The Journal of Mind and Behavior
Autumn 1993, Volume 14, Number 4
Pages 355–364
ISSN 0271–0137

What Counts as "Behavior"?

James J. Jenkins

University of South Florida

This paper considers the changes in the meaning of "behavior" in the hands of the cognitive psychologists as well as in the definition of the academic field itself. American psychology at mid-century constrained the "acceptable" subject matter in many ways, particularly through the editors of important journals. Gradually it became possible to write about "mental processes." This served as an important sign that what counted as "behavior" was changing, or that psychology was no longer defined as the science of behavior. Evidence that the field was changing was also signaled by definitions of the discipline appearing in introductory texts. In this case psychology as "the study of behavior" gave way to psychology as "the study of behavior and _____." (Fill in your favorite term, ranging from "experience" to "mental life.") Additional current evidence is that investigators whom we honor and recognize as leaders in the field are studying and writing about topics that would have been rejected out of hand by most psychologists at mid-century.

Although some of our colleagues maintain that there has been no "cognitive revolution" (see, for example, Leahey, 1992), those of us who have worked in the field over the last 45 years have seen a dramatic change in "what counts" as psychological subject matter and research. I can appreciate that this change may be hidden because often the identical words are used to describe the subject matter of our field. In spite of this superficial agreement with the past, the working definition of the words has changed in very significant ways, as Leahey (1993) has shown for the early period, and some new ideas have been added to the basic core of the discipline.

As an aside, it can be argued that there has been no revolution in psychology because there has never been an accepted paradigm: no paradigm, ergo, no possibility of revolution. While this may be a relevant consideration for

Paper originally presented at the Centennial Meeting of the American Psychological Association, Washington, DC, August 17th, 1992. Address correspondence to James J. Jenkins, Ph.D., Distinguished Research Professor, Department of Psychology, BEH 339, University of South Florida, Tampa, Florida 33620-8200.

the long range historical or philosophical overview, from the point of view of a participant, this is irrelevant. If one is trained as a member of a strong "school" that dominates the accepted journals, that is regarded as the standard of the societies whose meetings one attends, and that determines one's acceptance by colleagues whom one regards as significant psychologists, then from the point of view of that person, there is a paradigm in all of the other senses that Kuhn (1970) described.

Recent History

Perhaps it is difficult to believe that there is a difference in the subject matter of our field between the 1920s and the present day, but one only needs to consider the strictures laid on the field by the leaders of the behaviorist movement. Here, for example, are two items from Watson's *Behaviorism:*

> In his first efforts to get uniformity in subject matter and in methods the behaviorist began his own formulation of the problem of psychology by sweeping aside all mediaeval conceptions. He dropped from his scientific vocabulary all subjective terms such as sensation, perception, image, desire, purpose, and even thinking and emotion as they were subjectively defined. (Watson, 1924/1967, pp. 5–6)

That statement rules out much of modern psychology. And again:

> . . . behavioristic psychology has as its goal *to be able, given the stimulus, to predict the response — or, seeing the reaction take place to state what the stimulus is that has called out the reaction.* (Watson, 1924/1967, p. 18, italics in the original)

It is hard to appreciate fully what a severe constraint this is! It demands that stimuli and responses be in a one-to-one mapping; it admits no possibility of different sets of stimuli having the same effect on responses or of different responses occurring to the same stimulus complex depending on the state or disposition of the organism.

While Watson might be excused for his radical position in his role as polemicist for the new cause, he was not alone in the polarity of his beliefs. Statements of this extreme kind were common in the writings of the time and such dicta were given with great emotional force. Carroll C. Pratt, the distinguished psychologist and editor, wrote:

> The bitterest dispute in the twenties and early thirties was between the classical psychologists (the followers of Wundt and Titchener) and the behaviorists. The younger psychologists of today can scarcely imagine the violence of that dispute or the vitriolic manner in which it was carried on. Members of one side often looked upon their opponents as no better than high-grade morons in their addle-brained foozles about the subject matter of psychology. (1969, p. 11)

Nor did the biases totally disappear with the 1940s. Consider, for example, the work of Weston Bousfield in the 1940s and early 1950s on structure in recall, work that was important in the whole "verbal learning and verbal behavior" movement. For years Bousfield published his work in "non-standard" journals because recall was not considered a sufficiently scientific topic, and the notion of a *structure of memory*, other than a serial list, was simply unacceptable.

Similarly, in the mid 1950s when Wallace Russell, Walter Mink, and I experimentally demonstrated the relation between the free association strength of word pairs and the clustering of those words in recall, the editor of the *Journal of Experimental Psychology* did not even send our manuscript out for review. He simply wrote across the face of our letter of submission, "This would not be of any interest to my readers." (The work was finally published in a non-standard, pay-per-page journal [Jenkins, Mink, and Russell, 1958], and currently, is the most-cited study of the scores of experiments we did in the 1950s.) In short, even in the 1950s, although "learning" was an acceptable topic of study, "memory" and "recall," with their overtones of mentalism, were not.

This phenomenon was not unique to our laboratory. For example, consider Wendell Garner's recent account of his early interest in the perception of auditory structure.

> I had a lot of fun sitting in my lab and listening to these different sequential patterns, and it was obvious that there were some interesting perceptual differences between them. But I simply could not figure out how to do an experiment with them. I could hear interesting things, but I could not perform things with them. I was too brainwashed by behavioristic and psychophysical methodologies, and these were not appropriate to what I was hearing. (Garner, 1991, pp. 327–328)

A few years later when he was working with Fred Royer:

> As before, we first just listened to the patterns. There were obvious differences in the patterns, their simplicity and Gestalt goodness, but once again we could not figure out how to do any experiment with them. (Garner, 1991, p. 328)

They settled for having subjects discern the patterns and then tap them out, a procedure that Garner calls, "a finger phenomenological report." Not until the mid 1960s did Gottwald and Garner finally let the subjects describe the pattern themselves.

> . . . Gottwald came to my office and suggested that we simply ask subjects to describe the patterns after they had learned them. After feeling foolish for not having thought of that myself, I agreed. That verbal description gave us more information about how patterns were perceived than any other measure. (Garner, 1991, p. 328)

Another sign of the times is that the first work that Garner and Clement did on visual patterns was published in 1963 in an unlikely place, *The Journal of Verbal Learning and Verbal Behavior.*

> . . . because I did not think it would get through David Grant, then editor of the *Journal of Experimental Psychology;* it used ratings and classification and, horror of horrors, was a correlational study. But Leo Postman was editor of the new *Journal of Verbal Learning and Verbal Behavior* and he was tolerant enough to publish it. However, methodological problems were still around and did influence how I and others did our research and published it. (Garner, 1991, p. 330)

By the 1960s some of the grip of radical behaviorism was beginning to relax. Newell and Simon (neither of whom was a psychologist, of course), were optimistic enough in 1961 to write:

> Behaviorism accepted, and indeed reinforced the mechanistic assumption that the connections between stimulus and response were formed and maintained as simple deterministic functions of the environment . . . . Today psychology lives in a state of relatively stable tension between the poles of Behaviorism and Gestalt psychology. All of us have internalized the major lessons of both. We treat skeptically the subjective elements in our experiments and agree that all notions must eventually be made operational by behavioral measures. We also recognize that a human being is a tremendously complex, organized system, and that simple schemes of modern behavioristic psychology seem hardly to reflect this at all. (Newell and Simon, 1961/1963, p. 280)

A signal event of 1960 was the publication of *Plans and the Structure of Behavior* by Miller, Galanter, and Pribram. The aim of the book was to solve the problem of how to get from cognition to action; they cited, of course, Guthrie's remark about Tolman having left the rat lost in thought at the choice point. They pointed out that behaviorism did not relate knowledge to behavior and, in addition, did not recognize, much less deal with, the problems of levels of description of behavior that linguists and ethologists were writing about. As they reflected at the end of the book, they created a new classification of psychologists:

> Our emphasis was upon processes lying immediately behind action, but not with action itself . . . . Deep in the middle of this dilemma it suddenly occurred to us that we were *subjective behaviorists.* (Miller et al., 1960, p. 211)

Miller and Bruner together founded the Harvard Center for Cognitive Studies. Later Miller wrote:

> In 1960 we used "cognitive" in our name defiantly. Most respectable psychologists at the time still thought cognition was too mentalistic for objective scientists, but we nailed it to the door and defended it until eventually we carried the day. And now there are Cognitive Centers everywhere. (Miller, 1979, p. 11)

Miller goes on to write that the victory may have been more limited than all of these labels would lead one to suppose, however, he continues:

> But even our limited victory was important, for it served to lift psychology's 30-year ban on mentalistic terminology. A new generation of psychologists has now grown up without feeling naughty when they talk about mentalistic concepts like cognition, attention, imagery, memory, intuition, expectation, planning, intention, will, and so on, all of which had been banned by behaviorists as unscientific. (Miller, 1979, p. 12)

In 1967, Neisser gave the field official blessing with the publication of *Cognitive Psychology*. In discussing the organization of the book in the preface, he writes:

> It follows stimulus information "inward" from the organs of the senses, through many transformations and reconstructions, through to eventual use in memory and thought. (Neisser, 1967, p. vii)

Notice that it never gets out to action! It is pure cognitive psychology without external behavior (except as behavioral evidence is used to infer internal processes). The references show almost complete neglect of the leaders of behaviorism. There is one reference to Watson, one to Hull, two to Tolman and three to Skinner, and none of those citations refer to specific behavioral research. On the other hand, there are 33 references to G.A. Miller, 21 references to Bruner, 20 to Sperling, 19 to Broadbent, 17 to Selfridge, and 16 each to Bartlett and Postman. Neisser's concern was with "the world of experience" (p. 3), the "activity of the cognitive systems" (p. 3), and the "acts of construction" (p. 10) that are involved in seeing, hearing and remembering, and "the continuously creative process by which the world of experience is constructed" (p. 11).

As late as the early 1970s, however, one was still told to be cautious in the way one wrote about mental matters. Hyde and I (Hyde and Jenkins, 1969, 1973) said that subjects "processed" words differently under different sets of instructions; (those who judged pleasantness of words remembered them; those who counted letters or otherwise paid attention to the form of the words remembered them much less well). In his *Annual Review* chapter, Postman scolded us for choosing the notion of mental processing over an account stressing "differential responding to the stimulus items."

> Against the background of the functionalist tradition, the developments which we have described had the earmarks of a theoretical and methodological revolution. Now it is a commonplace observation that revolutionaries tend to turn conservative once they seize the reins of government. It is to be feared that intellectual revolutionaries are no exception. There are worrisome signs that a new orthodoxy and a *normative nominalism* may be in the making. We must guard against the obvious fallacy that a change to a preferred language entails, or even holds out the promise of, better understanding. (Postman, 1975, p. 294, italics added)

Estes' autobiography (1989, pp. 103–104) similarly recounts his own "breaking away" from the constraints of "stimulus–response learning theory"; beginning with his article on mental chemistry in 1960, to his 1972 paper on "An Associative Basis for Coding and Organization in Memory" which includes, he writes, "the appearance of concepts that would have been unthinkable, or at least inexpressible in the earlier tradition" (p. 104). Estes contrasts "operationism" and "constructivism."

> By constructivism I refer to a systematic effort to build representations of observable phenomena in terms of inferred, and often abstract, underlying entities and processes, an approach exemplified by the work of Bohr and Einstein in physics, Clark L. Hull among the early learning theorists, and the cognitive scientists on the current psychological scene. (Estes, 1989, p. 114)

A Useful Distinction

One problem in discussing this topic is that *behavior* and *behaviorism* are frequently confused. By behaviorism we should mean (as Skinner points out) the philosophy of science of a group of students of behavior. That philosophy of science as realized in mainstream American psychology, involved much more than a definition of the subject matter of the field. It embraced a number of strictures about what could and could not be considered as proper topics for investigation (what "counted" as behavior), about the possibility of inferred entities, about the complexity of theories, about the kinds of theoretical moves that could be considered acceptable, and so on.

I think a three-fold distinction is useful in clarifying the discussion. (I owe this tripartite distinction to Paul E. Meehl from many years ago.)

1. *Dogmatic, ontological behaviorism.* This position which characterized Watson (and sometimes Skinner) holds that there are no mental events; that such intuitions are illusory; and that such ways of talking are simply careless use of common, unscientific language. (Some psychologists work themselves into this category even though they might not initially appear to endorse such a statement. For example, Max Meyer [1922] and Floyd Allport [1924] both argue that congenitally deaf people cannot think *because* thinking is *just* talking to oneself, ergo, no ability to talk, no thinking. Deaf people have no concepts because concepts are *just* verbal labels; hence, no labels, no concepts. Writing is *just rearranging* words on paper; and so on.)

2. *Epistemological behaviorism.* This position is neutral with respect to the status of mental events but holds that *as scientists* we have no special access to such events. Our scientific *data* can only consist of observables in the physical world. Whether one uses inferred constructs of the mentalistic sort is a matter of taste and scientific utility. (In my classes, I have also called this "methodological behaviorism.")

3. *Domain-defined behaviorism.* This simply defines the subject matter with which one is concerned as the description, prediction, and control of *behavior.* Absent other methodological or metaphysical commitments, anything that helps in this endeavor is permissible.

What is Psychology Today?

Is psychology the study of behavior? One could accept the second position above, epistemological behaviorism, but reject the notion that psychology is concerned exclusively with the study of behavior. I believe that this is exactly what is happening. Consider for a moment the changes that Henley, Johnson, Jones, and Herzog (1989) have documented in their article on definitions of psychology given in introductory psychology texts. Briefly put, many psychologists want to include *more than behavior.* George Miller (1962) says explicitly that he wants to include "mental life." Many writers now say that the field must include within its subject matter, "mental processes," "experience," or even "human nature."

Table 1

Definitions of Psychology in Introductory Psychology Texts
(adapted from Henley, Johnson, Jones, and Herzog, 1989)

1980–1987	n = 76	
	Behavior	41 %
	Behavior and	38 %
1970–1979	n = 84	
	Behavior	50 %
	Behavior and	14 %
1950–1969	n = 40	
	Behavior	60 %
	Behavior and	8 %
1930–1949	n = 18	
	Behavior	38 %
	Behavior and	27 %
1887–1929	n = 15	
	Behavior	7 %
	Behavior and	7 %

The data of Henley et al. (1989) show that there is a trend toward broadening the definitions with an appreciable lag behind the field, of course. (I

have greatly simplified the data presented by Henley and his colleagues, but I do not think I have done violence to the trends that they discovered.)

To go to the other end of the spectrum, we can ask what is happening on the "cutting edge" of the field of cognitive science. Hunt's chapter on cognitive science in the *Annual Review of Psychology* defines the field as the "computational view of thought" (Hunt, 1989). Within that view, Hunt sees three separate levels of psychological theory: *information processing* theories that attempt to define "human mentalise" and the machine associated with it; *physical theories* that try to explain how this machinery is instantiated in the brain; and *representational theories* that express regularities in the way that relationships in the external world are captured by mental models. This is a far cry from activities of the muscles and glands.

If one examines Posner's (1989) admirable compilation, *Foundations of Cognitive Science*, one does not find a discussion of the field of psychology, or, indeed, definitions of psychology at all. There are a few references to the historical difficulties associated with behaviorism but, mainly, there is too much concern with the work at hand to spend much time in internecine warfare. For example, Simon and Kaplan's (1989) chapter on "Foundations" briefly mentions the limitations imposed by behaviorism during the first half of the century, but then rushes on. In his introduction, Posner defines cognitive science as:

> the study of intelligence and its computational processes in humans (and animals), in computers and in the abstract. (1989, p. 2)

He regards it as arising from:

> experimental and cognitive psychology, artificial intelligence (within computer science), linguistics, philosophy (especially logic and epistemology), neuroscience, and some others (anthropology, economics, and social psychology . . .). (p. 3)

Of the 34 authors of chapters in the Posner volume, 14 are in departments of psychology, and 6 more are from the department of Brain and Cognitive Science at MIT (which holds what used to be experimental psychology at MIT); thus, 20 of the 34 authors are in what seem to be departments of psychology. Four authors hold positions in "Centers for . . . something or other," four are in computer science departments, and three are in philosophy departments. Others are in anthropology, biophysics, or unidentifiable.

Some of the topics would annoy some of our recent ancestors but they might seem appropriate to a William James. The techniques, however, and special questions arising from modern technical tools would surely be surprising and perhaps incomprehensible to someone of James' period. Table 2 lists some chapter headings that a cognitivist does not find surprising but a behaviorist might find shocking.

Table 2
Some Chapter Titles from Posner's
Foundations of Cognitive Science (1989)

Symbolic architectures for cognition
The architecture of mind: A connectionist approach
Model–theoretic semantics
Brain and cognition
Discourse
Mental models
Concepts and induction
Problem solving and cognitive skill acquisition
The computational study of vision
Visual attention
Memory
Cultural cognition

How many of these are psychological? Perhaps all of them. How many are behavioral in the old sense of glandular action or body movement? Almost none.

In my view psychology is *the science of behavior* only in the most general epistemological sense given above. The old strictures of dogmatic behaviorism are all but gone and the limitations of topics in the field have been greatly loosened. Perhaps we can take the best of both views and couple the rigor of the Behaviorists with the imaginative and exciting topics and concepts of the Cognitivists. We need to get on with the business at hand however we choose to label it.

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