

- W: Enough to explain why I am saddened, and often depressed, by what has been happening to "behaviorism", to behavior science generally, since 1913. How far have we come since then? Seems to me we may have slipped backwards. It looks to me sometimes, in my more depressed moments, like eighty years of no progress.
- S: You've got me sympathizing with you again. What would you suggest be done? Abandon "behaviorism"?
- W: Hardly. I'd say rather that we need *more*, not fewer, "radical behaviorists". *More* stubborn, *more* intransigent, ones. If I judge you aright, I'd say more like you. *More* science, not less. *More* attention to the data, not less. *More* care in theorizing, not less. Behavior science needs to become *more* behavioristic, not less. And the arts and humanities be given *more* assurance that l'behaviorism" is not a threat to them, but rather a path toward a better understanding of man's humanness. Science does not have an aim to understand all of man's humanness, but only what it can, with its methods and techniques, assay. To say otherwise, is to raise false alarms.
- S: Time for another "manifesto"?
- W: Yes, with perhaps a bit more emphasis on the point that "behaviorism" is not a philosophy, but an approach to science. Fearful philosopher have been ranting against a "behaviorism" they have only imagined. Their armorium of critiques has been directed at non-existent target. They remind me of Cicero's wry thrust that "there is nothing so absurd that it has not been said by some philosopher".
- S: I always did think well of old Cicero, or old "Roman nose" as we young students used to call him.
- W: Which leads me to Voltaire's sardonic suggestion which, for us, would be to give our anti-behavioral philosophers singing lessons because "anything too silly to say, can always be sung". So, I'm on my way. I leave to you the arena, and the struggle, for a genuine science of behavior. Adieu, or better perhaps in this city and university, adios.
- S: I'll miss you, you old curmudgeon.
- W: I will miss you, too, youngster. But (if I rightly remember) that's life, or again better, *esta es la vida*.

Behaviorism from the standpoint of a neobehaviorist¹

El conductismo desde la perspectiva de un neoconductista

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Abstract

The essence of the behaviorist position is extracted out of John B. Watson's original views, in his 1913 paper and his 1919 book *Psychology from the standpoint of a behaviorist*. It is argued that these views are indistinguishable from what later became the body of metatheoretical tenets known as neobehaviorism.

Key words: John B. Watson, behaviorism, neobehaviorism.

Resumen

Se extrae la esencia de la posición conductista de los puntos de vista originales de John B. Watson, en su artículo de 1913 y su libro de 1919, *Psychology from the standpoint of a behaviorist*. Se sostiene que estos puntos de vista son indistinguibles de lo que posteriormente se convirtió en el cuerpo de los supuestos metateóricos conocidos como neoconductismo.

Palabras clave: John B. Watson, Conductismo, neoconductismo.

¹ This essay is adapted in part from a portion of Chapter I of a book, *Behaviorism, neobehaviorism, and Cognitivism in Learning Theory* (Amsel, 1989), and from an article, "B.F. Skinner and the Cognitive Revolution" (Amsel, 1992).

Over the past 30 years, there has been a shift in emphasis from behavior to cognition and to the acquisition of information as the objects of psychological study, and investigators, not only of human cognition but also of animal cognition, have taken it as a requirement of their new faith to rationalize this conversion. The major focus of their attacks have been on B.F. Skinner, who has represented radical behaviorism and, by historical extension, on the historical archetype of behaviorism, J.B. Watson. As I will point out, the cognitivists do not recognize the difference between B.F. Skinner of the 1930s and the J.B. Watson of 1925. My purpose in this paper is to extract the essence of the behaviorist position out of Watson's *original* views, his 1913 paper and his 1919 book, *Psychology from the standpoint of a behaviorist*. I will argue, as I have done previously (Amsel 1982, 1989), that these views are indistinguishable from what later became the body of metatheoretical tenets known as *neobehaviorism*.

Two Watsons and two Skinners

Skinner, in an obituary for Watson, judged Watson's *Psychology from the Standpoint of a Behaviorist* to be his most important book, and one might infer that, even at the time of the obituary, in 1959, Skinner took this book of Watson's to reflect, or at least to be the forerunner of, his own position. Better than any other of Watson's writings, I think, this book does reflect the original rather basic scientific tenets of the "behaviorist revolution." (Of course, Watson's 1913 paper, which we commemorate today, was the forerunner of this book.) My specific thesis is that the definition of psychology these modest tenets require is still by far the most acceptable one; that, paradoxically, it was the 1913/1919 Watson, and not his later more rigid position, that inspired and defined what came in the 1930s and later to be called "neobehaviorism," and that the later, more doctrinaire Watson of the 1925 book, *Behaviorism*, with the one outstanding exception, Skinner (and his followers, of course), does not represent any behaviorism since that time. And even in this case, the exception is the post-1950 Skinner of the paper, "Are Theories of Learning Necessary?" Skinner's *Behavior of Organisms* (1938) and his earlier papers were clearly not in the mode of Watson's more radical, later position; early Skinner was much more aligned with what later came to be called *neobehaviorism*, which, para-

doxically, was an offshoot of Watson's original behaviorism. Let me try to justify these views.

From 1930 to 1937, Skinner published a number of experimental and theoretical papers on conditioning (e.g., Skinner, 1931, 1932, 1935, 1936a,b, 1937). In the latter two of these papers he was engaged in a discussion that revolved about "types" of conditioned reflexes. In two others he addressed the concept of drive (Skinner, 1936a,b). In 1938, in his book, *Behavior of Organisms*, Skinner was very much a theorist, offering a set of hypotheses—laws, he called them—that had a neurophysiological flavor, very much in the Sherringtonian mode. As late as in 1941, Skinner published with Estes a paper with the word "anxiety" in the title. This paper introduced the term *conditioned emotional* response, which involved a procedure with animals later termed *conditioned suppression*, that was widely accepted as an indirect measure of aversive pavlovian conditioning. This was still Skinner very much in his early watsonian phase: Watson and Rayner (1920) had performed a similar experiment with the child, Albert, and they called their procedure *conditioned emotional reaction*, and the emotion, fear. In neither case, Estes and Skinner's in 1941 or Watson's in 1920, was the experiment under consideration guided by a radical behaviorism.

A decade later, a rather different Skinner emerged in the now-famous paper, "Are theories of learning necessary?" (Skinner, 1950). It marked a sea-change in Skinner's metatheoretical position. It represented a move very much like Watson's: away from the moderate, classical behaviorism of 1913/1919 toward his radical behaviorism post-1925. It was this later, more doctrinaire watsonian position, adopted by Skinner in 1950, that blossomed into what was called The Experimental Analysis of Behavior. In a recent note (Amsel, 1992) I ventured the opinion that the hardening of Skinner's metatheoretical position—from the earlier to the later watsonian form—was a powerful (perhaps the principal) factor, along with Chomsky's review (1959) of Skinner's 1957 *Verbal Behavior*, in the emergence of the Cognitive Revolution in psychology in the early 1960s.

The Watson of 1913/1919 and Neobehaviorism

It suited (and suits) the cognitivists in their polemical references to behaviorism do not, as a rule, address the distinction between behaviorism and neobehaviorism; they are misleading by omission about the tenets of neobehaviorism

and of early watsonian behaviorism, and the intellectual climate out of which they arose; and as we have seen, even the best of them seek to create a caricature of behaviorism in its most radical form. A good recent example is in Schwartz and Lacey's book, *Behaviorism. Science, and Human Nature* (1982), in which Skinner is identified as "the leading figure in behavior theory" and as "the spokesman for the discipline" (p. 226). And a particularly telling statement in this regard occurs early in the book: "What distinguished behavior theory is ... that an analysis of how environmental events affect behavior will tell us all we need to know about the determinants of action" (pp. 14-15). This may indeed have characterized the behaviorism of post-1950 Skinner and his followers; it does not, however, characterize Tolman's, Hull's, or other brands of neobehaviorism; it specifically does not characterize Watson's position in 1913/1919, or Skinner's in 1938.

Let me expand on this by examining Watson's *Psychology from the Standpoint of a Behaviorist* (1919). This book starts with a metatheoretical introduction and the message is, of course, the new Behaviorism. Consequently, in the spirit of the 1913 classic paper, Watson's book carefully omits terms and language connoting introspective mentalism. In other respects, however, the topics are like those in other psychology textbooks of the time. There are chapters on methods, the various sense organs, the motor system, emotions, instincts, habit, thinking, and memory. As Watson wrote in the preface, behavioristic psychology recognized that "the differences among the various sciences ... [are] only those necessitated by a division of labor." In this sentiment, he anticipated the main premise in Pratt's *The Logic of Modern Psychology* (1939), published 20 years later. Pratt's title was an obvious play on Bridgman's *The Logic of Modern Physics* (1927), which promoted the "operational character of concepts" as a way of dealing with the more relative nature of knowledge in the move from newtonian to einsteinian physics. Operational definition became the catchword, and to some extent the hallmark, of experimental psychology in the 1940s and 1950s.) The difference among sciences, according to Watson (and later, Pratt), are only those dictated by the different constructions of the world emerging out of the objectively verifiable perceptions of observers with different interests. Watson's strong position was that it should take no special training of the structuralist (Wundtian/Fitchenerian) variety to be an observer, that this special kind of training removed from the observer an essential naiveite, and that this special kind of observation of the structuralists, introspection, was in principle unverifiable and therefore unscientific. *This is what behaviorism meant to Watson.*

Reading the 1913 paper and first chapter of Watson's (1919) *Psychology* ("Problems and Scope of Psychology"), one is brought face-to-face with the differences between the metatheoretical positions of 1913/1919 Watson (and other early behaviorists) and the caricatures of Watsonian behaviorism that exist today, and which, it must be said, owe a lot to Watson's own later writings. Watson, radical behaviorist-to-be, refers to hereditary responses, both explicit and implicit; the muscle-twitch, switchboard psychologist writes "we can leave out ... [comparisons of] the action of the central nervous system with ... electric switchboards, and the like" (Watson, 1919, p. 19); the founding behaviorist who gave us what Bower (1975), in a moment of cognitive fervor referred to as the "colorless movement" and the "glandular squirt," writes that, to prepare to be a psychologist, learn your physiology, experimental zoology and pharmacology, spend some time (as an observer, one assumes) in the psychiatric clinic, learn something about business and law, and get some training in statistical methods. These are rather remarkable admonitions from a radical behaviorist-to-be. It has always struck me that, in some of these last respects, the ones having to do with practicality and application, Watson and The Behaviorist of our generation, B. F. Skinner, were very much alike; in other respects—e. g., physiology and statistics—they were of course very different. But, again, this was the earlier Watson, the Watson who summarized and represented the essential behaviorism of his time, and who, anachronistically in a sense, anticipated the broader neobehaviorist movement that was to follow. In the later book (*Behaviorism*, 1925, revised 1930), we see a more polarized, more doctrinaire Watson (no longer a member of the academic community). In 1959, nine years after the publication of his own "Behaviorist Manifesto" in 1950, Skinner, described the controversy that Watson's post-1919 writings aroused in these words: "Watson's taste for, and skill in, polemics led him into extreme positions from which he never escaped" (p. 19). The important point is that it is only to this side of Watson's writings that the catch-phrases of Watson's detractors of yesterday and today can be directed. I should like to take a moment to expand on this point, as I have done on other occasions (Amsel, 1982, 1989) by examining other parts of Watson's (1919) *Psychology*.

In 1919, methods, for Watson, fell into four categories: observation (direct and with the use of instruments), conditioned reflex (secretion and motor), verbal reports of all kinds, and psychological tests. The emphasis was on objectivity, and in a footnote the instructor was advised to demonstrate in the classroom a series of not-very-revolutionary, functionalist methods: the recording of respiration and motor changes, a word-association test, recording of

eye movements on reading, and the army alpha test. The new "revolutionary" behaviorism had no elementism, no "muscle-twitchism," no "building blocks" about it. No new methods were involved.

The 1913/1919 Watson dealt with emotions and instincts as "hereditary modes of response." The well-known Watsonian trio of emotional responses, fear, rage, and love (not mentalistic terms for Watson because, I suppose, he thought them closely tied to behavior), were described as part of "the original and fundamental nature of man." (These are not the words of an arch-environmentalist.) What is most remarkable, in one who came to be known as an archenvironmentalist, is Watson's adherence to the view that these emotional responses are hereditary, instinctive patterns of behavior, and his strong preference for a "genetic" (in our present sense of "developmental") approach to their investigation. This functionalist emphasis on "genetic psychology" is a strong feature of Watson's behaviorism, and it characterizes his famous experiment with Rayner (1920), to which I have referred. Without stretching things too much, this experiment can be regarded as the cradle of the conditioning and behavior therapies. For learning theorists, the concept and the substance of the "conditioned emotional response" of Estes and Skinner (1941)—even the more neutral-sounding "conditioned suppression" paradigm, so favored as an indirect measure of Pavlovian conditioning by contemporary theorists like Rescorla and Wagner, can also be found in Watson and Rayner's work with Albert and his conditioned fear of white furry objects (Watson and Rayner called it "conditioned emotional reaction"). The conditioned emotions of early Watson are close kin to the conceptualizations, in Pavlovian incentive-motivational terms, of emotional states such as fear and anxiety of the neobehaviorists (e. g., C. L. Hull, K. W. Spence, N. E. Miller, O. H. Mowrer, J. S. Brown, R. L. Solomon).

Watson's textbook: Heredity and Environment, the CNS, the Concepts of Instinct and Habit and "Building Blocks"

In his early systematic writings, Watson acknowledged the importance not only of the concepts of heredity and learning, as did the later neobehavioristic theories of Tolman and Hull, but also of the concept of instinct. "In instinct," Watson wrote, "the pattern and order are inherited, in habit both are acquired during the lifetime of the individual" (1919, p. 273). In his treatment of instinct and habit, Watson's commitment to the developmental ("genetic") approach is

again obvious. He describes in detail the development of eye-hand coordination in a child from the 80th to the 171st day. Another characteristic of early but not later Watsonian behaviorism, seldom mentioned, was his alliance of behaviorism and physiology. This could have been a forerunner of Skinner's (1938) physiologizing in *The Behavior of Organisms* and Hull's (1943) in *Principles of Behavior*. For example, in discussing the importance of kinesthesia in sequential habits, Watson raises the possibility that, late in training, cortical function may be replaced by activity at lower centers, a kind of short-circuiting process not at all out of line with current neurophysiological thinking, and he refers to the work of Franz and Lashley on what came to be known as equipotentiality of cortical function.

Gustav Bergmann, an astute historian and philosopher of science, in an essay on the contribution of J. B. Watson, wrote: "Watson had an amazingly naive and almost superstitious distrust of any appeal to the action of the central nervous system," (1956), he had to be referring to Watson's later more polemical positions, from which these considerations were absent. Bergmann's statement clearly does not fit the 1913/1919 Watson who, as I have claimed, was the driving metatheoretical force behind what came to be known as the neobehaviorist movement, whose early major figures were E. C. Tolman, C. L. Hull, and the B. F. Skinner of *The Behavior of Organisms*. Here are some further particulars: Under the heading "determiners of acts" Watson (1919) described what Hull later called the "habit-family hierarchy." Watson also concluded that, of a large number of possible responses to a stimulus, the response that will occur depends, in descending order, on the factors of recency; frequency; context, which he describes as "the general setting of the situation as a whole"; a factor which I will call "recently-induced set"; temporary "intra-organic" (motivational) factors; and the life history of the individual. It should be noted that these factors find parallels in most of the intervening variables in the systematic writings of Tolman and Hull. But what stands out again so clearly in Watson's discussion of habits is that there was nothing in it to justify the "building-blocks" epithet that was later tossed about in descriptions of his psychology. Watson's concept of habit in 1919 was first of all more Thorndikian than Pavlovian. As Skinner (1959) pointed out, whereas Watson's rejection of mentalism was greatly influenced by Lloyd Morgan [and, as Pratt (1939) has written, by James and Jennings], his concept of habit in 1919 bore the greatest resemblance to Thorndike's trial-and-error learning (as, obviously, did Hull's "habit-family hierarchy" and Skinner's concept of the operant).

What I have been describing are elements of a metatheory that can be

seen in Watson's 1919 textbook; however, in briefer terms, all of these were in the 1913 "Manifesto." There is already in 1913 a strong emphasis on application. This was, of course, a strong feature of the American functionalist movement in psychology. I have surprised my students for years by referring to B. F. Skinner as the major applied psychologist of his generation; Watson was, in many similar ways, the applied psychologist of his generation (and this even before he went to work for the J. Walter Thompson advertising agency). This feature of Watson's psychology becomes more explicit in the preface to the revision of his book, *Behaviorism* (1925), in which he calls upon "modern psychology to solve the problems that come from living in complex groups" (p. XI). This psychology can do, Watson writes, by throwing off the yoke of philosophy and the academic tradition, and by finding a way to "seek its facts in the daily lives of human beings."

The picture we have of Watson as a radical behaviorist and arch-environmentalist rests mainly on his "Give me a dozen healthy infants ..." statement, and on his discarding of instinct and down-playing of heredity in this later book, (*Behaviorism*, 1925). This picture does not in any way fit the Watson of the 1919 book, nor the Watson of the 1913 paper, nor the Watson of the Columbia lectures that were published in 1914 as, *Behavior: An Introduction to Comparative Psychology*. As a matter of fact, Watson's classic "brag" in the 1925 book "Give me a dozen healthy infants ..." ends with this not-often-quoted sentence: "I am going beyond my facts and I admit it, but so have the advocates of the contrary, and they have been doing it for thousands of years." Skinner, commenting on Watson's declaration some forty years later, wrote: "As an enthusiastic specialist in the psychology of learning [Watson] went beyond his facts to emphasize what could be done in spite of genetic limitations" (1966, p. 1205). (I have often wondered if Skinner was telling us something about his own environmentalism.)

As I pointed out in a recent little book (Amsel, 1989), commenting on the "building-blocks" characterization of Watson's concept of habit, Watson's original concept of habit was Thorndikian and gave way only later to a more Pavlovian emphasis in the 1925 book, *Behaviorism*. (The seeds of a conditioned-reflex approach could, however, have been sown in his presidential address to APA, "The place of the conditioned reflex in psychology," [Watson, 1916].) It was only later, when he appeared to have become a thoroughgoing Pavlovian, that the "building-blocks" characterization of complex habits, not at all a feature of the position of the earlier Watson, emerged. Such a characterization, incidentally, would be apt for recent work at the cellular level in *Aplysia*

reviewed by Hawkins and Kandel (1984), whose metaphor for building blocks was a "cell-biological alphabet." Ironically "building blocks" would also be an apt characterization of some of the discussions of connectionist neural-network computer models of cognitive function (e. g., Feldman & Ballard, 1982).

Many contemporary neobehaviorists, greatly influenced by the thinking of the major learning theorists—Tolman, Hull and the earlier Skinner—do not subscribe to the more radical behaviorism of the Watson of 1925; but whether they realize it or not, they do subscribe to the Watson of 1913 and 1919, whose message was quite simple: a reasonable and pragmatic level of objectivity and verifiability in psychology, as in all science. And this—and not the more radical, polemical side of his writings—is finally, manifestly, Watson's major legacy to contemporary neobehaviorists. Simply put, it was his insistence that behavior, and not the introspectively revealed mental event, is the datum of psychology that influences us most. As Kimble writes in his *Conditioning and Learning* (1961), "[Watson's early position] was an objective, association psychology, with an emphasis upon habit, biological in viewpoint and analytic in approach" (p. 23) The love-affair of neobehaviorists with operational definition of constructs derived from behavior, and with the unity of science came years after Watson's own infatuation with these ideas, which had, already, even in 1913, been around more informally for some years.

Behaviorists and Behaviorisms

As Gustav Bergman taught some of us years ago, at the University of Iowa, there have, in fact, been several reasonably distinct versions of behaviorism. The seminal one is, of course, Watson's early (1913/1919) version, which was modified and hardened in 1925, some would say to its detriment. There followed Tolman's purposive or molar behaviorism of the 1920s and 1930s; the operational behaviorism of the 1930s and 1940s—Pratt, Stevens, early Skinner, Hull, Bergmann, Spence, and others—which appears to have been anticipated by Watson in 1919 and in Lashley's later writings; and the most recent, the radical (descriptive) behaviorism of later Skinner, which can be dated from the famous article, "Are Theories of Learning Necessary?" (1950). This last version, which Skinner showed some signs of softening in some of his last writings, most resembles the later, more doctrinaire Watsonian behaviorism of 1925. In my judgment, the behaviorism of Hull, with its emphasis on stimulus-response, habit, unlearned S-R connections, adaptiveness of behavior, physiology, its

admonitions to guard against subjectivism and anthropomorphism, and its preference for S-R analyses of terms such as anxiety, purpose, and anticipation—is most like the 1913-1919 Watson (see Amsel & Rashotte, 1984). An historical note in this regard is that, in 1913, Watson acknowledged that his definition of behaviorism was anticipated by W. B. Pillsbury, who was Hull's teacher at Michigan.

But then the behaviorism of Hull's arch theoretical rival, Tolman, was (except that he thought Watson's behaviorism too molecular) also very close to the 1913/1919 Watson; and, except for the flavor of, in Tolman's term, the "intervening variable", and from the perspective of 50 years, was, at least in form, virtually the same as Hull's.

Attacks on Behaviorism: Where do they come from?

Give me a dozen healthy infants, well-formed, and my own special world to bring them up in and I'll guarantee to take any one at random and train him to become any type of specialist I might select—doctor, lawyer, artist, merchant chief and, yes, even beggarman and thief, regardless of his talents, penchants, tendencies, abilities, vocations, and race of his ancestors. I am going beyond my facts and I admit it, but so have the advocates of the contrary, and they have been doing it for thousands of years. Watson, from *Behaviorism* (1925)

As I have pointed out (Amsel, 1989), attacks on behaviorism have usually found a target in the extreme environmentalism reflected in the first sentence of this passage from Watson's *Behaviorism* (the second softening sentence, as Skinner pointed out, is seldom if ever mentioned). Of late, however, the main attacks have been against Skinner's post-1950 brand of behaviorism. They take several forms. Some of these attacks are against an experimental science of psychology and could as easily be directed at Wundt and Titchener. Here are some examples: When Carl Rogers (1964) criticized behaviorism in the book, *Behaviorism and Phenomenology*, it was the reaction of a phenomenologist and humanist against the idea that a science of psychology could be impersonal. When the founding linguist Chomsky, attacked behaviorism, the arguments seemed to be a nativist against scientific empiricism: He was reacting against Skinner's environmentalism and determinism, and, specifically, against any possibility that learning and reinforcement could be involved in the acquisition of language. And when the novelist, Arthur Koestler, in his *The Act of Creation*

(1964), attacked behaviorism, he seemed, like Rogers, to be expressing his distaste, not uncommon in humanists who are not unfriendly toward physical and biological science, for the very idea that a science of *complex human behavior* was even possible. There is not much we can do about criticism of behaviorism or neobehaviorism by the humanists. However, as I pointed out in my book, *Behaviorism. Neobehaviorism. and Cognitivism* (Amsel, 1989), the more recent attacks on behaviorism in the era of the "Cognitive Revolution" in psychology are another matter. I conclude with a passage from this book:

The attacks on behaviorism from within the pale of laboratory psychology are a little more difficult to characterize. There is seldom a quarrel with objectivism, although a keen ear can sense the stirrings in some quarters of a return to introspective methods. Nor is it a matter of choosing between pure and applied science: Many behaviorists, certainly Watson and Skinner, clearly have embodied both interests. If it has to do with what the psychologist does in the laboratory, it is not even always about some preferred area of investigation; ...many of the attacks on behaviorism and S-R psychology—they are seldom differentiated—come nowadays from investigators in the field of animal learning. If there is a disagreement between the positions of the behaviorists (including neobehaviorists) and these animal cognitivists, it is that the former invent constructs to explain behavior, whereas for the latter behavior in itself is said to be unimportant except as a "window on the mind." But many of today's animal cognitivists would not agree with this assertion of mine, mainly because they appear to have forgotten that most behaviorists and all neobehaviorists, like most scientists, theorize in terms of mediating constructs. In order for most of their arguments to have any validity, the defining characteristics of behaviorism must be restricted to the extreme, doctrinaire positions of the later Watson and of Skinner's "Are Theories of Learning Necessary?". If we accept that there are, and have been, other, less radical behaviorisms than Skinner's, the disagreement between these neobehaviorists and the animal-learning cognitivists is, as I have claimed, between explaining behavior and studying the mind; the older disagreement among neobehaviorists was about the explanatory power and rigor of the stimulus-response and the cognitive theories. The latter kind of disagreement was the one between Hull and Tolman; the former, in my view, is the fundamental disagreement that led to behaviorism itself. As we have seen, Watson himself was comfortable with operational definitions of states such as fear and anger. What made him (and makes neobehaviorists) uncomfortable was treating as scientific data the product of the direct examination of such states by the only person who could experience them. (Amsel, 1989, pp. 25-26)

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