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Some Historical and Conceptual Background to the Development of B.F. Skinner's "Radical Behaviorism" — Part 3

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The present article is the third in a series of three that outlines the historical and conceptual background of B.F. Skinner's radical behaviorism as a philosophy of science. Of special interest in this article is the intellectual context of a paper on operationism Skinner published in 1945, in which he first used the term "radical behaviorism" in print. Overall, Skinner's radical behaviorism was a thoroughgoing behaviorism that provided a naturalistic account of the full range of human functioning, including the influence on both verbal and nonverbal behavior of phenomena identified as "subjective."

Keywords: B.F. Skinner, radical behaviorism, operationism

Part 1 (Moore, 2005a) of this three-part series on the development of B.F. Skinner's radical behaviorism examined biographical details of Skinner's life prior to his entering graduate school in September 1928, as well as events in the first quarter of the twentieth century that led Skinner to become interested in psychology. Part 2 (Moore, 2005b) examined the influence of events during Skinner's graduate student (1928–1931) and post-doctoral (1931–1936) years at Harvard. According to Schneider and Morris (1987, p. 33), Skinner first used the term "radical behaviorism" in print in his contribution to a symposium on operationism that was published in 1945. The aim of the present article, the third and final in the series, is to examine the

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background of Skinner's contribution. We begin with a brief review of the historical development of behaviorism, which will lead us to a review of the general intellectual context during the second quarter of the twentieth century, when Skinner began to develop his unique ideas.

From Classical S–R Behaviorism to Mediational S–O–R Neobehaviorism

At the end of the nineteenth century, psychology was generally regarded as a science whose principal subject matter was consciousness, and whose principal method was introspection. However, any contentment with the discipline was more apparent than real. For example, critics questioned whether a psychology purporting to examine consciousness through introspective methods was going to contribute anything of practical value. Critics also questioned whether a psychology concerned with consciousness and introspective methods was ever going to be scientifically adequate, given the continuing failure of introspective psychologists to agree on their methods and findings. By early in the twentieth century the alternative seemed clear. In order to be a science worthy of the name, psychology must emphasize a subject matter that was publicly observable, such as the relation between stimuli and responses, if only so that its technology could ultimately be applied to education, child-rearing, and business. After considerable discussion within the discipline, classical S–R behaviorism, of which John B. Watson was the most vocal advocate, ascended to a position of some, if not total prominence.

Classical S–R behaviorism flourished from 1913 to the early 1930s. By the early 1930s, however, psychologists began to see at least three problems with the validity of classical behaviorism. As noted in Part 2 of this series, the first problem was that publicly observable stimuli and responses just were not always correlated with each other in the way that classical behaviorism required (Moore, 2005b). In particular, there were concerns about both the variability and apparent spontaneity of behavior. The term variability here means that often, the expected form of behavior did not appear, given the stimulus, or the same form of behavior could appear, given different stimuli. The apparent spontaneity of behavior means that often behavior would occur in the absence of an eliciting stimulus. If the S–R model adequately explained behavior, behavior should be neither variable nor spontaneous.

The second problem was that other sciences seemed to be making progress by postulating unobservables (e.g., physics with relativity theory and quantum mechanics). Was psychology handcuffing itself by restricting its concerns to publicly observable phenomena, as Watson seemed to be arguing it should do?

The third problem was that the S–R model did not easily accommodate any influence of "subjective" phenomena on verbal or nonverbal behavior.

Classical behaviorism had put forth primitive versions of the influence of certain phenomena that were not publicly observable — Watson thought lust was a tingling in the external genitals and thinking was subvocal speech — but many scholars questioned the adequacy of these peripheral accounts.

Consequently, many researchers in the early 1930s began to abandon classical S–R behaviorism in favor of a new form of behaviorism. The new form of behaviorism is here called mediational S–O–R neobehaviorism. In this new form, internal, unobservable "organismic" variables (O) were inserted between publicly observable stimulus (S) and response (R) variables to mediate the relation between stimulus and response. Psychological knowledge was held to consist primarily of statements about such matters as the operating characteristics of the mediating variables. The use of mediating variables allowed this new form of behaviorism to address the three problems that had previously been noted with classical S–R behaviorism.

An early example of this mediational approach was Woodworth (1929), who explicitly proposed an S–O–R formulation. For Woodworth, the "O" variables accommodated a wide variety of "organic states," motives, response tendencies, and purposes, which were presumed to mediate the effects of environmental stimuli. Other theorists of the time followed with their own renditions of this model, invoking moods, attitudes, and "sets," meaning predispositions. The historical record indicates that the grand learning theorists of 1930s, 1940s, and even 1950s contributed an ever-expanding set of mediating variables, not necessarily related to Woodworth's original senses of "response tendencies" and "organic states." For example, the neobehaviorist and learning theorist E.C. Tolman introduced such organismic variables as expectancies and cognitive maps, which were couched in the language of cognition (see Smith, 1986, p. 116 ff.). C.L. Hull (1943) introduced habit strength and reaction potential in his system, and his mediating variables tended to have a slightly physiological flavor, although unsubstantiated by any direct observation of physiology. O.H. Mowrer (1947) talked in terms of the "diffuse emotional responses" of fear, relief, disappointment, and hope. Most of these new approaches to psychological theorizing were regarded as forms of behaviorism, in that they drew their strength, either implicitly or explicitly, from the study of S–R relations as opposed to, say, introspective comments about consciousness. Nevertheless, they are properly regarded as examples of mediational neobehaviorism because they also included mediating variables that the advocates sought to relate to publicly observable data, at the stimulus end, the response end, or both ends. In any case, mediational neobehaviorism has proved exceptionally popular and influential, and to a large extent, the history of psychology since the advent of mediational neobehaviorism in the 1930s is the history of various sets of unobservable, mediating, organismic variables that theorists have proposed. One theorist

might emphasize variables associated with physiology, either directly or metaphorically, another theorist might emphasize those associated with perception, either directly or metaphorically, and yet another theorist might emphasize those associated with emotion, either directly or metaphorically, but in any case, the dominant underlying theme is one of mediation by unobservable acts, states, mechanisms, processes, or entities.

Logical Positivism, Operationism, and Radical vs Methodological Behaviorism

An important concern of the mediational neobehaviorists in the 1930s was how to remain scientifically respectable while proposing these organismic variables. How could one be sure that one was not just making something up that was unscientific, particularly when invoking "mental states" as mediating organismic variables? Did the proposed term refer to something that actually existed? How could theorists agree on a meaning for the term? Although the entire story is quite complicated, suffice it to say that during the 1930s, at the same time that mediational neobehaviorism was developing, philosophers as well as research scientists in both the natural and social sciences were seeking to work through problems associated with determining the meaning of scientific concepts.

The Rise of Logical Positivism

The various groups developed somewhat similar positions, and although their concerns were overlapping, for the most part the groups did not interact or cooperate as they worked through those concerns (Smith, 1986). The position that developed in philosophy is called logical positivism. The logical positivists were a group of scientifically minded philosophers and philosophically minded scientists who began to critically discuss the nature and meaning of scientific concepts, partly in response to what was happening in physics at the time. For example, physics was making astounding progress by talking about relations and principles affecting very small (quantum mechanics) or very large (relativity theory) matters. These relations and principles were well beyond anything that had been directly observed. How could the fundamentally empirical and observational character of science be preserved in light of the obvious advances taking place in physics?

Logical positivism answered such questions by adopting a verificationist theory of meaning. According to this theory, meaningful scientific statements or statements of cognitive significance were those that could be verified, by being reduced to the bedrock language of physics. Indeed, the logical positivists proposed that all sciences could be unified by reducing their

propositions to the language of physics. Symbolic logic was then used to express higher-order relations, which were codified in theories. Importantly, the vocabulary of science included (a) observational terms, which referred to observable events, things, and properties and were measured in units of physics; and (b) theoretical terms, which did not refer to observables but were linked via logic to publicly observable readings on counters, dials, meters, and pointers. The various higher-order concepts in physics that appealed to unobservables were therefore to be treated as theoretical terms. Hypothetico–deductive methods became the standard for doing science. Explanations were to be accommodated as logically valid conclusions in a deductive argument with a covering law as one premise and a statement of antecedent conditions as another. In sum, the logical positivists held they had provided a rational reconstruction of scientific epistemology that more or less resolved the critical questions, and all that remained was to mop up a few details. Although the logical positivists sought heroically to distance themselves from metaphysics through an appeal to verificationism and symbolic logic, some have subsequently come to believe that their epistemology nevertheless embraced a kind of metaphysics, if only implicitly.

The Rise of Operationism

Similar events were taking place in the sciences, but again these events were not necessarily identical with or even a subset of those in philosophy. For example, in 1927 the physicist P.W. Bridgman proposed a principle he called "operationism." According to this principle, the meaning of a scientific concept was synonymous with the corresponding set of operations (e.g., by which it is measured). Thus, the meaning of the term "length" was determined by the operation of measuring the distance in question. By adhering to operationism and defining a concept in terms of operations, scientists were therefore able to generate agreement about the concept.

The logical positivists knew about Bridgman's principle of operationism, and they welcomed it as compatible with their own views. The compatibility between logical positivism and operationism was that the operation that rendered a term scientifically meaningful was publicly observable, even though the operationists were not concerned with formal logic in the same way that the logical positivists were. Operationism became highly influential in the social sciences, and especially so for the mediational neobehaviorists in psychology. As the scene played out in psychology, stimuli and responses came to be regarded as observational terms, which were directly and immediately observable to anyone. The unobserved, mediating, organismic terms were regarded as "theoretical terms" in psychological theories and explanations, in that they were inferred, logical constructions. Although such theoretical ele-

ments were not directly observable through the use of any known scientific instrument, they were permissible to the extent that scientists could "operationally define" them by specifying a set of operations according to which they were measured. Hence, operationism became the linchpin of the new mediational approach, providing the requisite rigor that was taken to validate the approach.

Smith (1986) and Zuriff (1985) have extensively described the nature of the relation between logical positivism and operationism, on the one hand, and behavioral psychology, on the other hand, during the 1930s and 1940s. Moore (2001b) has also reviewed related matters in the development of behavioral psychology. On the one hand, the logical positivists sought to explain psychology in physicalistic terms, and thought that behavioral psychology, with its emphasis on publicly observable variables, was on the right track. On the other hand, it was not so much that psychology was immune from the influence of logical positivism, but rather that important behaviorists during the 1930s and 1940s, such as Tolman, Hull, and the emerging Skinner, were psychologists, not philosophers. Consequently, the behaviorists were influenced in different ways by the logical positivist movement, but actually more by concerns with operationism than logical positivism per se. As mentioned earlier, the formal logic of the logical positivists played little role, if any, in the activity of the behaviorists.

Operational Definitions: Exhaustive or Open-ended?

One important matter remained: What was the existential status of these unobserved, theoretical terms that were linked to observations? The original conception of meaning in logical positivism was that theoretical terms were exhaustively defined with reference to observables. That is, the terms did not refer to anything that necessarily existed — they were simply logical devices that facilitated accurate predictions and explanations. They existed only within an equation or theory, and not actually in the world at large.

However, an approach requiring exhaustive definitions led to technical problems (see Zuriff, 1985). After a period of intense philosophical discussion, Carnap (1936, 1937) led the logical positivists in a clarification of their initial position, in which they determined that the terms did not have to be exhaustively defined with reference to observables. It was enough for the theoretical terms to be linked to observables through logic, which allowed them to have a wide range of referents. This move was a fairly substantial one, but was dictated by technical features of symbolic logic more than actual scientific practice. However, this move also meant that the terms could refer to something that was presumed to exist, outside of its role in an equation or theory.

Similarly, the original conception of meaning in operationism was that the meaning of a term or concept as defined by an operation was exhaustive. Thus, length as measured by a ruler was one concept, because it was measured in one way, and length as measured by triangulation was an entirely different concept, because it was measured in a different way. In the discipline of psychology, expressing the meaning of a concept by relating it to the measurement of some publicly observable behavior in some publicly observable experimental operation became a conventionally accepted practice. The term was synonymous with the corresponding operation, and nothing more was implied, especially about its existence.

According to Boring (1950, pp. 656–657), Herbert Feigl, one of the original members of the logical positivists, came to Harvard on a fellowship in 1930 and introduced the Harvard psychologists to logical positivism and operational thinking. As the Harvard psychologists became better acquainted with these ideas, they came to adopt the operational point of view. After all, Bridgman was from Harvard, and Harvard was one of the leading academic institutions in the country, if not the leading institution. Those associated with the Harvard Department thought it clearly needed to exercise its leadership by embracing the latest developments in rigorous scientific thinking. However, an interpretation of operationism that required the exhaustive definition of theoretical concepts raised a number of concerns among scientists as they sought to make systematic statements involving their concepts: How could their concepts be regarded as general, and applying to more than one situation? How could general theories even be developed? Were there some terms to which operationism did not apply? Did the requirement to operationally define scientific concepts unduly restrict scientific theorizing? Skinner's contemporary in the Harvard Department, S.S. Stevens, tried to defuse the debates that arose about some of these matters in four papers on operationism published in the mid and late 1930s (Stevens, 1935a, 1935b, 1936, 1939). E.G. Boring of the Harvard Department praised Stevens' work, particularly the last of the four papers (see Boring, 1950, pp. 657–659, 663). Nevertheless, questions remained about the generality of concepts and their existential status, and psychologists continued to deploy theoretical terms in a wide variety of ways.

Skinner on Logical Positivism and the Conventional Interpretation of Operationism

Skinner was a graduate student and post-doctoral fellow at the time that many of the above matters arose, and they were of considerable interest to Skinner, given his underlying interest in epistemology. For instance, Cuthbert Daniel, a colleague in Mathematics, introduced Skinner to Bridgman's ideas and operationism while Skinner was a graduate student in the

fall of 1929 (Coleman, 1996, p. 111; Skinner, 1979, p. 41). Skinner was also a charter subscriber to *Erkenntnis*, the journal published by the logical positivists (Skinner, 1979, p. 115). One influence of operational analyses on Skinner's work was reviewed in Part 2 (Moore, 2005b) of the present series, in conjunction with the discussion of Skinner's dissertation and the eventual distinction between respondent and operant forms of behavior. Skinner was strongly influenced not only by Bridgman, but also by Duhem, Mach, and Poincaré in his approach to operational thinking. Part 2 of this series noted that Skinner's approach to the problems of variability and spontaneity was not to add organismic variables. Rather, he proceeded by (a) invoking "third variables" which were still outside the organism (Skinner, 1978, p. 117; 1989, p. 109); (b) talking of classes of stimuli and responses (Skinner, 1935a); and (c) distinguishing operant from respondent behavior (Skinner, 1935b, 1937).

Skinner was initially optimistic about the early efforts of the operationists and logical positivists in scientific epistemology. For example, Skinner (1979) said that at that time, "As far as I was concerned, there were only minor differences between behaviorism, operationism, and logical positivism" (p. 161). Similarly, in a letter to his colleague and close personal friend Fred Keller during the 1930s, Skinner (1979) suggested that "[Carnap] is the only European I have ever met who grasps the significance of modern behavioristic psychology and its implications for the problem of thought" (p. 213). Skinner even applauded the papers that Stevens published in the 1930s (Skinner, 1979, p. 163).

Later, however, Skinner came to better understand what was actually transpiring under the auspices of operationism and logical positivism, and he took an entirely different stand. He bitterly repudiated what operationism and logical positivism had become in psychology, directly disparaging the "operationism of Boring and Stevens" (Skinner, 1945, p. 292). Eventually, he openly criticized Stevens by stating that (a) "S.S. Stevens has applied Bridgman's principle to psychology, not to decide whether subjective events exist, but to determine the extent to which we can deal with them scientifically" (Skinner, 1964, pp. 83–84); and (b) "it was Stevens . . . who then continued to believe in the existence of mental life" (Catania and Harnad, 1988, p. 217). Worthy of note is that Day (1969) pointed out that "notions of logical positivism . . . were more widely discussed among experimental psychologists than understood" (p. 490).

Skinner also took strong exception to the entire logical positivist view of scientific methodology, as evidenced in the "Are Theories of Learning Necessary?" paper (Skinner, 1950) and one of his own autobiographical statements, "A Case History in Scientific Method" (Skinner, 1956). Indeed, Skinner later starkly contrasted his own position with the logical positivist

program and with that of other experimental psychologists like Clark Hull who viewed themselves as behaviorists:

> I never faced a Problem which was more than the eternal problem of finding order. I never attacked a problem by constructing a Hypothesis. I never deduced theorems or submitted them to Experimental Check. So far as I can see, I had no preconceived Model of behavior certainly not a physiological or mentalistic one and, I believe, not a conceptual one . . . . Of course, I was working on a basic Assumption that there was order in behavior if I could only discover it but such an assumption is not to be confused with the hypotheses of deductive theory. It is also true that I exercised a certain Selection of Facts but not because of relevance to theory but because one fact was more orderly than another. If I engaged in Experimental Design at all, it was simply to complete or extend some evidence of order already observed. (Skinner, 1972, p. 112)

> The behavior of the scientist is often reconstructed by scientific methodologists within a logical framework of hypothesis, deduction, and the testing of theorems, but the reconstruction seldom represents the behavior of the scientist at work. (Skinner, 1974, p. 343)

True to his own empirical approach, and showing the influence of Mach, Skinner formulated scientific activity at a descriptively consistent level. Showing the influence of Bertrand Russell, Skinner also formulated scientific activity in terms of the factors that actually caused scientists to work in particular ways, on the basis of observing the work of the scientist he knew best — himself.

The Rise of Methodological Behaviorism

What was actually transpiring under the auspices of operationism and logical positivism was methodological behaviorism. For example, the implication of the operationism of Boring and Stevens, which Moore (2001a, 2001b) argues has unfortunately continued to dominate traditional psychology, is that despite the original sense of exhaustive definitions of theoretical terms, mental processes cause behavior. However, the implication continues, psychology as a science cannot deal directly with the mental processes because they are not publicly observable. Instead, psychology must confine itself to events that are accessible to at least two observers. Hence, psychology can only describe the mathematical relation between publicly observable independent and dependent variables and do nothing else. This stance is the very essence of methodological behaviorism.

Research in sensation and perception provides a convenient illustration. A time-honored term in such research is "sensation." As conventionally used, the term referred to a mental, subjective phenomenon that determined some subsequent behavior, for example, a verbal report. Thus, the traditional view held that when subjects reported red, they were reporting not a property of a

stimulus, but rather a property of their response to the stimulus. To understand a subject's behavior, one had to know the strength of the sensation. However, the sensation was mental and unobservable. It could not be part of science. Fortunately, according to the traditional view, the S–O–R model of mediational neobehaviorism provided an alternative. There was S — the stimulus, O — the mediating organismic variable of the sensation, and R — the verbal response. The task of the scientist was to mathematically express the relation between publicly observable elements and the subjective sensation by providing a quantitative description of how parametric variations in the physical property of the stimulus affected probability of particular forms of verbalizations about the stimulus. One could then infer what properties the mediating mental sensation must have. In so doing, one had successfully circumvented the problem created by directly including any unauthorized elements like the subjective sensation in a scientific statement.

If one was concerned with a nonverbal response, the logic was essentially the same. A discrimination procedure involving some other response, such as operating a manipulandum, would be used in place of a verbal report. Some other property of the response — its latency, or magnitude, or probability on a series of test trials — would be used as the basis for inferring the strength of the sensation, with perhaps converging evidence from some property of the stimulus.

Again, the perspective that underlies this whole approach is noteworthy. Nothing was said about how such a verbal response developed. The perspective presumes that (a) there is a mind in a mental dimension; (b) the mind observes the strength of the sensation; and (c) having observed the strength of the sensation, the mind causes the individual to respond veridically in verbal or nonverbal ways. The experimental protocol is designed to reveal these presumed features of the situation in a way that is conventionally regarded as respectable, and then to allow researchers to restrict their language to publicly observable relations, thereby circumventing problems created by directly talking about variables that are not publicly observable. To anticipate an argument that would be made somewhat later, it is as if subjects had a "private language" that enabled them to describe their own sensations. The perspective is mentalism, and the traditional interpretation of operationism lies at the heart of institutionalizing mentalism, through what appears to be a valid scientific method. A passage from Kimble (1985) outlines the mainstream position as it then existed:

> Even in Watson's day there were those, most notably Tolman, who attempted to bring mentalistic-sounding concepts back into psychology by means of what amounted to operational definitions. In a general way, the operational point of view did nothing more than insist that terms designating unobservables be defined in ways that relate them to observables. From there it proceeded to a further insistence that concepts defined in this way must have a relationship to behavior. In this way these concepts

> became intervening variables, ones that stand between observable antecedent conditions on the one hand and behavior on the other. The diagram below serves to summarize this point:
>
> Antecedent Conditions—Mentalistic Concepts—Behavior
> Independent Variables—Intervening Variables—Dependent Variables
>
> Obviously, there is nothing in this formula to exclude mentalistic concepts. In fact, the whole point of it is to admit unobservables. (p. 316)

In any event, discussions of operationism and its role in psychological theorizing continued into the 1940s, as philosophers of science (and others) remained concerned about scientific methods purportedly dealing with subjective entities, the fundamental character of verbal behavior, the distinction between observational and theoretical terms, the existential status of theoretical terms, and how theoretical terms such as mediating organismic variables were to be defined.

The 1945 Symposium on Operationism in *Psychological Review*

Skinner jumped full force into this discussion in 1945, with his contribution to a Symposium on Operationism organized by E.G. Boring of Harvard. The impetus for the Symposium was an article by Israel and Goldstein (1944) that criticized psychologists for being inconsistent when operationally defining their concepts (see also Moore, 2001b). Israel and Goldstein framed the issue in terms of whether the operational definition of a scientific concept (or theoretical term) referred to the operations from the point of view of the experimenter — from the independent variable side, or of the subject — from the dependent variable side. The implication was that if the definition referred to operations from the point of view of the experimenter, the definition was exhaustive, and the term did not refer to anything that actually existed. In contrast, if it referred to operations from the point of view of the subject, the definition was open-ended, and the term could be referring to something that actually existed.

Theoretical Concepts in Psychology: Revisiting Exhaustive vs Open-Ended Definitions

In retrospect, we can see that psychologists were grappling with the same question with which such logical positivists as Carnap (1936, 1937) had grappled in the mid-1930s: Were the definitions of theoretical concepts to be exhaustive or open-ended? Did the entities that were referred to actually exist or not? Those familiar with this story as it played out in psychology recognize that eventually MacCorquodale and Meehl (1948) resolved this question by

proposing a distinction between (a) the intervening variable interpretation of theoretical terms, in which the definitions were exhaustive; and (b) the hypothetical construct interpretation, in which the definitions were open-ended. MacCorquodale and Meehl explicated the distinction using the phrase "surplus meaning." Intervening variables did not admit surplus meaning; their definitions were exhaustive, and the theoretical terms were not assumed to refer to anything that existed outside of the equation or theory. In contrast, hypothetical constructs did admit surplus meaning; their definitions were therefore open-ended, and the theoretical terms were assumed to refer to things that existed in the world at large. Whereas Carnap had explicitly favored an open-ended approach, MacCorquodale and Meehl suggested scientists could use either approach, as long as they were consistent. In practice, most theoretical psychologists came to favor the hypothetical construct interpretation, primarily because of the greater latitude in theory construction it afforded.

In any case, Israel and Goldstein (1944) reinvigorated a debate in psychology that had actually been going on since at least Stevens' articles from the 1930s. Boring (1945, 1950, p. 663) developed the Symposium by inviting several prominent individuals from the scientific and philosophical community to respond to certain rhetorical questions he had composed. These individuals were P.W. Bridgman of Harvard, H. Feigl of Minnesota, H. Israel of Smith, C.C. Pratt of Rutgers, and B.F. Skinner of Minnesota. Boring also made his own contribution. Unfortunately, Stevens and Tolman were unable to participate. Boring's overall aim was to clear the air and see if some sort of consensus could be reached among researchers, theorists, and philosophers. Their contributions were published in 1945. If readers examine the issue of *Psychological Review* that has the complete Symposium in it, they will see occasional references to these matters and the Israel and Goldstein article.

Skinner's Contribution

In his contribution Skinner used the term "radical behaviorism" in print for the first time, distinguishing radical from methodological behaviorism. To be sure, Skinner had been thinking for many years about the epistemological issues raised by behaviorism. Readers may recall that during his time as a Junior Fellow, Skinner titled one of his important projects "A Sketch for an Epistemology." As its title suggests, this project (notes for it go back to 1932) consisted of a behavioristic treatment of a wide variety of epistemological issues. Skinner sought to address these issues in the Machian tradition, from general to specific, from what knowledge means to how scientists become

knowledgeable (see Skinner, 1979, pp. 116–119). Then, showing the influence of Russell, Skinner sought to outline the functional role of verbal processes in the behavior of the scientist. In addition, Skinner had been interested in a general treatment of verbal behavior for many years, perhaps ever since his Dark Year when he read Russell (1926), who had explicitly endorsed the causal analysis of verbal behavior. Also relevant is the apocryphal conversation with Whitehead in March 1934 (Skinner, 1957, pp. 456–460). In 1944 Skinner had secured a Guggenheim Fellowship to work on a book about verbal behavior. His efforts during 1944 paved the way for the William James Lectures, which he delivered at Harvard in the fall, 1947, and eventually his book *Verbal Behavior* (Skinner, 1957). Skinner's contribution to the Symposium grew out of all these sources.

Schneider and Morris (1987) pointed out that the term radical behaviorism had occasionally been used before (e.g., Calkins, 1921), but never as incisively as Skinner was then using it. By 1945 Skinner had moved beyond many of the traditional concerns about the definition and existential status of theoretical terms, and how to formulate the contribution of so-called "subjective" entities to either verbal or nonverbal behavior. Consequently, Skinner would have none of the ongoing debate over operationism. For him the whole issue could be resolved by critically examining the fundamental character of language. If readers examine the beginning of Skinner's (1945) article, they can see that he is laying out the case for a particular, causal view of verbal behavior. As Skinner saw it, the whole debate in traditional psychology, featuring observational vs theoretical terms and exhaustive vs open-ended definitions, was totally wrong and beside the point. Indeed, Skinner saw Boring's rhetorical questions that underlay the whole Symposium as fundamentally misguided. The debate was predicated on a logical, symbolic, reference view of language, which was a comprehensively mentalistic position that was itself in error:

> Attempts to derive a symbolic function from the principle of conditioning . . . have been characterized by a very superficial analysis . . . . Modern logic, as a formalization of "real" languages, retains and extends this dualistic theory of meaning and can scarcely be appealed to by the psychologist who recognizes his own responsibility in giving an account of verbal behavior. (Skinner, 1945, pp. 270–271)

One needed to discard entirely the referential view of verbal behavior, in favor of viewing verbal behavior as an operant phenomenon. One then analyzed verbal behavior in terms of its determiners, not on the basis of its structure. There was no more reason to ask what a term referred to or whether its definition should be exhaustive or open-ended than to ask what a rat's lever press referred to or whether its definition should be exhaustive or open-ended:

> A considerable advantage is gained from dealing with terms, concepts, constructs, and so on, quite frankly in the form in which they are observed — namely, as verbal responses. There is then no danger of including in the concept that aspect or part of nature which it singles out . . . . Meanings, contents, and references are to be found among the determiners, not among the properties, of response. (Skinner, 1945, p. 271)

Once language was understood as a behavioral phenomenon, other aspects of human behavior can be similarly understood, and there is no need to retreat to mentalism and what would come to be called a "private language."

Of course, Skinner's whole approach — a "thoroughgoing" behaviorism (see Michael, 1985) — was so revolutionary that no one else in the Symposium had the faintest idea what he was talking about. They were all wedded to the traditional, mentalistic, referential view of verbal behavior. As a result, they simply went on and on about traditional issues to which Skinner did not relate at all. In Boring's (1945, p. 278) own comments, he said that Skinner, full of his unfinished book, scared him and that Skinner was probably implying something that he (Boring) missed. How right Boring was, on all counts! Boring (1950) later quipped that Skinner "was certainly a practising operationist all along even when not a participant in a common concern" (p. 657). However, Boring failed to appreciate the kind of operationism Skinner was practicing.

Operational Analysis, Subjective Terms, and Methodological Behaviorism

The present paper earlier suggested that one of the problems associated with classical S–R behaviorism was that it could not convincingly account for the use of subjective terms. In his contribution to the Symposium, Skinner presented an account of how an operant behavioral approach could do so. He suggested radical behaviorism does not assume that language purportedly identifying "subjective" entities in another dimension necessarily and literally does so, such that they must be dealt with a different way by a science of behavior, if at all. Such an assumption is at the heart of methodological behaviorism, which argues that the way to deal with unobservable subjective entities is to formulate them in terms of publicly observable data. Skinner argued that clearly, this position would not do.

Rather, Skinner argued the general point that verbal behavior was the result of an operant behavioral process, rather than a mental, logical, referential, or symbolic process that is somehow independent of behavior. Skinner then went on to emphasize the specific point that some verbal behavior ostensibly about subjective entities might not actually refer to valid topics for scientific analysis. It might identify only "explanatory fictions," whose source was in linguistic traps, grammatical habits, unfortunate metaphorical exten-

sions, dualistic cultural traditions, and so on. Indeed, to even accept the traditional distinction between objective and subjective dimensions created a problem that could not be overcome — it was only legislated out of immediate concern.

Thus, to accept the traditional distinction between objective vs subjective was to accept two highly troublesome implications. The first implication was that the entities in the subjective dimension had to be ignored, or at best not be spoken of directly, because they are from a different dimension. The entities are not publicly observable, and thus science must remain silent about them. Skinner (1953) was to comment just a few years later on this implication in the following way

> Modern science has attempted to put forth an ordered and integrated conception of nature. Some of its most distinguished men have concerned themselves with the broad implications of science with respect to the structure of the universe. The picture which emerges is almost always dualistic. The scientist humbly admits that he is describing only half the universe, and he defers to another world — a world of mind or consciousness — for which another mode of inquiry is assumed to be required. Such a point of view is by no means inevitable, but it is part of the cultural heritage from which science has emerged. It obviously stands in the way of a unified account of nature. (p. 258)

Ironically, the sense of radical in radical behaviorism is sometimes taken to mean an "extreme" behaviorism, wherein talk of anything that is not publicly observable is categorically rejected, not because the phenomena in question do not "exist" in some sense, but rather because the talk is not scientifically respectable (see Schneider and Morris, 1987). No doubt this sense can be linked to some ultra-positivistic views of science, and perhaps even Watson's early view, in the first half of the twentieth century. Clearly, this sense is exactly wrong when it comes to Skinner's sense of radical behaviorism.

The second troublesome implication was that if psychologists wanted to speak of the so-called subjective entities, they could only do so indirectly or inferentially, through the publicly observable measures that were taken to be their surrogates and in terms of which they might be measured. Either of these two implications insulated what were called subjective entities from appropriate analysis, and preserved inappropriate conceptions of them unharmed. Skinner (1945) commented extensively on this matter:

> The operational attitude, in spite of its shortcomings, is a good thing in any science but especially in psychology because of the presence there of a vast vocabulary of ancient and non-scientific origin . . . . Psychology, alone among the biological and social sciences, passed through a revolution comparable in many respects with that which was taking place at the same time in physics. This was, of course, behaviorism. The first step, like that in physics, was a re-examination of the observational bases of

> certain important concepts. But by the time Bridgman's book was published, most of the early behaviorists, as well as those of us just coming along who claimed some systematic continuity, had begun to see that psychology actually did not require the redefinition of subjective concepts. The reinterpretation of an established set of explanatory fictions was not the way to secure the tools then needed for a scientific description of behavior. Historical prestige was beside the point. There was no more reason to make a permanent place for "consciousness," "will," "feeling," and so on, than for "phlogiston" or "vis anima." On the contrary, redefined concepts proved to be awkward and inappropriate, and Watsonianism was, in fact, practically wrecked in the attempt to make them work.
>
> Thus, it came about that while behaviorists might have applied Bridgman's principle to representative terms from a mentalistic psychology (and were most competent to do so), they had lost all interest in the matter. They might as well have spent their time in showing what an eighteenth century chemist was talking about when he said that Metallic Substances consisted of a vitrifiable earth united with phlogiston. There was no doubt that such a statement could be analyzed operationally or translated into modern terms, or that subjective terms could be operationally defined. But such matters were of historical interest only. What was wanted was a fresh set of concepts derived from a direct analysis of the newly emphasized data, and this was enough to absorb all the available energies of the behaviorists . . . . What happened instead was the operationism of Boring and Stevens . . . . It was an attempt to acknowledge some of the more powerful claims of behaviorism (which could no longer be denied) but at the same time to preserve the old explanatory fictions unharmed. The strategy adopted is more apparent in Boring's present paper than in Stevens' earlier publications. A concession is made in accepting the claim that the data of psychology must be behavioral rather than mental if psychology is to be a member of the United Sciences, but the position taken is merely that of "methodological" behaviorism. According to this doctrine the world is divided into public and private events, and psychology, or order to meet the requirements of a science, must confine itself to the former. This was never good behaviorism, but it was an easy position to expound and defend and was often resorted to by the behaviorists themselves. It is least objectionable to the subjectivist because it permits him to retain "experience" for purposes of self-enjoyment and "nonphysicalistic" self-knowledge . . . . The position is not genuinely operational because it shows an unwillingness to abandon fictions . . . . What is lacking is the bold and exciting behavioristic hypothesis that what one observes and talks about it is always the "real" or "physical" world (or at least the "one" world) and that "experience" is a derived construct to be understood only through an analysis of verbal (not, of course, merely vocal) processes. (pp. 271, 292–293)

In short, methodological behaviorism was the outcome in psychology of applying (a) the logical positivist model of science, and (b) the traditional conception of operationism based on referential views of verbal behavior. Methodological behaviorism gives the superficial appearance that scientists agree on their subject matter and methods, which many take to be the hallmark of science, but Skinner argued that even if agreement was gained, science did not necessarily advance in the long run. For Skinner, what was of concern was whether scientists were improving their control over nature, and this concern seemed to be secondary in methodological behaviorism to promoting agreement. Again, Skinner (1945) commented:

> Disagreements can often be cleared up by asking for definitions, and operational definitions are especially helpful, but operationism is not primarily concerned with com-

> munication or disputation. It is one of the most hopeful of principles precisely because it is not . . . . The ultimate criterion for the goodness of a concept is not whether two people are brought into agreement but whether the scientist who uses the concept can operate successfully upon his material all by himself if need be. What matters to Robinson Crusoe is not whether he is agreeing with himself but whether he is getting anywhere with his control over nature. (p. 293)

On the positive side, Skinner's contribution to the Symposium advocated the operational analysis of scientific verbal behavior, including verbal behavior purportedly about what were called subjective entities. However, his usage of "operational analysis" in the article differed significantly from the conventional. As had Russell (1926), Skinner called for a causal analysis of verbal behavior, and viewed an operational analysis as nothing less than a causal analysis in terms of operant contingencies:

> What we want to know in the case of many traditional psychological terms is, first, the specific stimulating conditions under which they are emitted (this corresponds to "finding the referents"), and, second (and this is a much more important systematic question), why each response is controlled by its corresponding condition . . . . We may generalize the conditions responsible for the standard "semantic" relation between a verbal response and a particular stimulus without going into reinforcement theory in details. There are three important terms: a stimulus, a response, and a reinforcement supplied by the verbal community . . . . The significant interrelations between these terms may be expressed by saying that the community reinforces the response only when it is emitted in the presence of the stimulus. (Skinner, 1945, pp. 271–272)

In science, one of the discriminative conditions that gives rise to a scientific concept is the data from an experimental operation, and Skinner linked the development of his ideas about this sort of scientific verbal behavior to Bridgman, Duhem, Mach, Poincaré, and Russell. Nevertheless, the verbal behavior in question was still the result of an operant behavioral process, not a mental process of symbolic representation and reference. Skinner's argument was that scientific verbal behavior needs to be examined on a case-by-case basis, to determine the conditions of which it is a function.

At the end of his contribution, Skinner (1945) posed his own rhetorical questions, one of which reads as follows:

> How can we account for the behavior of talking about mental events? The solution must be psychological, rather than logical, and I have tried to suggest one approach in my present paper. The complete lack of interest in this problem among current psychological operationists is nicely demonstrated by the fact that the only other members of the present panel who seem to be interested in a causal analysis of verbal behavior are the two non-psychologists (and one of them a logician!). [p. 294, italics in original]

In other words, Skinner called for the causal analysis of the term said to identify a subjective entity, but as an instance of verbal behavior controlled

by operant reinforcement contingencies. An operational analysis would reveal that some of the terms were nothing more than simple explanatory fictions or mischievous metaphors, controlled more by social than scientific variables. But there were quite obviously other instances in which individuals respond on the basis of private events, that is, on the basis of events not accessible to others. For instance, Skinner emphasized that

> There is, of course, no question of whether responses to private stimuli are possible. They occur commonly enough and must be accounted for. But why do they occur, and what, if any, are their distinguishing characteristics? (p. 273)

How are we to account for such responses, as matters of epistemology, and what are the implications of this approach for the broader question of what it means to say that anyone knows anything? From there, Skinner noted we can apply the operant model to more general questions: "The additional hypothesis follows quite naturally that being conscious, as a form of reacting to one's own behavior, is a social product" (p. 277). The alternative was to accept the Cartesian doctrine that internal events were better known than external, but this stance was the essence of dualism. It only preserved the old explanatory fictions unharmed, instead of dispensing with them. As Skinner (1974) later put it,

> As a philosophy of a science of behavior, behaviorism calls for probably the most drastic change ever proposed in our way of thinking about man. It is almost literally a matter of turning the explanation of behavior inside out . . . . I contend that behavioral science has not made a greater contribution just because it is not very behavioristic. (pp. 256–257)

In sum, much of the stimulus control over Skinner's composition of the 1945 article came from the work he had been doing on epistemology and verbal behavior since his graduate student years. However, he had been occupied with a variety of other matters, such as finding a permanent academic position, establishing himself professionally, writing *The Behavior of Organisms* (Skinner, 1938), parenthood, Project Pigeon during World War II (see Skinner, 1972, pp. 574–591), and relocating from Minnesota to Indiana at the end of the War. For the 1945 Symposium he took bits and pieces of the work he had done, in various stages of completion and stitched them together — if he had had a word processor, we would say it was a "cut and paste" job. As a literary piece, the paper is somewhat disjointed in places but its message is clear.

Verbal Behavior Under the Control of Private Stimulation

The Environment: Public and Private

A central feature of the epistemological position that Skinner put forth in the 1945 paper was that not all elements of contingencies responsible for the behavior called knowledgeable are publicly observable, although they are still in the behavioral dimension. As Skinner (1953) later put it,

> When we say that behavior is a function of the environment, the term "environment" presumably means any event in the universe affecting the organism. But part of the universe is enclosed within the organism's own skin. Some independent variables may, therefore, be related to behavior in a unique way. The individual's response to an inflamed tooth, for example, is unlike the response which anyone else can make to that particular tooth, since no one else can make the same kind of contact with it. Events which take place during emotional excitement or in states of deprivation are often uniquely accessible for the same reason; in this sense our joys, sorrows, loves, and hates are peculiarly our own. With respect to each individual, in other words, a small part of the universe is private.
>
> We need not suppose that events which take place within an organism's skin have special properties for that reason. A private event may be distinguished by its limited accessibility but not, so far as we know, by any special structure or nature. (pp. 257–258)

Thus, the validity of the operant approach to verbal behavior was a function of how well the approach could account in behavioral rather than mental terms for (a) the use of what were called subjective or mental psychological attributes, as in how speakers came to describe their feelings or sensations; and (b) the influence of "thinking," as in how individuals came to be influenced by their covert as opposed to overt behavior. In other words, verbal behavior, including verbal behavior under the control of private forms of stimulation, develops by virtue of the differential reinforcement supplied by the verbal community. How can speakers come to meaningfully use terms under the control of private forms of stimulation, given that language acquisition is a public process? Differential reinforcement from verbal community is necessary in the process, but the verbal community has no indication of when to reinforce a verbal response emitted in the presence of the appropriate discriminative stimulus because it has no access to that discriminative stimulus. As Skinner (1974) was to later put it, "The question, then, is this: What is inside the skin, and how do we know about it? The answer is, I believe, the heart of radical behaviorism" (p. 218). In addressing this matter, Skinner arrived at an early but nevertheless comprehensive answer to the "private language" problem in philosophy.

The Transfer of Discriminative Control from Public to Private Stimulation

Skinner's argument, expressed first in 1945 and subsequently in at least four other places (e.g., Skinner, 1953, 1957, 1964, 1974), is that discriminative control of the behavior in question develops on the basis of public stimulation, but then transfers to private stimulation. For example, the verbal community teaches us to label a pain caused by a sharp object as a sharp pain, and by a dull object as a dull pain. Subsequently, a pain of a similar nature occasions the descriptive use of the term we have earlier been taught to use, on the basis of its relation to public circumstances. Excruciating pains are those associated with crucifixion. Burning pains are those caused by hot objects, and so on.

In addition, some usages of the term "thinking" are occasioned by private activity that started out in an overt form, but then because of environmental circumstances receded to a covert form. The behavior exerts a discriminative effect in a particular situation because of generalization or coinciding properties of overt and covert form (see Moore, 2001a, for a review). Interestingly, a related form of this argument appeared in Russell (1927), a source that Skinner identified as seminal in his own thinking. For instance, Russell discussed images in the following passage:

> a visual image is an occurrence having the visual quality but not due to a stimulus to the eye, i.e., not having as a direct causal antecedent the incidence of light waves upon the retina . . . . We might therefore conclude that an image is an occurrence having the quality associated with stimulation by some sense-organ, but not due to such stimulation. In human beings, images seem to depend on past experience . . . . The sight of an object may bring with it a visual image of some other object frequently associated with it . . . . [A]n "image" is an occurrence recognizably visual (or auditory, as the case may be), but not caused by a stimulus which is of the nature of light (or sound etc, as the case may be), or at any rate only indirectly so caused as a result of association. With this definition, I do not myself feel any doubt as to the existence of images. (1927, pp. 183–185)

With Skinner's argument that even the processes by which individuals come to know themselves are behavioral and are linked to the public, social environment, matters fall into place, and radical behaviorism has demonstrated that it can account for the full range of epistemological phenomena in terms of behavioral processes. Questions of what is called consciousness, or introspection, or a variety of other "subjective" terms may be understood in a new light, as questions about behavioral rather than mental processes. Indeed, Natsoulas (1978) commented a number of years ago about

> the most detailed account of consciousness that psychology is currently in a position to offer. It should not come as a surprise, although I think it will to some, that this account has been put forward over many years by B.F. Skinner and other radical behaviorists. (p. 139)

Summary and Conclusions: When Did Skinner Become a Radical Behaviorist?

As we have seen, radical behaviorism is the unique epistemological position arising from the perspective of B.F. Skinner. It evolved during the second quarter of the twentieth century, as Skinner pursued his interests in the empirical study of behavior. Skinner came to psychology with a background in literature and the arts, although with an objective, empirical outlook. Before entering graduate school, he was influenced by Bacon, Pavlov, Russell, and Watson. After entering graduate school, he was very strongly influenced by Bridgman, Crozier, Loeb, Mach, and Poincaré. As Skinner worked on his own research program, both in graduate school and later, he applied his objective, empirical world view to analyses of not only the behavior of his subjects but also his own behavior as a scientist, including his verbal behavior. Radical behaviorism is at heart a philosophy of mind, and hence an epistemological position, derived from the unfolding of his view of verbal behavior (Malone, Armento, and Epps, 2003).

At first blush, it may seem reasonable at this point to pose our own rhetorical question concerning the development of radical behaviorism: "When did Skinner become a radical behaviorist?" However, we suggest such a question needs to be dealt with cautiously, for fear that it would bias an understanding of the dynamics of intellectual development, Skinner's included. For example, this question may be taken to imply radical behaviorism was a discrete, pre-existing static entity, and that one day Skinner was not a radical behaviorist, because he had not yet adopted any of its precepts, but that the next day he was, because he had. This conception does not seem apt, if only because a definition of just what radical behaviorism is may be hard to come by, without inadvertently lapsing into static, essentialist tendencies. Perhaps it is more reasonable to conceive of radical behaviorism as a progressive development in the work of Skinner and others. Given this conception, it appears reasonable to trace where Skinner was at certain points in his career, and then relate his stance at those points to where he eventually ended up.

We can say with some confidence that Skinner had objective, empirical inclinations in 1926, judging from writing that accompanied the death of his grandfather (see Part 1 of this series). We can say that he probably was not yet a behaviorist when he started graduate school in September 1928, although he had behaviorist leanings and wanted to identify himself as a behaviorist (Skinner, 1979, p. 299). We can say that he had become a behaviorist of one form or another by 1931, when he received his doctoral degree, but Skinner's intellectual development and indeed the evolution of his behaviorist thinking clearly continued after that time. The behaviorist he became clearly differed from the behaviorist he was in 1931.

We can note that a major step in Skinner's progression from behaviorist to the radical behaviorist he was to become was the publication of three papers in the mid-1930s. The first two were published in 1935. One was on the generic nature of stimuli and responses (Skinner, 1935a). The other distinguished between two kinds of reflexes and anticipated the operant–respondent distinction (Skinner, 1935b). The third paper, published in 1937, formalized the operant–respondent distinction (Skinner, 1937). We can also note that in the background of this work was the ongoing concern with verbal behavior. Readers will recall that Skinner considered the 1935 paper on the generic nature of stimuli and responses as part of his "Sketch for an Epistemology."

As evidenced by Skinner's use of the term drive and his postulating the reflex reserve in *The Behavior of Organisms*, we can say that he still had some distance to go in 1938. These hypothetical elements were vestiges of more traditional approaches, and although Skinner's use of them was not entirely consistent with the practices of traditional psychology, their presence suggests his epistemology, and hence his radical behaviorism, were still developing (see also Skinner, 1989, pp. 130–131, for his views of the "reflex reserve"). Indeed, Skinner (1978) acknowledged that

> It would have been the right time to abandon "reflex," but I was still strongly under the control of Sherrington, Magnus, and Pavlov, and I continued to hold to the term doggedly [sic] when I wrote *The Behavior of Organisms* (1938). It took me several years to break free of my own stimulus control in the field of operant behavior. From this point on, however, I was clearly no longer a stimulus–response psychologist. (pp. 119–120)

We have previously noted that Skinner first used the term radical behaviorism in print in his 1945 paper on operationism. Should we identify him as a radical behaviorist then? Clearly, he had progressed beyond the position he had held in 1938, but had radical behaviorism reached some reasonably complete approximation to its final stages? Again, the answer is uncertain. For example, an important feature of radical behaviorism is the thesis of selection by consequences. This thesis was not introduced until *Science and Human Behavior* (Skinner, 1953, pp. 90, 430). The thesis was subsequently applied to the evolution of cultures in *Beyond Freedom and Dignity* (Skinner, 1971) and received still further treatment in Skinner (1981; see also Catania and Harnad, 1988). In addition, Moxley (1998, p. 78) suggests that as late as 1956 Skinner seemed to be writing from a mechanistic position that resembled conventional mediational S–O–R neobehaviorism. We can say that radical behaviorism reached a major milestone in 1957, with the publication of *Verbal Behavior* (Skinner, 1957), which Skinner himself regards as his most important work (Skinner, 1978, p. 122). Nothing like the previous commitments to a reflex reserve, drive, or S–R mechanisms was in evidence here.

In short, the answer to the question of "When did Skinner become a radical behaviorist?" presumably depends on the time that what one considers to be the most salient characteristic of radical behaviorism first appears. For some, Skinner's position on private events is that characteristic. If so, their answer would be 1945. For others, selection by consequences is that characteristic. If so, their answer would be 1953. For still others, the comprehensive statement about verbal behavior would be that characteristic. If so, their answer would be 1957. And so on.

In any event, with specific regard to epistemology and radical behaviorism as a philosophy of mind, Skinner's ultimate argument is that even the processes by which individuals come to know themselves are behavioral and are initiated by verbal interactions with the social environment. This argument, first presented in 1945, allows a wide variety of matters to fall into place, and with it radical behaviorism has demonstrated that it can account for the full range of epistemological phenomena in terms of behavioral rather than mental processes. Questions of what is called consciousness, or introspection, or a variety of other "subjective" terms may be understood in a new light, as behavioral rather than mental. And in the final analysis, that is certainly a principal characteristic of a radical, thoroughgoing behaviorism.

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