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Can the Experimental Analysis of Behavior Rescue Psychology?

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An Editorial Note by Nicholas Wade (1982) in the *New York Times* called "Smart Apes, or Dumb?" read in part as follows:

> In the May issue of *Psychology Today,* 11 of "the best minds in the field" describe what each considers to be "the most significant work in psychology over the last decade and a half." The results are astonishing: it would seem that there has been none.
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> "Significant work" implies work generally agreed to be important, but the 11 Best Minds in psychology agree on hardly anything. Stanley Milgram of the City University of New York hails the teaching of sign language to apes as an enduring recent achievement. But another contributor, Ulric Neisser of Cornell, cites as important the evident *failure* to teach sign language to apes.
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> B. F. Skinner, alleging himself not well informed of recent progress in other fields of psychology, recounts the advances in behavioral psychology, which he pioneered. But two other sages, Jerome Bruner of the New School for Social Research and Richard Lazarus of Berkeley, laud the escape from Skinnerian psychology as the major achievement of the period . . .
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> Almost the only recent achievement hailed by more than one contributor is the discovery of endorphins, the brain's natural painkillers. This is certainly an interesting development, but the credit belongs to pharmacologists and physiologists; psychology had little to do with it.
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> The failure of the 11 psychologists to agree on almost anything evinces a serious problem in their academic discipline. Physicists or biologists asked the same question would not concur on everything but there would be a substantial commonality in their answers. Can psychology be taken seriously as science if even its leading practitioners cannot agree on recent advances?

That is a strong indictment of an established science in a prestigious newspaper. Unfortunately, many of us who have called ourselves psychologists will agree with much of it. Psychology as a science is, in fact, in shambles. And unwittingly two of the contributors to *Psychology Today* have, I think, explained why. As Jerome Bruner puts it, there has been a "continued movement . . . away from the restrictive shackles of behaviorism."

Bruner had broken away from similar restrictive shackles once before. Three members of the Department of Psychology at Harvard (Bruner, Harry Murray, and Gordon Allport) did not feel that their students should have to submit to standards imposed by S. S. Stevens and E. G. Boring, particularly a strong examination in statistics, and therefore left the Department and joined sociologists and cultural anthropologists in a new Department of Social Relations. That mistake, repaired only a quarter of a century later, was a curious anticipation of what is now happening to psychology as a whole. Those who so happily announce the death of behaviorism are announcing their own escape from the canons of scientific method. Psychology is apparently abandoning all efforts to stay within the dimensional system of natural science. It can no longer define its terms by pointing to referents, much less referents measurable in centimeters, grams, and seconds. It has returned to a hypothetical inner world. Bruner boasts of having rejoined the philosophers in the study of mind, language, values, and perception. Rollo May is pleased that "psychology has moved into matters that used to be left to poetry," and Philip Zimbardo suggests that cognitive science may now consider implanting a little soul.

There is no doubt of the freedom which is thus enjoyed. A great many things can be talked about when standards are less rigorous. The field of psychology has expanded enormously. The very divisions of the American Psychological Association suggest the current range—childhood

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development, personality, social issues, arts, clinical and other counseling, industry, education, public service, the military, aging, rehabilitation, philosophy, community, humanism, mental retardation, ecology, family services, health, psychoanalysis, law . . . And a new feature of the *American Psychologist* is devoted to public policy. Unfortunately, as psychology has expanded in this way it has moved farther and farther away from anything that is called science.

One can admire the concern and compassion which lead people to consider these matters, and one can acknowledge the practical usefulness of much of what they say. One can admit that at the present time it is not always easy to say more in a scientific way; that has been true of all the sciences, especially in their early stages. There is still a part of human behavior with respect to which one must simply do one's best with available resources. But if we are ever to do better, if concern and compassion are ever to be matched by achievement, it will be with a science of human behavior, and psychology once considered itself that science.

Part of what was called psychology has been lost to other fields. As the *Times* noted, the discovery of endorphins may be an advance, but it can scarcely be attributed to psychology, and physiology has taken over much more of the old territory. A field which once bestowed respectability on psychology—the study of sense organs in the name of the elements of consciousness—is now part of physiology and is studied with the instruments and methods of biological science. Psychologists like Lashley, Hebb, and Klüver studied the brain, using a mixture of psychological and physiological methods, but neurology and biochemistry have taken over that field. In short, certain parts of the human organism are now being studied, as they should be, with the methods and concepts of physical and biological science.

That does not mean that cognitive psychologists have abandoned the brain. A touch of physiology seems to save them from dualism, and many of them use "brain" and "mind" interchangeably. Freud took a similar position much earlier. He assumed that we should some day know what the ego, superego, and id, the conscious, preconscious, and unconscious, and all the dynamisms *really were* in neurological terms. Chomsky (1980) has denied any ontological import in his references to mind (in other words, he does not claim to know the nature of the stuff of which it is made). Rather, he is concerned with an "abstract characterization of the properties of certain physical mechanisms." (His comment that they are "almost entirely unknown" will be challenged by many physiologists,) In that issue of *Psychology Today* the cognitive psychologists are less hesitant about ontology. Bruner, for example, tells us that the mind is here to stay, presumably never to be replaced by a neurological account.

When statements about mind are offered as statements about a model of what will eventually, be described in physical terms, we must ask whether it is the right model. There is good reason to believe that it is not. It is derived, of course, from the computer, which spurred the revival of cognitive science. The human organism, like a computer, is said to store copies of the external world (as "representations") and to process them according to rules which are either part of a genetic endowment or learned from experience. As I have pointed out elsewhere (Skinner, 1975), representations and rules may be nothing more than fanciful internalizations of contingencies of reinforcement. Behavior occurs in a given setting; the organism is thereby changed and will behave differently in a similar setting later on. There is no evidence whatsoever that it stores a copy of the setting or of the contingent relations among setting, behavior, and consequence. The external world remains where it has always been—outside the organism. Rules *describe* contingencies; they are not to be found *in* them or in the organism which they have changed.

In following the Pied Piper of cognitive science, psychology has lost its hold upon reality. It is therefore more than ever subject to the whims of fashion, to revisions and reconsiderations, and to controversy. It is not surprising that it has made so little progress. For more than a quarter of a century we have been promised a new discipline that would tell us what we have always wanted to know about knowledge and thought. The promise has, I believe, not been kept. The freedom—the license—which cognitive science enjoys has been costly.

The experimental analysis of behavior, in contrast, is steadily building upon its past and proceeding in a reasonably ordered way to embrace more and more of what people are actually doing in the world at large. But has it also serious flaws? Certainly there have been rumblings. The Brelands' paper, "The Misbehavior of Organisms," was an early example (1961). Herrnstein's "The Evolution of Behaviorism" was another (1977). And what about the Garcia Effect? Or autoshaping? And cannot all learned behavior be brought under the rubric of associationism?

Some of these issues arise from a misunderstanding of the relation between operant conditioning and natural selection. Contrary to the beliefs of many ethologists, behaviorists do not deny that some behavior is innate, but the contribution of genetics surely needs to be made clear. A plausible account of the evolution of behavior might run as follows: Natural selection is responsible both for internal processes like digestion and respiration and for certain necessary interactions with the environment. In more complex environments more complex features of anatomy and physiology and also more complex repertoires of behavior would have evolved. (I have suggested (Skinner, 1975) that plate tectonics, or continental drift, may explain some extraordinary examples of behavior which could not have appeared full-blown as variations to be selected by their contribution to survival.) But behavior arising only from natural selection is not always effective in new environments. A means of making slight changes in behavior during the lifetime of the individual must have had survival value, and the processes of respondent and operant conditioning could evolve. (Along with the process of operant conditioning, there must also have evolved a susceptibility to particular kinds of consequences.)

The first contribution of the evolving process of operant conditioning may have been simply this support of phylogenic behavior. Neil Peterson (1960) showed that a young duckling not only tends to follow its mother or any large object but that following is reinforced by the increasing proximity. In that example operant conditioning functions as a redundant mechanism having the same effect as natural selection; the combined result is that the duckling is more likely to stay close to its mother. We need not assume, however, that the consequences of phylogenic behavior are always reinforcing. Fish and insects copulate in ways which have emerged through natural selection, but they are not necessarily having fun.

Once a susceptibility to reinforcement had arisen, behavior would be reinforced which had no survival value. As Peterson showed, a duckling will peck a key if a peck brings an imprinted object closer. When sexual contact became reinforcing, new forms of sexual behavior, such as masturbation or homosexuality, emerged which had no survival value. Instances of such behavior still puzzle some of those concerned with the evolution of behavioral repertoires. A species in which operant conditioning has become highly effective has less need for a phylogenic repertoire, and conditioning may then take over, as it has done most extensively in the human species.

The Breland Effect. A good example of the failure to understand the interaction between natural selection and operant conditioning is the use which has been made of the interesting facts reported by the Brelands (1961). When Keller Breland first told the Harvard "Pigeon Staff" about them in 1960, we were impressed. Contrary to certain claims, we were far from "disturbed." Apparently an organism which has repeatedly manipulated an object as a token will sometimes begin to treat it like an object found in its natural habitat. There is no reason why, upon occasion, phylogenic behavior should not intrude in this way upon ontogenic. Certainly intrusions in the other direction are common enough. Civilization shows the extent to which operant conditioning has suppressed phylogenic tendencies.

Superstition. The effect of an accidentally contingent reinforcer offers some of the best evidence of the power of operant conditioning, and possibly for that reason it has been challenged—as, for example, by Staddon and Simmelhag (1971). The behavior is said to drift toward phylogenic forms. I am quite sure of my original observation (Skinner, 1948). I have repeated it many times, often as a sure-fire lecture demonstration. Deliver food every twenty seconds to a hungry pigeon and it will soon exhibit a food-getting ritual of unpredictable topography. I see no reason why there should not be a drift toward phylogenic behavior. It would be something like the Breland Effect unopposed by operant contingencies.

"Misleading" Simplifications. In all the experimental sciences it is a fundamental practice, when studying one process, to eliminate all others which may affect the data. Chemists use pure substances for obvious reasons. Physicists hold irrelevant variables constant. The experimental space used in analyzing behavior is as free as possible of distracting influences including the releasers of innate behavior. Barry Schwartz (1981) has drawn a strange conclusion from this. Operant conditioners, he says, "capture the behavior of pigeons and rats in laboratory environments by eliminating possible biological influences." He goes on:

> The experimental chamber generally seems to prevent the occurrence of behaviors like these; hence the claim that it reveals universal principles. One must wonder, however, about whether any situation which prevents the occurrence of behaviors as powerful as these is not fundamentally distorting our understanding of the principles of behavior. It seems that if the conditioning chamber in fact prevents these sorts of species-typical behavior patterns, it cannot be telling us anything very important about the control of behavior in the natural environment.

If that is true, ethologists are equally guilty when, in studying natural behavior in the field, they make sure that there has been no chance for conditioning. Must we conclude that they cannot therefore be telling us anything important about behavior in the natural environment?

Schwartz explains the success of applied behavior analysis by pointing to other simplifications. The behavior of factory workers has been "captured" because the factory has eliminated other influences—sociocultural rather than biological. But social behavior in the world at large is certainly due to conditioning, and if we are to understand it, we must look to the basic processes.

Sociobiology. Ethology has spawned a child which threatens to play Oedipus and kill its father. It has also been said to threaten the experimental analysis of behavior. The term—with its roots "bio-" and "socio-"—alludes to the roles played by genes in biology and society, but skips over the individual. As I have pointed out (Skinner, 1981), selection is a causal mode, found only in living things, which operates at three levels. Darwin revealed its role in natural selection, but Herbert Spencer had already pointed out, if none too clearly, a role in the behavior of the individual and in the evolution of cultural practices.

A recent issue of *Science* (Levin, 1982) contains an interview with Ernst Mayr, a leading figure in evolutionary theory and the author of a new book called *The Growth of Biological Thought* (Mayr, 1982). In explaining why evolutionary theory is misunderstood by physicists, Mayr neglects an important point about selection. As to the differences between physical and living systems, he says, "There isn't a process in a living organism that isn't completely consistent with any physical theory. Living organisms, however, differ from inanimate matter by the degree of the complexity of their systems and by the possession of a genetic program." Complexity itself is not a difference in kind, nor was the "organization" with which biologists, at an earlier date, usually defined an organism. The "genetic program" points, though not directly, to the real difference: Organisms differ from physical things because they show selection by consequenes.

In *Sociobiology* (1975), E. O. Wilson points to certain features common to natural selection, operant conditioning, and the evolution of cultures, and attributes them all to genes. Genes no doubt explain behavior which is due to natural selection, and they are also responsible for operant conditioning as a process, but once that process has evolved, a different kind of selection accounts for the behavior of the individual and the evolution of cultural practices.

Autoshaping. I studied another process said to threaten an operant analysis in the late 'forties and tried to get a graduate student to take it up for her thesis in 1950. In my experiment, a spot of light moved across a screen and when it reached one edge, a food magazine operated. The pigeon began to peck the spot as if it were driving it across the screen. Epstein and I (1980) recently confirmed this result, although it is not clear that the pigeon is driving the spot; it may be merely following it. In the middle 'fifties, W. H. Morse and I were curious about the great variability in extinction following continuous reinforcement. After a given number of reinforcements, some pigeons would emit many hundreds of responses and others only a few. We thought the difference might be due to the fact that some pigeons often missed the key or pecked too lightly to operate it and were therefore actually on an intermittent schedule. We made a very sensitive key and evoked a clear-cut exploratory response with the method Brown and Jenkins (1968) later called autoshaping. We spoke of it as "conditioning a hot key." (Incidentally, we got our answer, though we never published it. If you make sure that all responses are reinforced, you can reinforce many thousands of times and still get fewer than a hundred responses in extinction.) Organisms presumably possess a repertoire of innate behavior with which unusual features of the environment are explored. Through a kind of Pavlovian conditioning, a key which lights up before food is delivered becomes the kind of feature eliciting or releasing such a response. An article in the current *Journal of the Experimental Analysis of Behavior* (Buzsaki, 1982) argues that some instances of Pavlov's "orienting reflex" may be examples. The fact that the response to the key may actually reduce frequency of reinforcement should occasion no surprise.

Comments by two reviewers of *Autoshaping and Conditioning Theory* by Locurto, Terrace, and Gibbon (1981) are relevant. In *Contemporary Psychology* (1981), Barry Schwartz writes:

> The key is lit, and then food is delivered. Procedurally, this is a mundane example of classical conditioning, with the key as a CS and food as a US. But what is the classically conditioned response? It is not salivation, or an eye blink; it is a peck at the key. The classically conditioned response is, or seems to be, what used to be viewed as a voluntary response, not a reflex. What is going on? Is the key peck voluntary or reflexive? Is autoshaping classical or instrumental? Is there something wrong with our distinction between the two conditioning processes?

There is nothing wrong except Schwartz's analysis. An operant cannot be identified by topography alone; the controlling variables must be specified. When several different variables are operative, as in verbal behavior, a structural or formalistic approach is especially troublesome, as linguists are learning to their sorrow. Pecking a key is an operant when it is primarily due to a particular history of reinforcement. It is a released innate response when the lighting of a key is followed by the presentation of food, as in the autoshaping procedure.

Schwartz draws another supposedly threatening conclusion:

> What autoshaping suggested was that pecking might indeed be special—peculiar to pigeons (and perhaps other birds) in feeding situations. In consequence, it raised the serious possibility that the massive accumulation of empirical generalizations about the determinants of pigeons' pecking might not be applicable to all the instrumental behavior of all organisms. Instead, these generalizations might only be true of pigeons—or of organisms in situations in which the required instrumental response was biologically related to the reinforcer.

But pigeons can press levers and rats can peck keys and will do so under appropriate contingencies of reinforcement. As I pointed out in a recent paper in the

Behavior Analyst (Skinner, 1980), there are several kinds of pigeon pecks, and they are not all concerned with ingestion. Ferster and I explicitly acknowledged the ethological sources of the pecking response we studied.

Schwartz continues:

> Because autoshaping involves a commonly studied behavior, in a commonly studied situation, the autoshaping phenomenon implies not only that an organism's biology might contribute to how and what it learns, but also that the said biology *has been contributing* all along, in studies that were presumed to have purged biology as a significant variable. Because of this, autoshaping is a dual threat to traditional learning theory. It is a threat because it suggests, as does taste aversion, that learning theory must take biology seriously. And it is a threat because it suggests that learning theory has been misunderstanding its own experiments.

But who are these people who believe that they have purged the behavior of an organism of biology as a significant variable? And what has been misunderstood?

In a review of the same book in *Science* (1981) Peter Killeen says that "in 1968 Brown and Jenkins demonstrated that Pavlovian contingencies (pairing a key light with food in a standard experimental chamber) yielded faster conditioning of the pigeon's key pecks than did traditional hand-shaping procedures." His next sentence begins, "As if this were not bad enough . . ." How bad it is depends on who does the shaping. Pavlovian conditioning is certainly slower than operant conditioning; I know of no instance in which one pairing has ever been shown to be effective (Pavlov's record-breaking dog showed a small effect after five pairings of tone and food), but, as I reported nearly fifty years ago, a single reinforcement of pressing a lever may be followed by a sizeable extinction curve. I dare say the same thing can be shown for pecking a key. Killeen also says that the work on autoshaping means that the discipline is moving close to the biological bases of behavior, "a position it was a mistake ever to have left." Again, I should like to know who has left it.

The Garcia Effect. Many years ago taste aversion was known as "stomach memory." The unusual thing about it is the time which elapses between behavior and consequence. In operant conditioning, a reinforcing consequence must be closely contingent upon behavior. If it were not, all intervening behavior would also be reinforced and chaos would follow. Yet in the Garcia Effect a tendency to eat a particular food is affected by consequences occurring many hours later. The result has obvious survival value in protecting organisms from the further ingestion of poisons or highly indigestible foodstuffs. Presumably the punishing consequence would affect the eating of any other unusual foodstuff at the same time or during the interval but not other kinds of behavior. There is little chance for confusion, because it is a special consequence of ingestion. If other kinds of deferred punitive consequences had a comparable effect, it would be felt by all intervening behavior. There is nothing in the Garcia Effect that contradicts any part of an operant analysis or throws into question any established facts. The consequence is punishing rather than positively reinforcing and seems to work exactly as I describe punishment in *Science and Human Behavior.* Through Pavlovian conditioning, stimuli arising from a situation in which behavior has been punished become aversive, and any behavior resulting in their reduction or removal is reinforced as escape or avoidance.

Probability of Reinforcement. In an operant chamber the organism is in contact with the contingencies only at the moment of reinforcement. Ferster and I designed much of our research to show that schedules have their appropriate effects by virtue of the stimuli present at just that time—stimuli generated in part by the organism's recent behavior. Several writers have recently implied that organisms may be sensitive to an increase in the mere probability of reinforcement when no reinforcer is immediately contingent upon a response. I do not think that the possibility of a conditioned reinforcer has been satisfactorily eliminated as an explanation, but I will rest my case on the following experiment, which takes advantage of the fact that the role of a reinforcer is clearer in shaping behavior than in maintaining it. Let small measures of food be delivered to a hungry pigeon at two different rates—once a minute and three times a minute, for example, not equally spaced. The experimenter holds a switch with which the rate can be changed from the low to the high rate and is asked to use it to shape a bit of behavior—say, a clockwise turn. Superstitious responses will emerge, and it is conceivable that one of them will be turning, but that will happen only if there is an accidentally contingent reinforcement. If the rates are very fast—say, ten times a minute and thirty times a minute, the repeated delivery of food may serve as a conditioned stimulus and accidental contingencies will be much more likely, but at the rates of delivery which are said to show an effect on the maintenance of behavior, I predict that no effect will be demonstrated.

"Learning Processes". Another source of misunderstanding is the strong inclination to look inside a system to see what makes it tick. Operant conditioners are criticized because they refuse to do so. They are said to be interested in controlling behavior but not in understanding the mechanisms responsible for it. I am sure there are mechanisms, but they belong to a different discipline—physiology. Whether there are two processes of conditioning or only one is not a question about behavior, because the external contingencies in respondent and operant conditioning are clearly different. Both may occur in the same setting but, even so, can be easily distinguished. The question is about a common process—an inferred mechanism.

It is usually discussed as associationism. Pavlov's dog is said to have associated the bell and the food. But, as I have pointed out (Skinner, 1977), it was Pavlov who associated them, that is, who put them together side by side. There is no evidence that the dog engages in any such process internally. Incidentally, I am not sure that Pavlovian conditioning is a good model of associationism. Though I have used the expression, I now think that "stimulus substitution" is misleading. Too often there is no unconditioned stimulus. In the Estes-Skinner experiment, for example, a tone which is repeatedly followed by shock soon suppresses any operant behavior in progress, but a shock alone does not suppress the behavior. Similarly, in autoshaping the response to the key need not be the type of response elicited by the reinforcer. Jenkins and Moore (1973) have shown a slight similarity of the autoshaping peck to the consummatory responses of eating and drinking, but they note that exceptions have been reported by others. Autoshaping is not a "mundane example of classical conditioning." The salivary response has idiosyncratic properties which are rare even in other automatic responses. Reflex responses or released behaviors have evolved which have no unconditioned stimuli or releasers. Stimuli must acquire the power to elicit or release them during the lifetime of the individual. They acquire it when they precede positive reinforcers (as in autoshaping) or negative reinforcers (as in the Estes-Skinner effect). Some examples of association, particularly involving emotional responses, may show a substitution of stimuli as in Pavlov's experiments, but many are clearly operant and have to do with the pairing of discriminative stimuli.

So long as we study observed behavior as a function of genetic and environmental variables, we are on safe ground. We shall no doubt continue to discover new facts, some of which may be puzzling but, if we are to judge from the past, they will eventually be assimilated to that corpus of knowledge which is at the heart of the experimental analysis of behavior. But how will historians of science treat the digressions which I have just examined? I should hope that they will see that the critics of an experimental analysis of behavior have not properly understood it. Recently I was heartened when a psychiatrist sent me a book he had just published, containing the following passage:

> E. L. Thorndike, as early as 1890, demonstrated in a very convincing way the ability of animals to learn if a reward is given them. In the "Skinner box," a test animal put into a closed box will vainly

search for an escape hole. A lever connected to an invisible opening, if touched accidentally, will permit escape. As the experiment is repeated several times, the animal—rat, mouse, hamster, monkey or otherwise—will take less and less time to find the solution of escape by touching the lever. Ultimately the animal becomes most proficient.

A passage like that is consoling because it makes one realize how far some of the critics of an operant analysis are from understanding it.

So-called objections to operant theory need not detain us. There is work to be done. My own contribution to that issue of *Psychology Today* read in part as follows:

I am inclined to rank progress in basic laboratory analysis first. With the aid of miniaturized controlling equipment and computers, behavior is now observed and measured with increasing precision in operant laboratories throughout the world. Repertoires of behavior are being studied which have a much greater breadth and complexity. It is still a hallmark of the operant-conditioning method . . . that the results may be formulated in centimeters, grams, and seconds rather than in the nonphysical dimensions of mental life.

These advances have greatly increased the extent to which the terms and principles drawn from an experimental analysis can be used in interpreting behavior in the world at large. Interpretation has not been well analyzed by scientific methodologists, and it has been widely misunderstood by critics of the operant field. Among the processes which have been submitted to more careful analysis and interpretation are many that have been attributed to . . . concept formation, creativity, and decision-making. A number of these are being clarified as an operant analysis, particularly of verbal behavior, is better understood. Some behavior is contingency-shaped; it has been selected by reinforcing consequences in the past. [Other behavior may consist of] imitating the behavior of, or following the advice of, another person whose behavior has already been selected by its consequences. This distinction between rule-directed and contingency-shaped behavior is only one example of a new approach to the analysis of so-called cognitive processes.

One advantage in relating behavior directly to environmental conditions is that one can then move directly to technological control. An experimental analysis points to the conditions which must be changed to bring about changes in behavior for practical purposes.

Interest in the experimental analysis of behavior and its use in interpretation and practical control has spread rapidly throughout the world during the last 15 years. Associations have been organized and annual conferences held in the United States, Latin America, Europe, Israel, Japan, and elsewhere. The Association for Behavior Analysis, an international organization, attracts new members each year and its programs show an increasing scope.

Philosophers, political scientists, economists, and others who once dismissed behaviorism as rat psychology are now seriously considering its implications. The journal *Behaviorism,* with its large international board of editors, now in its 10th year, has become an important forum.

I myself am most concerned with the possible relevance of a behavioral analysis to the problems of the world today. . . . If there are solutions to those problems, I believe that they will be found in the kind of understanding to which an experimental analysis of human behavior points.

The experimental analysis of behavior is alive and well. Psychology needs it.

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