUSION

In conclusion, behaviorists are asked to cultivate a humble science of behavior. Self-assuredness, aggressiveness, guerrilla warfare, methodological consistency, and philosophical purity may be effective in the short run. Humility is difficult when fighting for grant funds, laboratory space, research support, faculty positions, and students. Nevertheless, over the long run, humble behaviorism may be most likely to effect our scientific and social goals.

Four objections are anticipated: (a) "Humble" behaviorism blurs the distinction between behavioral research and other fields. (b) The author defines "humble" behaviorism in terms of his own research. (c) He writes about constructive criticism but attacks behavioral practices. (d) Anyone who admonishes humility cannot be very humble. Each of these criticisms will be considered briefly.

(a) There is much overlap between the "field" of behavioral research and other areas. The humble behaviorist's goal is to discover laws and solve problems (together, whenever possible, with colleagues from different "fields") rather than to prove the correctness of his or her own field or philosophical commitment. If humble behavioral practices yield scientific and social progress, they will survive the test of time.

(b) I chose examples from areas I know best. Readers might try to strengthen the argument that contemporary behaviorism is, in fact, a humble science, and describe other ways and reasons to increase our humility as researchers and practitioners.

(c) It is difficult to criticize those closest to you. If any of these ideas is worthy, I ask the reader's help to transmit them in a way that does not alienate.

(d) Finally, Golda Meir told an associate (cited in Safire & Safir, 1982, p. 162): "Don't be so humble. You're not that great." The humble behaviorist accepts Golda Meir's challenge by attempting to complete the contingency statement, "If behaviorists were more humble, then . . ."

REFERENCES

Altman, L. K. (1987). *Who goes first? The story of self-experimentation in medicine,* New York: Random House.

Balaban, M. T., Rhodes, D. L., & Neuringer, A. (in press). Orienting and defense responses to punishment: Effects on learning. *Biological Psychology.*

Barlow, D. H., & Hersen, M. (1984). *Single case experimental designs.* New York: Pergamon.

Beveridge, W. I. B. (1950). *The art of scientific investigation.* New York: Vintage.

Boakes, R. (1984). *From Darwin to behaviourism.* Cambridge: Cambridge University Press.

Carlton, P. L. (1983). *A primer of behavioral pharmacology.* New York: Freeman.

Cautela, J. R. (1967). Covert sensitization. *Psychological Reports, 20,* 459–468.

Chambers, D., & Reisberg, D. (1985). Can mental images be ambiguous? *Journal of Experimental Psychology: Human Perception and Performance, 11,* 317–328.

Cohen, L., Neuringer, A., & Rhodes, D. (1990). Effects of ethanol on reinforced variations and repetitions by rats under a multiple schedule. *Journal of the Experimental Analysis of Behavior, 54,* 1–12.

Davey, G. C. L. (Ed.). (1983). *Animal models of human behavior.* New York: John Wiley.

Fantino, E., & Abarca, N. (1985). Choice, optimal foraging, and the delay-reduction hypothesis. *Behavioral and Brain Sciences, 8,* 315–330.

Ferster, C., & Skinner, B. F. (1957). *Schedules of reinforcement.* New York: Appleton-Century-Crofts.

Franklin, J., & Sutherland, J. (1984). *Guinea pig doctors: The drama of medical research through self-experimentation.* New York: William Morrow.

Garcia, J., Hankins, W. G., & Rusniak, K. W. (1974). Behavioral regulation of the milieu interne in man and rat. *Science, 185,* 824–831.

Ghiselin, B. (Ed.). (1952). *The creative process.* New York: New American Library.

Gleick, J. (1987). *Chaos.* New York: Viking.

Glenn, S. S., & Hughes, H. H. (1978). Imaginal response events in systematic desensitization: A pilot study. *Biological Psychology, 7,* 303–309.

Greene, B. F., Winett, R. A., Van Houten, R., Geller, E. S., & Iwata, B. A. (Eds.). (1987). *Behavior analysis in the community.* Lawrence, KA: Journal of Applied Behavior Analysis.

Grosch, J., & Neuringer, A. (1981). Self-control in pigeons under the Mischel paradigm. *Journal of the Experimental Analysis of Behavior, 35,* 3–21.

Hearst, E. (1975). The classical-instrumental distinction: Reflexes, voluntary behavior, and categories of associative learning. In W. K. Estes (Ed.), *Handbook of learning, and cognitive processes: Vol. 2. Conditioning and behavior theory* (pp. 181–223). Hillsdale, NJ: Erlbaum.

Herrnstein, R. J. (1984). Objects, categories, and discriminative stimuli. In H. L. Roitblat, T. G. Bever, & H. S. Terrace (Eds.), *Animal cognition* (pp. 233–261). Hillsdale, NJ: Erlbaum.

Hineline, P. N. (1980). The language of behavior analysis: Its community, its functions, and its limitations. *Behaviorism, 8,* 67–86.

Hineline, P. N. (1986). Retuning the operant-respondent distinction. In T. Thompson & M. D. Zeiler (Eds.), *Analysis and integration of behavioral units* (pp. 55–79). Hillsdale, NJ: Erlbaum.

Judson, H. F. (1980). *The search for solutions.* New York: Holt, Rinehart & Winston.

Kahneman, D., Slovic, P., & Tversky, A. (Eds.). (1982). *Judgment under uncertainty: Heuristics and biases.* Cambridge: Cambridge University Press.

Kimura, M. (1983). *The neutral theory of molecular evolution.* Cambridge: Cambridge University Press.

Lattal, K. A., & Gleeson, S. (1990). Response acquisition with delayed reinforcement. *Journal of Experimental Psychology: Animal Behavior Processes, 16,* 27–39.

Lee, C., & Boothe, R. G. (1981). Visual acuity development in infant monkeys (*Macacca nemestrina*) having known gestational ages. *Vision Research, 21,* 805–809.

Lyons, G. (1976). *Constructive criticism.* Berkeley, CA: Issues in Radical Therapy.

Meck, W. H., Church, R. M., & Gibbon, J. (1985). Temporal integration in duration and number discrimination. *Journal of Experimental Psychology: Animal Behavior Processes, 11,* 591–597.

Meichenbaum, D. (1977). *Cognitive-behavior modification.* New York: Plenum Press.

Mischel, W., & Ebbesen, E. B. (1970). Attention in delay of gratification. *Journal of Personality and Social Psychology, 16,* 329–337.

Mischel, W., Ebbesen, E. B., & Zeiss, A. (1972). Cognitive and attentional mechanisms in delay of gratification. *Journal of Personality and Social Psychology, 21,* 204–218.

Moerk, E. L. (1983). *The mother of Eve—As a first language teacher.* Norwood, NJ: Ablex.

Mowat, F. (1963). *Never cry wolf.* New York: Atlantic Monthly Press.

Neuringer, A. J. (1970). Superstitious key pecking after three peck-produced reinforcements. *Journal of the Experimental Analysis of Behavior, 13,* 127–134.

Neuringer, A. (1981). Self-experimentation: A call for change. *Behaviorism, 9,* 79–94.

Neuringer, A. (1984). Melioration and self-experimentation. *Journal of the Experimental Analysis of Behavior, 42,* 397–406.

Neuringer, A. (1986). Can people behave "randomly?": The role of feedback. *Journal of Experimental Psychology: General, 115,* 62–75.

Page, S., & Neuringer, A. (1985). Variability is an operant. *Journal of Experimental Psychology: Animal Behavior Processes, 11,* 429–452.

Peirce, C. S. (1923). *Chance, love and logic.* New York: Barnes & Noble.

Porter, D., & Neuringer, A. (1984). Music discriminations by pigeons. *Journal of Experimen-*

tal Psychology: Animal Behavior Processes, 10, 138–148.

Pryor, K. W., Haag, R., & O'Reilly, J. (1969). The creative porpoise: Training for novel behavior. *Journal of the Experimental Analysis of Behavior, 12,* 653–661.

Rachlin, H. (1989). *Judgment, decision, and choice.* New York: W. H. Freeman.

Rachlin, H., Battalio, R., Kagel, J., & Green, L. (1981). Maximization theory in behavioral psychology. *Behavioral and Brain Sciences, 4,* 371–417.

Reichenbach, H. (1951). *The rise of scientific philosophy.* Berkeley: University of California Press.

Rescorla, R. A. (1985). Pavlovian conditioning analogues to Gestalt perceptual principles. In F. R. Brush & J. B. Overmier (Eds.), *Affect, conditioning, and cognition* (pp. 113–130). Hillsdale, NJ: Erlbaum.

Roberts, S. (1981). Isolation of an internal clock. *Journal of Experimental Psychology: Animal Behavior Processes, 7,* 242–268.

Root-Bernstein, R. S. (1988). Setting the stage for discovery. *The Sciences, 28*(3), 26–34.

Safire, W., & Safir, L. (1982). *Good advice.* New York: Times Books.

Seligman, M. E. P. (1975). *Helplessness.* San Francisco: W. H. Freeman.

Shettleworth, S. J. (1975). Reinforcement and the organization of behavior in golden hamsters: Hunger, environment, and food reinforcement. *Journal of Experimental Psychology: Animal Behavior Processes, 1,* 56–87.

Skinner, B. F. (1948). "Superstition" in the pigeon. *Journal of Experimental Psychology, 38,* 168–172.

Skinner, B. F. (1953). *Science and human behavior.* New York: The Free Press.

Skinner, B. F. (1957). *Verbal behavior.* New York: Appleton-Century-Crofts.

Skinner, B. F. (1974). *About behaviorism.* New York: Knopf.

Skinner, B. F. (1987). How to discover what you have to say: A talk to students. In B. F. Skinner, *Upon further reflection* (pp. 131–143). Englewood Cliffs, NJ: Prentice-Hall.

Staddon, J. E. R., & Simmelhag, V. L. (1971). The "superstition" experiment: A reexamination of its implications for the principles of adaptive behavior. *Psychological Review, 78,* 3–43.

Teller, D. Y., Morse, R., Borton, R., & Regal, D. (1974). Visual acuity for vertical and diagonal gratings in human infants. *Vision Research, 14,* 1433–1439.

Yerkes, R. M., & Dodson, J. D. (1908). The relation of strength of stimulus to rapidity of habit-formation. *Journal of Comparative Neurological Psychology, 18,* 459–482.

Zuriff, G. E. (1985). *Behaviorism: A conceptual reconstruction.* New York: Columbia University Press.