

ON THE CONCEPTUAL AND LINGUISTIC ACTIVITY OF PSYCHOLOGISTS: THE STUDY OF BEHAVIOR FROM THE 1890S TO THE 1990S AND BEYOND

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ABSTRACT: In the early twentieth century psychology became the study of “behavior.” This article reviews developments within animal psychology, functional psychology, and American society and culture that help explain how a term rarely used in the first years of the century became not only an accepted scientific concept but even, for many, an all-encompassing label for the entire subject matter of the discipline. The subsequent conceptual and linguistic activity of John B. Watson, Edward C. Tolman, Clark L. Hull, and B.F. Skinner, as they attempted to explain “behavior” throughout the course of the twentieth century, is then discussed. Finally, the article suggests the need for greater conceptual and linguistic diversity in psychology. In this last regard, reference is made to cognition and consciousness, to William James and John Dewey, and to the fact that prediction and control might not be the most relevant aims of contemporary psychology.

Key words: behavior (concept of), behavior (science of), behaviorists, behaviorism

When B.F. Skinner died in 1990 an important era in American psychology came to an end. The many tributes and retrospectives occasioned by his death were certainly appropriate. In many ways the things that Skinner stood for—most notably an objective understanding of behavior and a desire to have that understanding used for the improvement of human life—had become inextricable parts of American psychology. One did not have to be a behaviorist, much less a Skinnerian, to recognize their significance or to appreciate Skinner’s contributions.

At the same time, it was clear that American psychology had not become what Skinner wanted it to be. It was neither the exclusive domain of behaviorists nor dominated by operant analysis. The cognitive revolution—the so-called “second American revolution” (Hebb, 1960)—had not only been successful, its impact had been strengthened through multidisciplinary developments within the broader field of cognitive science. Similarly, neuropsychology had received a strong boost from advances within a new complex of fields known as neuroscience, and the systematic study of cross-species and species-specific behaviors was reviving interest in evolutionary psychology, which included

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investigations of mental capacities ignored by prior generations of behaviorists. In sum, at the end of his life, Skinner had reason to wonder, “Whatever Happened to Psychology as the Science of Behavior?” (1987).

Of the many assessments of “the science of behavior” that took place around the time of Skinner’s death, one of the most astute was offered by Gerald Zuriff (1985), who concluded that

the received wisdom of today is that behaviorism has been refuted, its methods have failed, and it has little to offer modern psychology. Attacks against behaviorism have reached the frequency and vehemence that marked behaviorism’s assaults against its own predecessors. . . . Factors other than effectiveness hold sway, and the search for truth is lost in the battles between movements. Clearly, this is not useful to psychology, or to society. What is needed rather is an accurate portrait of behaviorism and an honest search for what is still valuable in it. (p. 278)

In the following years various portraits of behaviorism have been offered and the search for its valuable aspects has been pursued by many individuals. Developments along the lines of “teleological,” “theoretical,” and “biological” behaviorism—not to mention research projects that have pushed the conceptual and methodological limits of more traditional operant psychology—have shown that behaviorism is far from dead (see O’Donohue & Kitchener, 1999).

Unfortunately, Skinner himself would not have celebrated most of these latter-day developments. Not only did his unrelenting orthodoxy lead him to resist inconvenient facts (as Staddon, 1993, p. 83, has pointed out), it also presented a roadblock to the kinds of change that seem to me and to many others to be heading in the right direction. No one should feel compelled to hold onto old premises that are no longer accepted by the kinds of philosophers and scientists who generated them in the first place, nor should anyone feel embarrassed about trying out diverse perspectives and principles—even incompatible perspectives and principles—as part of a broader and more inclusive program of research on animal and human activity, whether that activity be construed as “cognitive,” “behavioral,” or “organismic.” Psychology is not alone among disciplines, much less interdisciplines, in having to advance tentatively, with hunches, rules of thumb, and theoretical conjectures, nor is it alone in possessing explanatory formulae that are not unified at this time. Although the concepts of “mind,” “consciousness,” and other hypothetical entities, states, and processes are indeed historical constructions, so too is the concept of “behavior,” and so too, for that matter, are “gravity,” “energy,” “valence,” “circulation,” and all other scientific terms. The referents of these terms need be no less real simply because of that fact (see Leary, 1990).

From all that might be said about these things, perhaps it will be most useful to consider the historical construction of “behavior,” to reflect on some of the ways in which major psychologists—Watson, Tolman, Hull, and Skinner—conceptualized and spoke about behavior, and finally to suggest some lessons and perspectives that might be drawn from these historical accounts, especially in relation to behavior as a subject of study.

The History of “Behavior”

As Skinner (1989) himself noted, “the word *behave* is a latecomer” in the history of the English language (p. 13). Even later in emergence was the word “behavior,” and later still was the use of this term to represent a general class of actions supposed to be amenable to systematic study and explanation. In fact, even in 1901-1902, when the first two volumes of James Mark Baldwin’s monumental *Dictionary of Philosophy and Psychology* (1901-1905) were published, “behavior” did not appear among the nearly 900 pages of entries written by experts who scrupulously treated every conceivable term used in philosophy and psychology, not to mention relevant terms from anthropology, biology, neurology, physiology, and education. Although “conduct” appeared in Baldwin’s *Dictionary*, it was defined as “the sum of an individual’s ethical actions,” and its use was prescribed for treatments of “moral action” (Vol. 1, p. 211). In short, it was a term for philosophical ethics rather than psychology. Even three years later, in 1905, when the 1200-page third volume of the *Dictionary* appeared (a volume devoted to a “Bibliography of Philosophy, Psychology, and Cognate Subjects”), there was no category for references dealing with “behavior.” Clearly “behavior” was not yet conceived to be a subject of study and analysis.¹ Interestingly, “learning” was not included either, even though the *Dictionary* drew upon relevant terminology from the field of education.

This raises an obvious question: What happened between 1905 and 1913, when John B. Watson issued his famous manifesto proclaiming not only that psychology should treat “behavior” but that “behavior” should encompass psychology’s entire subject matter? Perhaps it is better to ask: What had already been happening even before 1905, though it was not yet apparent in the terminology of psychology and its cognate disciplines, so that “behavior” could so quickly become a topic—and arguably the exclusive topic—of psychology? Many aspects of a complete answer have been given by others (e.g., Boakes, 1984; Buckley, 1989; Burnham, 1968; Danziger, 1997; Hibbard & Henley, 1994; Kitchener, 1977; Leahey, 1993; Logue, 1985; Mackenzie, 1977; Mills, 1998; O’Donnell, 1985; Richards, 1987a; Samelson, 1981). I will give my own summary version organized around three interrelated lines of development: (1) from within comparative animal psychology, (2) from within functionalist human psychology, and (3) from within American society and culture.

Animal Psychology

The best-known aspect of the emergence of “behavior” as a subject of disciplinary study revolves around the history of comparative psychology, which

¹ This is not to say that the word “behavior” can never be found in psychological texts of this period. William James, for instance, used the term occasionally, even in the titles of two chapters (ch. 3, “The Child as a Behaving Organism” and ch. 4, “Education and Behavior”) in his *Talks to Teachers on Psychology* (1899/1983c), but he used the word interchangeably with “action” and “conduct,” with no special or singular meaning.

rose in the aftermath of Charles Darwin's *Origin of Species* (1859), *Descent of Man* (1871), and *Expression of the Emotions in Man and Animals* (1872). Among their other important contributions, these classic works broke down the old and rigid distinction between humans and other animals. In doing so they invited comparative research on a variety of species. Thus, the work of George Romanes (1882) and C. Lloyd Morgan (1890) fell in a direct line from Darwin. This line led, in turn, to the work of psychologists like Edward L. Thorndike, whose research on cats, dogs, and chicks formed the basis of his 1898 dissertation on "animal intelligence." As the title of his dissertation suggests, this tradition focused on the mental capabilities of animals. Still, the inferences it drew about the relative intellectual powers of different species were based upon observations of the actions of animals. It was in this context that Morgan contributed to the development of "behavior" as a general term for the subject matter of comparative psychology when he published a book entitled *Animal Behavior* (1900).

When John B. Watson started his graduate study at the University of Chicago in the early 1900s he committed himself to comparative psychology, strengthening his research with advanced study of the nervous system (under the supervision of the neurologist H. H. Donaldson) and giving it a distinctive focus on biological mechanisms (under the inspiration of the physiologist Jacques Loeb). Though his dissertation on "the psychical development of the white rat" (1903) incorporated traditional psychological terminology, it laid the foundation for his future research. In the following years he and others became more and more hesitant about projecting "psychical" attributes onto their research animals. In 1906 H.S. Jennings used "behavior" in the title of his book on *The Behavior of the Lower Organisms*, and in 1908 Watson followed suit by using the term in the title of an article on "The Behavior of Noddy and Sooty Terns." By that time it was no longer uncommon for psychologists to refer to "behavior" in their writings; indeed, "behavior" was even used in a 1908 definition of the subject matter of psychology, not just comparative psychology, and not by a comparative psychologist or someone who thought that consciousness and other mental activities should be excluded from the discipline (as we shall see later). In 1911 "behavior" reached yet another level of acceptance when it was incorporated into the name of a series of "Behavior Monographs" and into the title of the new *Journal of Animal Behavior*.

By December of 1912 "behavior" was being used so frequently as the designation of a topic of study that Watson's former graduate school mentor, James R. Angell, felt compelled to reflect upon "Behavior as a Category of Psychology" at the annual meeting of the American Psychological Association (Angell, 1913). The emergence of "behavior" as a psychological category, he said, was due largely to the fact that comparative psychologists preferred to focus on the objective record of "animal behavior" without complicating matters by inferring conscious states and processes. In addition, he noted, growing criticism of "consciousness" as a philosophical as well as psychological concept and increasing concern about the sufficiency of introspection as a method for investigating the data of consciousness had contributed to the shifting of focus toward "behavior." Granting the appropriateness of some of this criticism and concern, Angell

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nevertheless expressed doubt that psychology (or even the subfield of comparative psychology) should or could get along without reference to consciousness or without reliance on introspection, however attenuated. Although he wished “the experimental ‘behaviorist’” well, he expressed hope that “the movement toward objective methods and objective description in psychology” would “forego the excesses of youth” (Angell, 1913, pp. 264, 270).

Just a few months later Watson (1913) demonstrated all the excesses of youth when he issued his famous manifesto. In the following years many who agreed with his call for objectivism did not become “behaviorists,” and many who considered themselves to be “behaviorists” continued to refer to consciousness and to use some form of introspection (see Woodworth, 1924; Samelson, 1981). Nonetheless, “behavior” was soon firmly established not just as an acceptable conceptual category for psychology but as a taken-for-granted term for all or part of its subject matter.

Functional Psychology

Angell might have regretted Watson’s excesses, but he nonetheless bore some responsibility for having pointed Watson and others toward the study of “behavior.” Although his own psychological theorizing highlighted the role of mental processes, it was the ultimate outcome of these processes in organismic functioning (i.e., behavior) that was of most concern to Angell. In this he drew upon the earlier work of William James and John Dewey.

James, of course, did as much as anyone to establish modern psychology in the United States and elsewhere, particularly through the publication of his magisterial *Principles of Psychology* (1890/1981). Like others, James was affected by the evolutionary theory of Charles Darwin and was in touch with other developments in biology. In fact, he was trained in physiology, which had led him to the following conclusion:

The only conception at the same time renovating and fundamental with which Biology has enriched Psychology, the only *essential* point in which “the new Psychology” is an advance upon the old, is, it seems to me, the very general, and by this time very familiar notion, that all our activity belongs at bottom to the type of reflex action, and that all our consciousness accompanies a chain of events of which the first was an incoming current in some sensory nerve, and of which the last will be a discharge into some muscle, blood-vessel, or gland. This chain of events may be simple and rapid, as when we wink at a blow; or it may be intricate and prolonged, as when we hear a momentous bit of news and deliberate before deciding what to do. But its normal end is always some activity. (James, 1888/1983b, p. 217)

What James was saying was that the mind, consciousness, internal processing, or whatever you wish to call it is thoroughly embedded in nature and has the chief function of selecting what to act upon, then acting. All cognition, starting with the most rudimentary sensation, is purposive, aimed at action within and upon the surrounding world. As James contended in this and other contexts—in his

pragmatic philosophy and his functionalist psychology—the consequences of an action determine its appropriateness. Consciousness is not a thing in and for itself; it is a function—a tool—in the struggle for existence. Like Darwin and the comparative psychologists who followed him, James was persuaded that consciousness had evolved because it played a prominent role in human and animal affairs: in short, because it made a difference. As a result James built a psychology that situated consciousness smack in the middle of the ongoing stream of natural life-processes. His was not the static mind of an idealist philosopher but a dynamic node within everyday experience.

John Dewey, meanwhile, started out as a Hegelian idealist but was converted to naturalism and ended up doing as much as anyone else to elucidate and extend certain aspects of James's thought. In particular, his classic article on "The Reflex Arc Concept in Psychology" (Dewey, 1896/1972) argued that psychological acts involve complicated, continually spiraling "circuits" of stimulus-reflection-and-response, with each response entering into the pattern of stimuli upon which the organism must then reflect and act, rather than simple, one-time, one-way sensory-motor "arcs." Although this article was later voted the most important contribution in the first 50 years of the *Psychological Review* (Langfeld, 1943), its basic insight was never completely absorbed into psychology. The fault, it must be said, can be attributed partly to Dewey, who never exhibited James's facility for clear and compelling language. As a result, Dewey's influence was less than it might have been, particularly in relation to John B. Watson, for even though Watson was initially attracted to Chicago by the idea that he might study with Dewey, he soon discovered that he was unable to understand what Dewey was talking about (Watson, 1936, p. 274). He turned for guidance, instead, to Dewey's disciple, James R. Angell.

It was Angell (1907), in his 1906 Presidential Address before the American Psychological Association, who formally defined "functional psychology" in contrast to the "structural psychology" being advanced at that time by E.B. Titchener and his students. Drawing on the legacy of James and Dewey, Angell argued that consciousness, far from being set apart from the physical world, was essentially embedded within it. As he wrote,

We shall regard all the operations of consciousness—all our sensations, all our emotions, and all our acts of will—as so many expressions of organic adaptations to our environment, an environment which we must remember is social as well as physical. (Angell, 1904, p. 7)

This point, that our environment is social as well as physical, became an important tenet for the "Chicago functionalists," who were influenced by George Herbert Mead and John Dewey. Although it is a significant point with continuing relevance, I cannot pursue it here, but I will return to it briefly later in this article. All I wish to indicate at this point is that the shift toward a focus on "behavior" in American psychology was consistent with a functional as well as comparativist viewpoint. Indeed, Angell (1907) concluded that "behavior" is "the most inclusive" of all the functional categories dealing with the biological realm (p. 76).

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With functional psychology and animal psychology focusing more and more on observable activity, it is not surprising that others, too, began to focus on the physical activity of humans and other animals. Also influenced by James was the British (and future American) psychologist William McDougall. As McDougall developed his own brand of functional or purposive psychology, he defined psychology in 1905 as “the positive science of the conduct of living creatures” (McDougall, 1905, p. 1). In 1908 he added “behavior” to his definition, saying that

Psychologists must cease to be content with the sterile and narrow conception of their science as the science of consciousness, and must boldly assert its claim to be the positive science. . . of conduct or behavior. (McDougall, 1908, p. 15)

Within yet another few years—still prior to Watson’s manifesto—“behavior” was not only appearing in other definitions of psychology but even in the titles of books on human psychology. 1911 was a banner year in these regards (e.g., Meyer, 1911; Parmelee, 1911; Pillsbury, 1911). One year later McDougall (1912) added another book title to the list. Amazingly, in just one decade, a term and concept that had failed to warrant treatment in Baldwin’s *Dictionary* had become prominent in the discipline.

American Society and Culture

How could this have happened? Could the developments we have reviewed, by themselves, account for this extraordinarily fast emergence of “behavior” as a major topic in psychology? A strong hint that an additional factor was involved can be gleaned from Watson’s preface to the second edition of his *Psychology from the Standpoint of a Behaviorist* (1924). The preface to this work makes it clear that Watson was ultimately concerned, not with animals in general, but with the humans and, more particularly, their adjustment to modern life. As he wrote,

Civilized nations are rapidly becoming city dwellers. With this increase in the concentration of homes there come changes in our habits and customs. Life becomes complex. The strain of adjusting ourselves to others increases daily. . . . If we are ever to learn to live together in the close relationships demanded by modern social and industrial life we shall have to . . . enter upon a study of modern psychology. . . . Fortunately, psychology is prepared to help us. (p. xi)

Watson wrote these words in the aftermath of the First World War, when psychologists had received positive notice for their contributions as psychologists (Samelson, 1979). He also wrote them within the larger historical context of the discipline’s sustained efforts, reaching back to the 1880s, to legitimate itself as a new profession that could serve the common good (Leary, 1987). These efforts were contemporaneous and consonant with—indeed, they were an integral part of—the Progressive Movement in the United States. Social control, to be used for the common good, was a central concern of this movement. A belief in

environmentalism was also essential for many of those who hoped to change the lot of the average person in America, and to do so as quickly as possible.²

Though no one had to be a behaviorist to do so, many concerned individuals directed their attention to the ill-advised or unproductive behavior of the poor, the powerless, the sick, and the otherwise disconnected and disenfranchised. It was in this context that discussions about improving human behavior, especially through services offered by the newly emerging professions of psychology and psychiatry, became staples of the era (see Burnham, 1960, 1972, 1987; for a review of the utopian visions of four major psychologists of the time, including Watson, see Morawski, 1982). John Dewey gave classic expression to the social concerns and cultural aspirations of the time in his 1899 Presidential Address before the American Psychological Association (Dewey, 1900/1976). Whether in a “quest for community” (Wilson, 1968) or a “search for order” (Wiebe, 1967), many psychologists came to see themselves, their discipline, and their profession as having a “rendezvous with destiny” (Goldman, 1952). It is in this larger social and cultural context that a focus on “behavior” came to the fore.

Understanding the environment within which early twentieth-century psychology developed helps to explain the rapid sea change from age-old talk about “souls” and “minds” to new kinds of talk about “behavior” and its “prediction and control.” It should also alert us to the possibility that in the rush for tangible results—to show how the new behaviorist psychology could improve the human condition here and now—psychologists made some rash conceptual and linguistic moves.

Thinking and Speaking About Behavior

Indeed, very soon after John B. Watson (1878-1958) called for “making *behavior*, not *consciousness*, the objective point of our attack” in psychology (Watson, 1913, p. 177), it became apparent to him that “it is one thing to condemn a long-established method, but quite another thing to suggest anything in its place” (Watson, 1916a, p. 89). Given the confident, even strident tone of the assertions in his 1913 manifesto, he needed a way to think and speak about behavior that would (hopefully) make behavior amenable to prediction and control. Pressured by his election to the Presidency of the American Psychological Association, Watson cast his lot, with fingers crossed, behind a relatively new and still poorly understood physiological concept that he and Karl Lashley had only recently begun to study. So it was that in his 1915 Presidential Address, with acknowledgement of the research done by Ivan Pavlov and Vladimir Bekhterev, Watson argued for “The Place of the Conditioned-Reflex in Psychology” (Watson, 1916a). In this address Watson made a number of conjectures that went well beyond any data, much less any proof that was available at that time, from his laboratory or from anyone

² The alternative to environmentalism and its related forms of “scientific management,” for those who wanted to ameliorative human affairs but believed in the power of heredity, was eugenics. Neither eugenics nor its relation to psychology is part of the story I am telling here, but it is relevant to note that it forms a parallel strand in the history of psychology. See Haller (1963) and Zenderland (1998).

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else's. In short order the "conditioned reflex" developed by Pavlov and Bekhterev in very different contexts (see Razran, 1965; Todes, 1997; and Windholz, 1997) became a *vade mecum* for Watson: a fix-all category or mold into which he poured any and every conceivable psychological function.

This is not to say that Watson did no research in an attempt to give substance and credibility to this concept. Indeed, he did (e.g., Watson & Morgan, 1917), but the results of that research were unreliable and, at best, ambiguous. Furthermore, Watson poured into his new conceptual and linguistic mold not only his own empirical observations but also the theoretical viewpoints of others. For instance, he translated the psychoanalytic views of Sigmund Freud into talk about conditioned reflexes (e.g., Watson, 1916b, 1916c). As a result of moves like this, it could be argued (as Adams, 1934, argued) that no truly novel insight into human behavior proceeded from Watson's work. What he provided was simply new ways of expressing old insights, with the unconscious (for instance) being discussed in terms of "unverbalized responses" and the results of childhood trauma being described as "conditioned emotional responses."

The "conditioned reflex" was not the only thing that Watson imported from physiology into psychology. One other set of crucial imports included two terms that became the most privileged words in the behaviorist lexicon: "stimulus" and "response." Even more than "conditioned reflex," these terms shaped the ways in which twentieth-century American psychologists thought and spoke about behavior. As Watson (1919) put it, the goal of psychological study—the way in which it would predict and control behavior—was

the ascertaining of such data and laws that, given the stimulus, psychology can predict what the response will be; or, on the other hand, given the response, it can specify the nature of the effective stimulus. (p. 10, italics dropped)

Unfortunately, although Watson granted that "we have to extend somewhat" the original meaning of these terms when we use them in psychology (p. 10), he never spelled out the ways in which *psychological* stimuli and responses differ from *physiological* stimuli and responses. The resulting uncertainty about their precise meaning plagued the rest of twentieth-century psychology. As graphically illustrated by the reports of major behaviorists in Sigmund Koch's monumental *Psychology: A Study of a Science* (1959-1963), psychologists have used "stimulus" and "response" in reference to many different things (see Koch, 1959). As a result, to the extent that the nearly universal use of these two terms made behavioral views seem more monolithic than they were, their use has obscured the significant variety of ways in which behaviorists have actually thought about behavior.

From our vantage point in history, Watson served primarily as an advocate for new ways of thinking and talking about human behavior. This was especially true after 1920, when he left academic life for a career in advertising. About the same time a new proponent of the objective study of behavior, Edward C. Tolman (1886-1959), entered the field with an article on "A New Formula for Behaviorism" (1922). Tolman was as open and nondogmatic as Watson was unbending and aggressive. His thoughts and language were unencumbered by any

form of orthodoxy. Perhaps no one else in the history of modern psychology has created such a panoply of neologisms and compound terms, from “sign-gestalt-expectancies” and “means-end readinesses” to “belief-value matrices,” all offered in attempts to capture and explain the nuances of empirical observations that fall outside previously accepted conceptual categories and linguistic formulations.

Tolman was, in fact, a “cryptophenomenologist,” as he himself acknowledged (Tolman, 1959, p. 94). Instead of relying on physiology for insights he turned to his own experience. Stimulated by the neorealism of his teachers at Harvard, especially E.B. Holt and Ralph Barton Perry, who were in turn inspired by William James (see Smith, 1982), Tolman developed the first major neobehaviorist psychology—a psychology focused on “molar” rather than “molecular” behavior: on “actions” rather than “twitches.” Instead of banishing purpose and cognition, Tolman found them “out there” in the world of observable behavior (Tolman, 1925). His ingenious experimental designs allowed organisms to *show* their thinking and choices by means of their actions. For instance, he argued that rats demonstrate decision-making at “choice points” in mazes and that they convey the purpose of their actions when they persevere—despite making mistakes and reaching dead-ends—until they reach the end-point of a maze (Tolman, 1938a).

These examples from research that used mazes are particularly appropriate because, as Smith (1990) has shown, the maze provided a perfect expression of Tolman’s vision: He actually believed that the world *is* a maze in which organisms, including humans, have to find their way, using maps and other cognitive strategies. Thus, Tolman found that he needed to use concepts like “maps,” “hypotheses,” and other “intervening variables” to understand and convey the factors that guide behavior, whether performed by humans or other animals (see Tolman, 1948). Like Watson, Tolman incorporated the insights of others, including Egon Brunswik and Kurt Lewin, but unlike Watson he did so by enlarging his system to include their insights rather than shrinking their insights to fit his system (see Tolman & Brunswik, 1935; Tolman, 1939).

Perhaps more than any other major behaviorist, Tolman approached experiments as “demonstrations”—not in the sense of providing proof but rather in the sense of providing examples or illustrations of what he thought and meant to say. He underscored the tentativeness of his views in his magnum opus, *Purposive Behavior in Animals and Men* (1932), by urging his readers not to take up his propositions, “save in a somewhat amused, a somewhat skeptical, and a wholly adventure-seeking and pragmatic behavior-attitude” (p. 394). His sincere desire that others, including his students, should think and speak for themselves is one of the reasons, besides the overall dominance of the behaviorist ethos in mid-century, that it took almost three decades before other behaviorally oriented psychologists started to discover the cognitive dimension of behavior that Tolman saw so clearly (see Krantz & Wiggins, 1973).

Even as Tolman was proposing his purposive behaviorism with its rejection of physiological mechanisms as explanatory devices, Clark L. Hull (1884-1952) was giving new life to the proposition that the conditioned reflex should be the central explanatory concept in psychology (Hull, 1929). In fact, Hull felt that conditioned

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reflexes could be used to explain thinking and other forms of behavior. The key, for Hull, was that both thinking and other forms of behavior were mechanical: they all worked according to carefully regulated S-R sequences. In the case of thinking, the S's have simply become so routinized, according to Hull (1930), that they have faded into "pure stimulus acts" within an automated, "short-circuited" R-R-R- . . . sequence.

Though not as apparent later in his career, when he cloaked his system in terminology and structures that were consonant with logical positivist strictures (see Smith, 1986), Hull came to the study of behavior from a background in engineering and with a preexisting commitment to think and talk about phenomena—any and all phenomena—as embedded within a world-system that entailed diverse levels of functionally related mechanisms. In his view the world was a relentlessly rationalized system in which the scientist's own behavior, including observation, experimentation, thinking, and theory-construction, was as intricately enmeshed and determined as anything else. With good reason, he thought about naming his major work *Psychology from the Standpoint of a Mechanist* rather than *Principles of Behavior* (1943), which was its eventual title (Smith, 1990, p. 252).

It is difficult for us, half a century later, to fully appreciate just how dominant Hull's way of thinking and talking about behavior became in psychology between 1943, when *Principles of Behavior* was published, and 1954, when Sigmund Koch issued a devastating analysis of the empirical bases of Hull's exquisitely defined and logically related categories. (The ink that had been spilled over "excitatory potential" and "drive reduction" alone would have provided a month-long supply for several colleges.) Benefiting from widespread concern about the continuing existence of multiple "schools of psychology," including multiple schools of behaviorism (see Heidbreder, 1933; Murchison, 1930; Woodworth, 1931), Hull (1937) appealed to those who yearned for a confirmation of psychology's scientific status and for quicker progress toward definitive explanation (hence prediction and control) of behavior. Psychologists looked to Hull as they also looked to logical positivism and operationism (see Green, 1992; Koch, 1992) for inspiration and instruction as much as for specific psychological conclusions. Although others, including Tolman (1936), had already begun to use operational definitions to clarify what they thought and meant to say, it was Hull's *Mathematico-Deductive Theory of Rote Learning: A Study in Scientific Methodology* (Hull et al., 1940) that gave the most extreme statement of the idealized form of scientific thought and expression to which Hull himself aspired. With this statement and his subsequent promulgation of principles from which all the facts of individual and social behavior were supposed to be deducible (Hull, 1943), Hull shifted the priority in behavioral analysis from the experimentalist's laboratory to the psychologist's mind and from informal theoretical formulations to rigorous theory-construction. Indeed, Hull himself did no experimentation once he came into his own as a theorist. His self-appointed job was to dictate and systematize what others were to study and discover (see Morawski, 1986). If Tolman distrusted theory, especially his own theory, and encouraged others to continue exploring and amending what

they thought and said about behavior, Hull aspired to theoretical conclusiveness—and had others work to show that his theory possessed it.

Given how much Hull treasured his principles and how pressured he felt to follow through on his promise to deduce the individual and social phenomena of human behavior from them, it must have been hard for him to acknowledge that he was having difficulty linking his principles with reality. In 1951, instead of publishing the deductions regarding individual behavior that he had promised, he issued *Essentials of Behavior*, a book-length correction of the principles he had enunciated in 1943. Then, in 1952, the year of his death, he published *A Behavior System: An Introduction to Behavior Theory concerning the Individual Organism*. Even before Koch's (1954) critique of his *Principles* and *Essentials*, it was apparent that Hull's system had some holes in it.

It was in this context that the work of B. F. Skinner (1904-1990), which was much more empirical in nature and (in some regards) much more humble in prospect, came to the fore. If Hull was, for a while, the hare in the race for a definitive theory of behavior, Skinner was the tortoise who just kept coming and coming—indeed, for another four decades after Hull's death. Though Skinner was never accepted by the mainstream of the discipline to the extent that Watson, Tolman, and Hull were (e.g., he was never elected to the presidency of the American Psychological Association as each of them were), his system of behavioral psychology has nonetheless lasted longer than theirs, and not just because he outlived his behaviorist peers. Despite the controversies his psychological system aroused, Skinner's system fit its time better than any contemporary alternative, and it fulfilled part of behaviorism's original mission by offering control of at least some behavior under at least some conditions, even if it fell short of the behaviorist ideal of being able to predict and control all behavior.

Skinner started from the premise that the reflex should be the central concept of psychology (Skinner, 1931), but he soon came to distinguish two types of reflex or behavior, “respondent” and “operant,” and he devoted himself to clarifying the relations between “operant behavior,” on the one hand, and environmental contingencies on the other (Skinner, 1938). Only later did he articulate the Darwinian vision that enfranchised his way of thinking and talking about behavior (e.g., Skinner, 1966, 1969). Operant psychology, he said, views the behavior of individuals as simply emitted and then selected (or not selected) according to its consequences. Some consequences lead to (or more precisely, are correlated with) the survival or repetition of a behavior; others are not. Here, in a nutshell, is the Darwinian triad of variation, selection, and utility, or more simply, “selection by consequences” (Skinner, 1981). Just as individuals within species vary randomly and are differentially able to propagate their kind according to the utility of their varying characteristics, so too are the behaviors of individuals more or less likely to be propagated according to their consequences.

This was not a new way of thinking and talking about psychological phenomena. Half a century earlier William James (e.g., 1878/1983a, 1890/1981) had already taken a thoroughly natural selectionist approach to psychological phenomena. One big difference, however, is that James applied the approach to all

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psychological phenomena, including mental functions, both conscious and unconscious. Like Darwin, he assumed that mental processes had evolved because of their tangible benefits or survival value; they are not epiphenomenal or vestigial. As noted earlier in this article, James believed that they make a difference. Beyond that, James claimed, selection operates at every level of functioning from sensation to perception, conception, thought, evaluation, and action. While developing a psychology that relied upon a natural selectionist framework, James also developed a pragmatist view of beliefs and other types of comprehension, and he insisted that all forms of behavior, conceptualized in traditional terms as “reflexes,” “habits,” and “ideo-motor actions,” are similarly defined by their consequences. (One of his students, Edward L. Thorndike [1898], applied this general conception in his articulation of “the law of effect.”) Soon after James’s classic treatments of these topics, John Dewey insisted, as we have seen, that all conduct (other than trivial reflexes) involves mental processing as a vital and effectual component of fully integrated “reflex circuits.” Later, although he welcomed the advent of behaviorism in general, Dewey (1930/1984) expressed disappointment that behaviorists had missed an opportunity to eliminate rather than perpetuate the Cartesian separation of the mental and the physical, not to mention the social.

In any case, whether entirely novel or not, Skinner’s selectionist approach was distinctive within the narrower confines of the behaviorist tradition, and once it began its rise to prominence in the waning years of Hull’s influence, it generated a great deal of experimental and applied research (e.g., see Honig, 1966). One attractive feature of operant psychology was that its contentions could be readily demonstrated in an experimental setting. Ernest R. Hilgard, for instance, was pleased to discover how well operant conditioning worked when he first tried to demonstrate it in front of students during a summer course in 1947, though (as he noted) Edwin Guthrie provided an alternative explanation of the phenomenon (Hilgard, 1994, ch. 14, pp. 2-3). Even if the effects were limited to the confines of a Skinner box, behavior could clearly be controlled and changed within a very short period of time. Before long, practical applications—outside the Skinner box—were found to be more or less successful. Even partial successes were notable and suggested that there was something real and worthwhile in what Skinner was saying. His approach *worked*, at least sometimes and in some situations in which interventions had been impossible or less successful in the past.

The prospect of finally gaining control of behavior, especially human behavior, was exciting—and controversial. The publication of *Walden Two* (Skinner, 1948) and *Beyond Freedom and Dignity* (Skinner, 1971), the establishment of behaviorist communities, and the application of operant conditioning in classrooms, clinics, prisons, and homes sparked public enthusiasm, protest, and debate. Nonetheless, Skinner’s way of thinking and talking about behavior caught on. Even those who were bothered by the deterministic worldview that was implied by operant conditioning began to ask themselves about the consequences of their own behavior in relation to their children, spouses, and workmates. Certainly, the suppression of punishment and the increased emphasis

on rewards that was apparent in many social settings in the United States during the final three to four decades of the twentieth century were related to larger cultural trends, of which Skinnerian psychology was but one element, but Skinnerian psychology clearly influenced American culture even as its use was fostered by it, as Skinner himself would have expected. (On Skinner's place in American culture and the changing status of the culture's technological ideal, see Rutherford, 2000, 2003; Smith, 1992, 1996.)

Still, the empirical evidence for the efficacy of operant conditioning was and remains limited as well as real—limited in relation to instances in which it works and in relation to the degree to which it works. Furthermore, even when operant psychology works, it might do so for reasons other than those provided within its own conceptual framework. In this regard, the power of specifically cognitive contingencies, especially with regard to fully functioning adults in natural environments, suggests that a broader understanding and application of Darwinian principles, along the lines represented by the thought and work of James and Dewey, might be appropriate.

Some of the developments of operant psychology have already extended in this direction (e.g., see Gifford & Hayes, 1999). The difficulty these extensions are likely to encounter might revolve around the arguably special nature of social phenomena and the resulting difficulty of analyzing higher-order (social) processes and integrated units into distinguishable parts (individual processes) that can be used to generate knowledge that is unexpected, useful, and satisfying. Allport (1939) pointed to this problem in relation to John Dewey's psychological ideas. About the same time, Tolman (1938b) offered some relevant speculations on the primacy of the social vis-à-vis the psychological, but his later (1951) contribution of "a psychological model" appropriate for a more "general theory of action," in which he used Lewinian concepts like "behavior space," exemplifies the difficulty that has been pointed out. Earlier, J.R. Kantor had tried to address similar issues in his interbehavioral psychology. His approach might still hold promise (see Hayes & Fredericks, 1999), but in the United States, at least, his work has been largely neglected except among a relatively small circle of disciples. All in all, the radical individualism in American culture and psychology, despite recent criticisms (echoing Sampson, 1977, 1981), does not provide fertile ground for social and cultural analysis (as called for by Malagodi, 1986). It may well be that developments along these lines will have to take place in Mexico and other countries where transindividual theorists and theories have stronger audiences and support.

Returning to Skinner's way of thinking and talking about behavior, several additional things might be said. First, when Skinner stepped out of the laboratory, he showed little hesitation in extrapolating from circumscribed experimental situations and results (Ferster & Skinner, 1957) to behavior at large. By "behavior at large" I mean behavior outside the laboratory involving humans instead of pigeons: Behavior that is much less limited in complexity and context. I am far from the first to point out that talking about speaking, voting, seeing, and introspecting as so many forms of behavior (as Skinner did in speaking of "verbal

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behavior,” “voting behavior,” “the behavior of seeing,” “the behavior of introspecting,” etc. in Skinner, 1964, 1974, 1987) makes “behavior” cover an amazingly broad range of activities, which themselves entail a variety of sub-activities (e.g., see Hibbard & Henley, 1994, pp. 563-564). As for Skinner’s operant “translations” of these activities and his treatments of other behaviors that are typically described with intentional language, Daniel Dennett’s (1978) criticism and conclusion seem valid: Skinner failed to show that a psychology capable of dealing with such topics can do so without some form of mentalism. For related reasons Mackenzie (1977) argued that Skinner might best be described as a phenomenologist (pp. 160-170)! (On this point, see Day, 1969). All of this suggests the appropriateness of a return to some kind of “mentalism,” perhaps of the sort associated with James and Dewey.

One other prominent feature of Skinner’s way of thinking and talking about behavior of all sorts—especially when he stepped (as he often did) beyond the available experimental evidence—was his extraordinary glibness, which could frustrate supporters and opponents alike. Rather than engaging in analyses that might, once in a while, reach a conclusion that he had not expected, Skinner would simply assert what was so, based on more or less logical deductions from his own pre-established assumptions. As Plotkin (1987) complained, “we are simply told, and the telling is unconvincing and inadequate” (p. 147). While Plotkin said this in relation to Skinner’s natural selectionist analogy, it was also true in many other contexts. Skinner seemed to have answers for anything and everything. Many of those answers were at variance with the theories of other psychologists; some were at variance even with the data of his colleagues. If he wondered what had happened to the science of behavior, it may simply have outgrown—and broken—the premature conceptual and linguistic frameworks that had been placed around it. No amount of thinking and talking within these frameworks was likely to put them back together again.

Conclusion

In this article I have emphasized not only the historical novelty of the twentieth-century quest for *a* scientific account of “behavior,” I have also underscored that there were a variety of attempts, none of which earned the right to be *the* account of “behavior.” Indeed, even the basic terms of such an account, including “stimulus,” “response,” and “behavior,” were defined in a variety of ways, and in the early twenty-first century, consistency is still nowhere in sight. To be sure, within individual research traditions—especially the Tolmanian, Hullian, and Skinnerian traditions—there were periods of relative unanimity, but those periods are probably best explained by socialization and related social factors. As Skinner (1987) noted, “when we became psychologists, we learned new ways of talking about human behavior” (p. 783). Koch (1976) has given a particularly persuasive analysis of the ways in which individuals are inducted into “language communities” that function as “search cells” in the advancement of “psychological

studies.” (For other treatments of the social dimensions of scientific research traditions see Krantz, 1971; Polanyi, 1958; and Toulmin, 1972.)

What enlivens and extends a scientific tradition is, to borrow a phrase from the Brelands (1961), “the *misbehavior* of organisms”—in this case, the misbehavior of scientists. To the extent that behaviorism may be said to have moved forward as well as left and right and back again over the past century, we might grant, with a nod to Feyerabend (1975), that it has been the result of a bit of anarchy, of individuals following new leads based on their own idiosyncratic premises. In each of the cases we have reviewed it is clear that the premises have been derived from fresh and compelling hunches, in turn stimulated by metaphorically structured views of the world and its creatures. Tolman’s maze, Hull’s machine, and Skinner’s natural selectionism have oriented what each of them thought and said about behavior.³

If there is something to learn from all of this, it could be that variation, selection, and consequences are also at play in the history of science (see Campbell, 1960, and Richards, 1977, 1987b). From the vast array of ideas and data that scientists have at their disposal, some are selected for further elaboration or accumulation, and the theoretical and practical consequences of the resulting conceptual and linguistic constructions are noted and evaluated, sometimes by the public as well as by the scientific community. In the process of trading one insight off another, scientific understanding evolves over time. William James, for one, would not have been surprised to hear someone suggest this; in very specific terms, he claimed long ago that this is precisely how science and understanding advance (see James, 1878/1983a, 1880/1897/1979; Leary, 1992, 1995b).

As regards our earlier review of the historical development of the concept of “behavior,” arising as it did from comparative and functional psychology and within the social and cultural context of the Progressivist Era, we might note the following:

1. Comparative psychologists were never converted, as an entire group, to the strictest code of behaviorist theory and practice. Even during the 1930s, when behaviorism was arguably at the height of its ascendancy, research on animal cognition flourished—and not just in the form represented by E.C. Tolman and his students (see Dewsbury, 2000). In more recent years there has been a huge amount of work in this area (see Griffin, 1984, 1992; Vauclair, 1996). The critical question, then, is not whether research on animal consciousness, cognition, and decision-making is *possible*—that is rather like asking if the bumblebee can fly—the question, instead, is how far it has taken us. At minimum, it has taken us beyond the claims that such work could never achieve significant results. Ultimate truth in this field might be as problematic as in any other, but it is not as distant as

³ I have written in other places about the general role of metaphor in the history of psychology (Leary, 1990) and about its particular role in one interesting and illustrative episode in the history of behavioral psychology, namely John A. (Tony) Nevin’s recent use of Newtonian metaphors in his work on “behavioral momentum” (Leary, 1995a). In addition, Larry Smith, upon whose research I have drawn repeatedly in this article, has dotted the *i*’s and crossed virtually every *t* with regard to Tolman’s, Hull’s, and Skinner’s metaphorical visions (Smith, 1986, 1990).

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it once seemed to those who condemned talk of consciousness and cognition out of hand.

2. Something like functional psychology of the kind espoused by James and Dewey seems to offer continuing insight. To many, James still has something worthwhile to say about the experience of perceiving, thinking, and creating, and we would do well to heed Dewey's exhortation that we think broadly and persistently about the interrelated and invariably *social* nature of situations and events. Given advances in knowledge since the time of James and Dewey, perhaps it is best to think of their theories as orienting devices. In any case, we do not seem to have come so far that we can simply ignore them. Rough maps are better than no maps at all, and often they will do all we need to have done. Certainly they show the overall terrain better than detailed snapshots of minute pieces of territory, taken through the latest lenses that are here today and gone tomorrow.

3. If there was a felt need for prediction and control in an earlier historical era, our culture now seems more interested in asking us to deal with the problematic relations between the diversity of individual identities, desires, and meanings, on the one hand, and the aspiration for social solidarity and workable democratic procedures on the other (in short, how humans can live and work together in peace and mutual respect). Obviously, the apparent demands of our times might create—and are even likely to create—partial blindness and premature theorizing, as in the past, but chastened by awareness of prior hubris we are probably in greater danger of doing too little than stretching too far.

All in all, it seems that the history of “behavior” as a subject of study over the past century can serve several useful functions. It can prompt us to respect the concerns and achievements of those who have gone before us, and it can encourage us to be more self-conscious and self-critical—but not timid—as we consider the challenges of our own particular moment in time. At minimum, these challenges include the continuing quests to understand the relations between consciousness and behavior (or, as Dewey put it, between experience and conduct), the relations between humans and the rest of the universe (including but not limited to the rest of the animal kingdom), and the relations between these preceding relationships and our participation in the evolution—preferably the amelioration—of our world's various societies and cultures.

Whatever became of the science of behavior? We are not yet able to give a final answer.

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