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Some Historical and Conceptual Background to the Development of B.F. Skinner's "Radical Behaviorism" — Part 2

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The present article is the second 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 are Skinner's academic and research experiences between 1928, when he entered graduate school at Harvard, and the late 1930s, when he had assumed his first academic position. The article also examines the intellectual climate that emerged during the second quarter of the twentieth century, which is the context out of which radical behaviorism developed as a unique position. Overall, the views for which Skinner is recognized were significantly influenced by such figures as Francis Bacon, Percy Bridgman, William Crozier, Jacques Loeb, Ernst Mach, Ivan Pavlov, Bertrand Russell, and John B. Watson.

Keywords: B.F. Skinner, radical behaviorism, history of psychology

Part 1 of this three-part series on the development of radical behaviorism examined biographical details of Skinner's life prior to his entering graduate school in 1928 as well as the intellectual climate at the beginning of the second quarter of the twentieth century that led Skinner to become interested in psychology. The aim of the present article, Part 2 of the series, is to examine the influence of events after Skinner entered graduate school at Harvard in September, 1928, and extending into the later 1930s.

We previously noted that after Skinner received his undergraduate degree in 1926, he spent about a year trying to write "objectively" about the meaning of life and the human condition. However, he failed in his attempts at writing,

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and he became increasingly disaffected as the year wore on. He read independently during the year, and was significantly influenced by the commentary of progressive social critics as well as modernist trends of the time favoring science. In October, 1927 he decided to abandon his efforts at literature and directly pursue his interests in behavior, by studying psychology. Interestingly, he had virtually no exposure to psychology as a discipline while an undergraduate. As other authors have noted (e.g., Coleman, 1985a, p. 90), legitimate questions are just how much did he know of the discipline (a) when he chose psychology in October, 1927; (b) when he applied to Harvard in May, 1928; and (c) when he matriculated at Harvard in September, 1928. To be certain, in his own independent reading, he came in contact with the work of Bertrand Russell, who had applied objective and empirical principles of behavior to epistemology; John B. Watson, who had written persuasively about a genuine science of behavior; and Ivan Pavlov, who had actually pursued a science of behavior in the laboratory. Even though Skinner ambitiously tried to define himself as a "behaviorist" as early as January, 1928 (Skinner, 1976, p. 299), it is unclear that he should be regarded as a psychologist in September, 1928, let alone a behaviorist, given his unsystematic exposure to the discipline. Indeed, Skinner (1979) indicated that when he arrived at Harvard, he was not yet "a fully committed convert to psychology" (p. 37). It seems more reasonable to conclude that in September, 1928, Skinner had a direction he wanted to go, and a few names he associated with that direction, rather than a fully mature conception of what lay in that direction. We continue with Skinner's entry into graduate school.

Skinner at Harvard: Graduate Student Years

The Development of Skinner's Research Program

Skinner arrived in Cambridge ready to pursue his new undertaking at full throttle. Unfortunately, he found that behavior as subject matter was not particularly well represented in the Harvard Department. Coleman (1982, p. 62) has pointed out that Psychology was institutionally subordinated to Philosophy in a combined academic department, situated in a higher administrative unit called the Division of Philosophy. In addition, Psychology was presided over by E.G. Boring, a former student of E.B. Titchener. For Boring, psychology was the study of the content and structure of conscious experience as revealed through introspection. As a result, Boring was somewhat antagonistic toward the study of behavior per se. After a period of uncertainty, Skinner found an intellectual home in the Department of General Physiology, in the Division of Biology. Here behavioral research was taken seriously. Early on, Skinner became affiliated with W.J. Crozier, a disciple of Jacques Loeb. For

example, Catania (1992, p. 1522) links Skinner's use of the term *organism* to his exposure to Loeb and Crozier. Loeb had long advocated the study of the organism as a whole, and under Crozier Skinner began his research by also studying the behavior of the intact organism, rather than simply a surgically isolated reflex mechanism.

Although Skinner's path to the study of behavior as a subject matter in its own right would seem to follow from Darwin to Lloyd Morgan to Watson to Pavlov — and surely Skinner was influenced by these figures, Herrnstein (1972, pp. 45 ff.) suggested that insofar as one is concerned with actual research practices, Skinner's path followed from Loeb to Crozier. For example, Loeb emphasized the mechanistic response of the intact organism to fields of force. Smith (1986) indicates that for Loeb,

> the behavior of higher organisms was simply a sum of the stimulating forces in the environment. Furthermore, Loeb believed that these causal factors could adequately be expressed as variables in equations representing functional relations between environment and behavior . . . . Loeb insisted that equations of this sort should contain no "arbitrary constants" and that all variables in them must have testable reference to observable phenomena. (p. 276)

Crozier extended this mechanistic approach to the complex response systems of intact organisms, for example, as found in tropisms. The hypothesis that guided Skinner's research at Harvard was that "the behavior of an intact organism is an exact, if involved, function of the forces acting upon the organism" (e.g., Skinner, 1972, p. 449). This hypothesis follows directly in the tradition of Loeb's mechanistic approach, which was represented in Skinner's research work in Crozier's laboratory. Vargas (2004, p. 138) points out that the five references in Skinner's first research article, on geotropism in ants, were all from journals in physiology.

One of the courses that Skinner took early in his time at Harvard was General Physiology 5, taught by Hudson Hoagland, a recent PhD in Psychology from the Harvard Department. As Skinner (1979) put it, "It was exactly the course I was looking for" (p. 17; see also Skinner, 1978, p. 113). Not only did it discuss Pavlov, whom Skinner had read during the winter of 1928 while he was living in New York, but it also included the pioneering work of Rudolf Magnus (1924), a professor of pharmacology at Utrecht who had expanded the concept of the reflex to account for an organism's ability to maintain itself in an upright position and move about despite any countervailing forces that impinged upon it. Skinner was particularly influenced by Magnus's work because it dealt with physical movement, instead of the glandular secretions with which Pavlov had dealt. Skinner followed up the Hoagland course by studying the work of Sherrington (1906), in which behavior also occupied center stage.

As had Loeb and Crozier, Skinner came to approach behavior in terms of the traditional concept of the reflex. As Skinner (1979) later put it, "I was convinced that the concept of the reflex embraced the whole field of psychology" (p. 70). Skinner's choice of dependent measures in his research typically came from reflex physiology (Coleman, 1984, p. 486). For example, reflex physiologists focused on changes in responsiveness across time: fatigue, refraction. Skinner initially sought to apply these measures to the behavior of the intact organism, eventually studying the development of "reflex strength" as a function of various manipulations. Overall, Skinner took the position that the study of the reflex was important because the reflex was a subject matter worthy of investigation in its own right, rather than because it was simply an indicator of something else, such as the activity of the nervous system. Skinner further adopted the position that the reflex could be studied experimentally and quantitatively, if only one applied the right tools. Skinner didn't have the correct tools initially, but they would emerge when he put together his dissertation, which we will describe later in the present article.

Reviews of Skinner's research activities during this period are available in Coleman (1984, 1987, 1996), Iversen (1992), and Vargas (2004, pp. 138–141). Skinner worked continuously in the laboratory, studying responses in various settings and seeking to formulate data in a way that would validate his emerging perspective, namely, that behavior was a subject matter in its own right. His primary objective was to find quantitative orderliness in measured behavior (Coleman, 1987). Skinner had many different experimental projects, and built many pieces of apparatus, in keeping with his mechanical talents. Coleman (1987, p. 64) states that Skinner's research program was actually a bit more complicated than Skinner's own portrayals indicate, in that Skinner pursued multiple approaches to research problems, abandoning those that didn't reveal quantitative order and sticking with those that did. He studied a wide variety of behavior, from geotropism in ants to locomotor activity in intact organisms, such as rats and squirrels. His research was not simply idle exploration, for in it he developed the ideas for his dissertation.

Recognizing the complexity and overlapping nature of Skinner's research activities as a graduate student, and recognizing that there was not a straightforward progression from one research topic to the next, Coleman (1987) identified several lines of research in which Skinner engaged. One line concerned postural reflexes and locomotion. This line of research followed directly from Loeb and Magnus. An example was the "Parthenon." According to Coleman (1987), this piece of apparatus was

> a tunnel into one end of which the rat could be introduced from Skinner's silent release carrying box, and out the other end of which the rat would emerge and hesitantly start to descend a couple of steps until Skinner presented a calibrated click which (so he

> thought at the time) inhibited the reflexes of progression (Magnus, 1924) and elicited withdrawal back into the tunnel . . . . (p. 50)

Related studies including recording the force with which baby rats tried to move forward when their tails were firmly held, and studying the activity of squirrels and rats in running wheels.

A second line of research also concerned locomotion, and employed a suspended runway. As Coleman (1987, p. 52) described this research, Skinner would initially train the rat to run from a start to a goal box on a runway that was suspended on transverse, tightly drawn piano wires. After this training, Skinner would present a loud sound that elicited a sudden halt in the rat's locomotion along the runway, somewhat similar to the Parthenon research. The apparatus was sensitive to the locomotor thrusts of the running and halting. As in Skinner's other research, the purpose was to see if he could find quantitative order among various characteristics of the rat's responses of running, for example, the impact of each step the rat would take toward the end of the runway, and of habituation of the reflexive halting as a function of repeated presentations of the loud sound.

A third line of research was concerned with the strength of the rat's eating, or ingestion "reflex." In the early portion of this research, Skinner also used a runway and measured behavior, but for a different purpose than to study simple locomotion: the quantitative aspects of running in the runway provided data about which to draw conclusions regarding the strength of eating. One runway was a double runway, in which the rat could go from a start box to a goal box and then return down the second, parallel alley to the start box. This double runway approximated the continuous stream of locomotor activity that might be observed in a running wheel. A modified version of the double runway was called a "tilt box." Here the weight of the rat into different sections of the runway caused the apparatus to dispense a food pellet automatically into the food cup, thereby alleviating the experimenter from having to intervene in a trail-by-trial fashion with the conduct of a freely moving organism. Interestingly, Skinner (1979, p. 115) noted that when he used this apparatus, the rat did not immediately start to return up the parallel runway after being fed. Skinner began to record the durations of the delays and found that the delays changed in orderly ways. He was convinced he had found evidence for an underlying behavioral process that could be studied apart from the details of the act of running.

In this third line of research, the use of a runway to study the strength of the ingestion reflex gave way in 1930 to the development of a "problem box," or small chamber. In this preparation the rat that was deprived of food for twenty-four hours would press a panel to get access to food pellets behind the panel. The chamber was placed inside a larger, soundproofed box, to

eliminate outside disturbances. Skinner measured the amount of food eaten by the rat over a two hour observation period.

In one of his autobiographical statements, Skinner (1972) stated that

> I never faced a Problem which was more than the eternal problem of finding order . . . . Of course, I was working on a basic Assumption — that there was order in behavior if I could only discover it. (p. 112)

The order would come from controlling conditions, as Skinner had learned from Pavlov ("Russell and Watson had given me no glimpse of experimental method, but Pavlov had: control the environment and you will see order in behavior," Skinner, 1967, p. 399; "I had the clue from Pavlov: control your conditions and you will see order," Skinner, 1972, p. 104), and it would be revealed through quantitative analysis. As noted earlier, much of the inspiration for the changes and modifications to Skinner's research apparatus during his graduate school years came from the desire to find order and express it quantitatively. To Skinner, one source of variability was the discrete-trial procedure, wherein the experimenter in effect interrupted the subject's behavior after a trial. The intervention of the experimenter was possibly a factor that might interfere with the research. Hence, Skinner was interested in developing relatively automated equipment in which research could be conducted without the experimenter having to intervene, such as the tilt box and an automated pellet dispenser in the problem box. He tried numerous devices to record aspects of behavior that he thought would show quantitative order, such as holding a pencil against strips of paper as the paper unwound on a rotating drum, and other forms of kymograph scratches on smoked paper. Skinner's thinking here was that such devices would make changes in behavior visible and conspicuous, readily available for scientific analysis.

Skinner was particularly fond of tinkering with apparatus and hardware in the Psychology Department shop (e.g., "I was confirmed in my choice of psychology as a profession not so much by what I was learning as by the machine shop in Emerson Hall," Skinner, 1979, p. 31). He worked more or less without supervision. The physiologists thought he was being monitored by the psychologists, and the psychologists by the physiologists. The result was that he ended up doing pretty much what he pleased (Skinner, 1979, p. 35). He later quipped that "In graduate school I had the advantage of scarcely being taught at all" (p. 402). In a letter during this time to Percy Saunders, Skinner said that he had "almost gone over to physiology, which I find fascinating. But my fundamental interests lie in the field of Psychology, and I shall probably continue therein, even, if necessary, by making over the entire field to suit myself" (p. 38).

In other respects, Skinner was essentially apolitical while a graduate student, despite the grave economic and social concerns that plagued the country during the years of the Great Depression. In addition, he clearly wasn't much interested in conforming to the view of psychology held by E.G. Boring. Indeed, Skinner's biography reveals his distinctly cavalier and at times decidedly irreverent attitude toward Boring during this period (see also Bjork, 1993, pp. 95–98; Coleman, 1985b).

The Enduring Concerns with Epistemology: Bacon

Even with his growing interest in the direct, empirical study of behavior, Skinner did not abandon his original concerns with epistemology. He subscribed to a journal published by an exceedingly distinguished group of scientifically minded philosophers and philosophically minded scientists known as the logical positivists. He took courses in the history and philosophy of science from Professors Henderson, Sarton, and Whitehead of the Harvard faculty. Somewhat later in his time at Harvard, he joined the History of Science Society. A colleague in mathematics, Cuthbert Daniel, introduced him to the works of the physicist Percy Bridgman, who had recently proposed an interesting new principle by which to understand the meaning of scientific concepts called "operationism" (Bridgman, 1927). As we shall see, this principle was to play an important role in the development of Skinner's dissertation in 1930 and 1931, as well as in the development of radical behaviorism in later years.

A particular influence on Skinner throughout his graduate school years was Francis Bacon. Readers will recall from Part 1 that Skinner had initially encountered Francis Bacon while Skinner was in eighth grade, when he somewhat ingenuously investigated whether Bacon had actually written plays credited to Shakespeare. Skinner read several of Bacon's essays about science, methods, and epistemology at this time, and later acknowledged he didn't really get much from them. In high school Skinner started but failed to complete a wandering treatise, titled *Nova Principia Orbis Terrarum*. This work was partly literary and partly philosophical, and was concerned with nothing less than developing "a new principle of the universe" along Baconian lines. In the first volume of his autobiography, Skinner (1976, p. 295) perhaps a bit diffidently states it is just as well that only the first two pages of the work survive. In a college literature class Skinner had again raised the Baconian theory about Shakespeare, but then demurred during a class discussion (Skinner, 1976, p. 234). However, when Skinner arrived at Harvard, he was ready to engage Baconian epistemology full force: "I also planned to observe the history of science as it unfolded and, following Francis Bacon a little too closely, to take all knowledge to be my province" (Skinner, 1979, pp. 49–50).

In later autobiographical comments, Skinner (1984, pp. 406 ff.) mentioned that three Baconian principles characterized his professional life. Presumably, Skinner began to formulate these principles as a graduate student, when he began to actively engage in research. The first principle was to study nature, not books. The second was that nature to be commanded must be obeyed. The third was that a better world is possible, but it would have to be built, with the help of science. For example the principle to study nature not books is evident in Skinner's direct approach, sometimes called "atheoretical." In point of fact, Skinner was never atheoretical. As he later put it,

> Behavior can only be satisfactorily understood by going beyond the facts themselves. What is needed is a theory of behavior . . . . [T]heories are based upon facts; they are statements about organizations of facts . . . . [W]ith proper operational care, they need be nothing more than that. But they have a wider generality which transcends particular facts and gives them a wider usefulness . . . . [E]xperimental psychology is properly and inevitably committed to the construction of a theory of behavior. A theory is essential to the scientific understanding of behavior as a subject matter. (Skinner, 1972, pp. 301–302)

Rather, Skinner simply emphasized the Baconian principle that science dealt with nature, not with words about nature. Bacon proposed four pathological forms of scientific thinking, which he termed Idols, and Skinner (1967) later stated that "Bacon's four Idols can be translated into an acceptable behavioral analysis of faulty thinking" (p. 409; see also Moore, 2003).

The second Baconian principle is evident in Skinner's pragmatism and stance toward selection by consequences. On Bacon's view, scientific laws were instruments to help humans conduct themselves effectively in their interactions with nature. Skinner arrived at this same position through the influence of not only Bacon but also the conventionalist, anti-metaphysical stance of the French mathematician/philosopher Henri Poincaré, whom he read in graduate school. From Bacon, Skinner particularly realized that humans must devise their laws with due respect for the data, not their preconceptions of how the data should turn out: "Science is a willingness to accept facts even when they are opposed to wishes" (Skinner, 1953, p. 12). From Poincaré Skinner learned that "Science was . . . a rule of action that succeeds" (Skinner, 1979, p. 83), and that scientific questions were those that were answerable on the basis of experimental evidence: some questions were not only insoluble but illusory and nonsensical (e.g., Skinner, 1972, p. 453). Above all, one must respect nature. The analyses of environment and behavior are not arbitrary, but rather must take "account of the natural lines of fracture along which behavior and environment actually break" (Skinner, 1935a, p. 40).

The third Baconian principle is the unabiding optimism of the progressive approach based on science. Just as modernism sought the better life through science, Skinner saw that social melioration was possible through harnessing

the principles of a science of behavior and applying them to the world at large. Various utopias had been contemplated through the ages, but all were based on essentially mentalistic conceptions of the human condition. None were based on a genuinely scientific conception. The promises of a scientific approach to human society were extraordinary, just as Bacon had foreseen. This principle was ultimately manifested in Skinner's novel about utopian life, *Walden Two* (Skinner, 1948). Overall, Smith (1996) provides an excellent summary of the relation between Skinner's epistemology and Baconian principles.

The Enduring Concerns with Epistemology: Mach

As important as Bacon was to the developing Skinner, Mach was even more important. Smith (1995) has provided an excellent review of the influence of Mach on Skinner's intellectual development. The influence was both direct and indirect.

The direct influence was that Skinner read Mach while Skinner was a graduate student, taking a course from William Henderson in the history/philosophy of science. In this course, Skinner was strongly influenced by the descriptive, historio-critical method that Mach used to analyze the development of scientific concepts in *Science of Mechanics* (Mach, 1883/1942), rather than *Analysis of Sensations* (Mach, 1886/1959), which one would think would be more applicable to psychology. Also worth noting is that Mach's approach was consistent with Skinner's desire to write "objectively" during his Dark Year.

In regard to the direct influence of Mach, Skinner resonated favorably to Mach's admonitions to apply clear empirical thinking to rid science of metaphysical speculation. The topic of causation received particular attention. Rather than conceiving of causation in traditional push–pull or mechanical sense, Mach approached causation descriptively, from the point of view of experimenters: what were experimenters actually describing during their research when they talked of causal relations? According to Mach, they were describing correlated changes in two classes of phenomena. The changes could be represented economically as the functional relation between the two classes of variables in an equation. By focusing on the factors with which the behaving scientist was actually dealing, Mach believed he could avoid the technical problems that arose when explanatory efforts strayed too far from the data. Ultimately, in the Darwinian tradition, science was a human activity that contributed to the adjustment and adaptation of humans to their environment.

The indirect influence of Mach was through Loeb and Crozier. For instance, Loeb dedicated his classic work, *Comparative Physiology of the Brain and Comparative Psychology* (Loeb, 1900), to Mach. Skinner had read Loeb's text in one of his undergraduate biology classes. Moreover, Loeb's work was

entirely consistent with the Machian tradition of avoiding metaphysical assumptions in favor of careful description of the scientist's experience with a particular subject matter. The view of causation that Loeb applied in his work, that of functional relation, was taken directly from Mach, and Loeb described Mach's ideas on science as a source of "inspiration" and "energy" to him. Indeed, Skinner (1989, p. 122) explicitly identified the important influence of the Mach–Loeb–Crozier lineage in his intellectual development (see also Bjork, 1993, p. 65).

As Smith (1995, pp. 44 ff.) notes, several features of Skinner's research program while a graduate student follow directly from Mach. First, Skinner's research emphasized the careful and systematic description of what happens in the experimental setting. Concepts are to be related to actual observations, and researchers are encouraged to stay as close to the data as possible.

Second, Skinner's research was also inductive. It built on established findings, rather than leapt prematurely into unknown and ultimately unprofitable grounds.

Third, it did not accept the vernacular as the point of entry. Rather, terms must have a certain "experimental reality" to them, and barring the requisite validity, they are not included. For Skinner as it was for Mach (as well as Bacon), it does no good to simply translate everyday terms into behavior and leave everything else untouched because everyday language is fraught with a wide variety of distortions and errors.

Fourth, in keeping with Mach's view that scientific behavior was another form of adaptation, the techniques of analysis were to be judged empirically. For example, visual displays of data are simply "images of natural facts, ordered in such a way as to facilitate comprehension, induce relevant comparisons, and suggest inductive generalizations" (Smith, 1995, p. 46). The scientist must always stay close to the data, and the visual displays, graphs, and charts must serve that function.

Finally, Mach took a particular view on the role of theories and hypotheses in the development of science, as did Bacon. For instance, Skinner (1953, pp. 13–14) endorsed Mach's position that the first laws and theories of a science were probably rules developed by artisans who worked in a given area. A relevant passage from Skinner's writing is as follows:

> [Science] is a search for order, for uniformities, for lawful relations among the events in nature. It begins, as we all begin, by observing single episodes, but it quickly passes on to the general rule, to scientific law . . . . As Ernst Mach showed in tracing the history of the science of mechanics, the earliest laws of science were probably the rules used by craftsmen and artisans in training apprentices . . . . In a later stage science advances from the collection of rules or laws to larger systematic arrangements. Not only does it make statements about the world, it makes statements about statements. (Skinner, 1953, pp. 13–14)

As these early artisans interacted with nature, they developed skilled repertoires. Descriptions of the effects brought about by relevant practices were then codified in the form of verbal statements that functioned as verbal stimuli. The verbal statements, often taking the form of maxims or other informal expressions (e.g., "rules"), supplemented or replaced private or idiosyncratic forms of stimulus control. The verbal stimuli became public property, and were transmitted as part of the culture, enabling others to behave effectively. All these features of scientific activity follow from Mach.

In another article, Skinner (1972) embraced an avowedly Machian line of reasoning and more explicitly outlined the development of scientific knowledge. Skinner suggested there were three important steps in the development of a theory, and related these steps to theories in psychology:

> The first step in building a theory is to identify the basic data . . . .
>
> Since we have not clearly identified the significant data of a science of behavior, we do not arrive well prepared at the second stage of theory building, at which we are to express relations among data . . . . A weakness at the first stage of theory construction cannot be corrected at the second . . . .
>
> This step — at the third stage in theory building — can be exemplified by a simple example from the science of mechanics. Galileo, with the help of his predecessors, began by restricting himself to a limited set of data. He proposed to deal with the positions of bodies at given times, rather than with their color or hardness or size. This decision, characteristic of the first stage in building a theory, was not so easy as it seems to us today. Galileo then proceeded to demonstrate the relation between position and time — the position of a ball on an inclined plan and the time which had elapsed since its release. Something else then emerged — namely, the concept of acceleration. Later, as other facts were added, other concepts appeared — mass, force, and so on. Third stage concepts of this sort are something more than the second-stage laws from which they are derived. They are peculiarly the product of theory-making in the best sense, and they cannot be arrived at through any other process.
>
> There are few, if any, clear-cut examples of comparable third-stage concepts in psychology, and the crystal ball grows cloudy . . . . When it is possible to complete a theoretical analysis at this stage, concepts of this sort will be put in good scientific order . . . .
>
> From all of this should emerge a new conception of the individual as the locus of a system of variables . . . . A proper theory must be able to represent the multiplicity of response systems. It must do something more: it must abolish the conception of the individual as a doer, as an originator of action. This is a difficult task. The simple fact is that psychologists have never made a thoroughgoing renunciation of the inner man. He is surreptitiously appealed to from time to time in all our thinking, especially when we are faced with a bit of behavior which is difficult to explain otherwise. (pp. 305–308)

Skinner's subsequent objection to traditional theorizing is that many "theoretical" statements in psychology have not gone through anything remotely resembling a developmental process, three stages or otherwise (e.g., Skinner, 1950). The verbal responses called "theoretical" are controlled to a large extent by factors that are cherished for irrelevant and extraneous reasons, as in Bacon's "Idols." The responses are the product of many mentalistic if not

dualistic factors, they entail unfortunate metaphorical extensions, and so on. They appeal to other dimensions at the first and second stages, and consequently get off track. The implication is that as a result of these mentalistic influences, the stimulus control over what are hailed as advanced third-stage activities is suspect. Skinner's (1950) argument was that it was simply not necessary that the first and second stage activities be carried out to test a theory. They can "proceed in a rather Baconian fashion" (Skinner, 1969, p. 82). Indeed, as Skinner has noted, it may even be wasteful to conduct research that presumes to test a theory. The appropriate knowledge base needs to be established before useful third-step concepts will appear, and in many cases psychology is so contaminated by mentalism that it has not gone through the appropriate prior steps to establish that base.

In this regard, Skinner followed Mach and embraced the position that the function of scientific laws and theories was ultimately to promote direct, practical action. As had Mach, Skinner argued that theories in their best sense are economical descriptions of facts, promoting simplicity of expression and efficiency of practice (Smith, 1986, 1995). Theories in their worst sense are explanations of observed facts that appeal to events taking place somewhere else, at some other level of observation, described in different terms, and measured, if at all, in different dimensions (e.g., Skinner, 1950, p. 193). Theories in their worst sense have a variety of liabilities, including being incomplete and vague, obscuring important details, allaying curiosity by getting us to accept fictitious way-stations as explanatory, impeding the search for relevant environmental variables, misrepresenting the facts to be accounted for, falsely assuring us about the state of our knowledge, and perpetuating the use of scientific techniques that should be abandoned, for example, because they are wasteful (e.g., Catania and Harnad, 1988, p. 102). Theories about supposed mental events suffered from these problems, but the objection to mental or cognitive theories is ultimately on pragmatic, as opposed to ontological grounds. As Skinner (1978) was to put it later, "The behavioristic objection is not primarily to the metaphysical nature of mind stuff" (p. 72). Rather, the objection was that ultimately, mental or cognitive theories with their appeals to other dimensions do not lend themselves to effective prediction and control of behavior. Practical, direct action, not merely passive contemplation, was the hallmark of scientific activity.

As noted in the above hierarchy, many scientific laws and theories therefore have the character of statements that specify the relation between (a) responses on the part of those who entertain the laws and theories and (b) the consequences of those responses. In this regard, scientific laws and theories should not be understood as statements that are obeyed by nature. Rather, scientific laws and theories are statements that exert discriminative control

over individuals who need to deal effectively with nature. As Skinner (1969) later put it,

> Scientific laws also specify or imply responses and their consequences. They are not, of course, obeyed by nature but by men who deal effectively with nature. The formula $s = {}^1/_2\, gt^2$ does not govern the behavior of falling bodies, it governs those who correctly predict the position of falling bodies at given times. (p. 141)

Here again we have the familiar Baconian and Machian themes about statements that organize observations and facilitate desired outcomes. An additional influence is the work of Poincaré, discussed earlier.

Finally, readers may recall the earlier passage from Skinner (1972), in which he emphasized the theme that theories are "based upon facts; they are statements about organizations of facts" (p. 302). Skinner's statements here presumably concern more advanced, third stage scientific formulations. Skinner's statements are presumably consistent with Mach's concerns about causation, as well as Russell's (1932, p. 180) comment that cause and effect statements may turn out to be absent from certain scientific renderings:

> All philosophers, of every school, imagine that causation is one of the fundamental axioms or postulates of science, yet, oddly enough, in advanced sciences such as gravitational astronomy, the word "cause" never occurs. (p. 180)

What was Russell's alternative? In the empiricist tradition of his time, Russell (1932) embraced the position noted earlier by Mach:

> We then considered the nature of scientific laws, and found that, instead of stating that one event A is always followed by another event B, they stated functional relations between certain events at certain times, which we called determinants, and other events at earlier or later times or at the same time. (pp. 207–208)

Skinner's point in his analysis of theories is that statements of facts identifying cause and effect relations may well be conspicuous at the first and second stages of theory development, but the terms cause and effect may be absorbed into higher-order, third-stage statements taken as theories and explanations because of the verbal processes inherent in their development.

Overall, Skinner showed the influence of Mach by arguing that scientific activity and knowledge is ultimately predicated on a descriptively consistent level of observation and analysis. Behavior in particular is to be explained at the level of behavior, that is, at the level of the operations and prevailing stimulus conditions that impinge upon the organism. Therefore, Skinner later came to argue that one explains behavior by locating it "in a frame of reference provided by the [object] itself or by various external objects or

fields of force" (Skinner, 1938, p. 6). The usage here of "fields of force" testifies to the continued influence of Loeb. When Skinner (1938, p. 44) suggested he was interested in description rather than explanation, he was simply indicating that he was subscribing to the Machian preference for formulating observed functional relations, and that he was not going to appeal to inferred "powers and forces" from other dimensions, such as the neural or mental dimension, as causes of the observed behavioral events.

The Concept of the Reflex in the Description of Behavior

Although Skinner was very busy conducting research as a graduate student, and pursued multiple lines of research, subsequent analyses indicates that much of the research in the first and second lines, despite its ingenuity, frankly led only to a dead end (e.g., Vargas, 2004, p. 139). However, the data from the third line appeared promising. At the instigation of his professors, Skinner began to contemplate what he would do for a dissertation (Skinner, 1979, pp. 70 ff.).

As it played out, Skinner's dissertation was titled *The Concept of the Reflex in the Description of Behavior*. It had two distinct parts. The first part was a historical and theoretical analysis of the concept of the reflex. The second was a set of data from research with the panel-pressing problem box that Skinner had conducted up to that point. Somewhat consistently with the overall nature of his graduate education in the Harvard Department, Skinner (1967) later quipped "My thesis had only the vaguest of Harvard connections" (p. 399). Coleman (1985b) provides an authoritative review of the background of Skinner's dissertation.

The first part of Skinner's dissertation is especially relevant for present purposes. In the spring of 1930, Franklin Fearing published a book titled *Reflex Action* (Fearing, 1930). Despite the seeming promise of its title, the overarching thesis of Fearing's book was that the reflex was not the appropriate foundational basis for psychology. Given that Skinner had dedicated himself to the thesis that the reflex was in fact the appropriate foundational basis for psychology, he vehemently disagreed with Fearing's book. He sat down and wrote a "vitriolic" and "splenetic" review of the book (Skinner, 1979, p. 63), then took a copy of the review to Crozier. Crozier toned down some of the inflammatory rhetoric, added his name as coauthor, and ultimately facilitated its publication in a leading journal (Skinner and Crozier, 1931). Presumably in response to Fearing's book, and to add some legitimacy to his own stance, Skinner began to read extensively on the history of the reflex in both traditional reflexology as well as the history of medicine, in the late spring and summer of 1930 (Skinner, 1979, pp. 66–77). This period of study confirmed for Skinner that the concept of the reflex was paramount in the

study of behavior, but that its meaning had unfortunately migrated to something that was epistemically troublesome. In this first part of his dissertation, Skinner took an unselfconsciously Machian, operational approach to the concept of the reflex. Skinner's thesis was that the reflex meant simply an observed correlation between stimulus and response. The orderliness of the correlation would be revealed by the appropriate quantitative analysis and the resulting "smooth curves." Skinner then critically examined the use of the term reflex by Descartes, Hall, Magnus, Sherrington, Whytt, and others. He then pointed out the difficulties with the meanings that had developed historically. First, the reflex was relegated to the status of something involuntary, unconscious, and unlearned. Coleman (1981) notes that as a result of this treatment, many researchers thought the reflex "was not a plausible basis for explaining the voluntary, consciously guided, acquired behavior sequences of humans" (p. 211). This stance challenged the deterministic nature of a science of behavior, and for the crusading Skinner this challenge could not stand.

Second, the term had acquired physical, chemical connotations pertaining to the anatomical events that were inferred to mediate the stimulus–response correlation. As Skinner saw it, these connotations clearly distorted the observational basis of the term reflex, since in most cases the prior researchers were simply speculating about anatomy they had not actually observed. As Skinner later put it, "Pavlov's (1927) book was subtitled *An Investigation into the Physiological Activity of the Cerebral Cortex*, although he got no nearer the cerebral cortex than a salivary fistula. These men were studying the Conceptual Nervous System" (Catania and Harnad, 1988, p. 469). These scientific practices violated the practices advocated by Mach and Loeb of describing what one had seen and manipulated, so as not to get into metaphysical problems.

Third, the physiological speculations distorted the relations between a legitimate science of physiology and psychology, since in most cases the discovery of the observed correlation between stimulus and response preceded any anatomical investigation (Coleman, 1985b, p. 305). Skinner's point was that without the observed behavioral correlation, what anatomy would a researcher know to investigate? In place of the anatomical approach, Skinner argued that it was necessary to recover the historically original and epistemologically preferable definition of a reflex as simply an observed correlation between stimulus and response (Coleman, 1985b, p. 303; Skinner, 1979, pp. 67 ff.). This approach would restore the observational basis of the reflex, and establish behavior as a subject matter in its own right.

The data from the second part of his dissertation were on the ingestive patterns of the rat, and provided the empirical support for the theoretical arguments in the first part. This research used the panel pressing preparation in the "problem box" that Skinner had developed. In this research, Skinner

measured the cumulative amount eaten and panel pressing as a function of time. Responding was taken as reflecting the strength of the reflex of "ingestion," as Loeb had earlier investigated it. What Skinner found was that when he transformed the data (time, number of pieces eaten) into log–log coordinates, the resulting relation fit a straight line. This meant that the data could be described by a power function, $N = kt^n$. The slope of the power function was represented by the exponent n of the power function, and was approximately 0.7. For Skinner, this orderliness vindicated the argument that the reflex was the appropriate analytical unit in the explanation of behavior. Quantitative order had been shown! In a letter to his parents in at the end of March, 1930, he remarked

> The greatest birthday present I got was some remarkable results from the data of my experiment. Crozier is quite worked up about it. It is a complicated business and deep in mathematics. In a word, I have demonstrated that the rate at which a rat eats food, over a period of two hours, is a square function of time. In other words, what heretofore was supposed to be "free" behavior on the part of the rat is now shown to be just as much subject to natural laws as, for example, the rate of his pulse. (Skinner, 1979, p. 59)

In the fall of 1930, Skinner wrangled over numerous drafts of his dissertation with E.G. Boring of the Harvard Department, but eventually the dissertation was approved in January, 1931. His dissertation committee consisted of W.J. Crozier (his nominal mentor while at Harvard), C.C. Pratt, and L.T. Troland (Coleman, 1985b, p. 303). Skinner's dissertation may therefore be seen as the unique combination of his experiences in graduate school. He was working in a laboratory of general physiology, as it was the only place in the Harvard Department where he could examine behavior. In the summer of 1930 he independently studied the history of medicine and reflex physiology, which laid the foundation for his dissertation work (Coleman, 1985b, p. 302), even though he applied his ideas to the behavior of intact organisms instead of surgically isolated preparations. When these influences were combined with Bacon, Mach, operationism, his own objective and empirical inclinations, the behavioral epistemology of Russell, the order he had found in his own research, and a fierce determination to succeed — despite challenges from the influential E.G. Boring of the Harvard Department (Coleman, 1985b, p. 305; Skinner, 1979, pp. 72–75), the result was his dissertation — a "declaration of independence from physiology" (Skinner, 1989, p. 129), and another step in the progressive development of radical behaviorism.

Skinner at Harvard: Post-Doctoral Years

After receiving his doctoral degree in 1931, Skinner continued at Harvard in a series of post-doctoral appointments until 1936. The last was a three-year

appointment as a Junior Fellow in the newly formed and highly prestigious Society of Fellows. Three developments during this period are important in the understanding the development of radical behaviorism.

Lever-pressing

The first development concerns the lever-pressing preparation. We previously noted Skinner pursued three lines of research while a graduate student: (a) postural adjustments and locomotion, (b) suspended runways, and (c) ingestion, as measured initially in a runway and then in the first generation problem box. His dissertation concerned (c), but interestingly, Skinner continued his research in (a), employing the running wheel, even after he had completed his dissertation (Coleman, 1987, p. 54). Presumably, the running wheel was attractive not only because it related to general questions about physical movement, but also because it provided a cumulative measure of that movement, which was a datum that Skinner thought he might use to some advantage.

In any event, Skinner continued during his post-doctoral years to refine the problem box and the panel pressing preparation he had used in his dissertation. One important refinement was the use of a bent piece of wire, as a lever, in place of the door/panel to be pressed. Skinner had noticed that the panel swung very freely on its pivots and gave inaccurate readings, which proved to be a source of extraneous variability. Consequently, Skinner modified the apparatus by inserting a loop of wire, in the shape of a lever, into the chamber. The rat would then press the wire to get access to the food, which was delivered by a dispenser. Skinner then compared the data from the panel pressing line of research with the data from the lever pressing line. As before, with the log–log transformation, Skinner found that the data fit a straight line, again indicating a power function, $N = kt^n$. Importantly, the slope of the power function was again approximately 0.7. In understated fashion, Skinner (1979) commented, "I found that when I added the response to the lever to the usual 'eating reflexes,' the shape of the ingestion curve did not change" (p. 62). For Skinner, this quantitative order meant that the same process must underlie both the panel- and lever-pressing preparations, providing further empirical support for the generality and the validity of the reflex as the appropriate analytical unit in the explanation of behavior. By showing uniformity, behavior clearly emerged for Skinner as a subject matter in its own right (Catania, 1980; Moore, 1999). Skinner pressed on with his research, investigating other factors, such as "drive," that influenced reflex strength.

Classes of Stimuli and Responses

The second development concerns the notions of classes of stimuli and responses. An important characteristic of radical behaviorism is that its analytic concepts are functional, relational, and generic (Catania, 1980; Moore, 1999). Skinner conceived of behavior as a subject matter in its own right, and that conception meant that psychology was a deterministic science. Coleman (1984, pp. 479 ff.) argues that the crusading Skinner followed Crozier's lead and saw two issues as especially challenging to a deterministic science of behavior. The first issue concerned the variability of behavior. In other words, if behavior was to be studied according to the reflex model, which holds that stimuli cause behavior in something like a one-to-one relation, how could one account for variations in the form of behavior when the stimulus remained constant, or constant forms of behavior when the stimulus varied? The second issue was the apparent spontaneity of behavior. In other words, if behavior was to be studied according to the reflex model, which holds that stimuli cause behavior in something like a one-to-one relation, how could one account for behavior in the apparent absence of the requisite stimulus?

Clearly, behavior, especially in an intact organism rather than a restricted reflex preparation, does sometimes occur without an identifiable antecedent eliciting stimulus of the sort ordinarily associated with the concept of the reflex. For example, Coleman (1981) cited the following challenges to the extant S–R conceptions of behavior: (a) the developmental research of Coghill (1929); (b) studies of the responses of the newborn by Irwin (1932); (c) vigorous attacks from Gestalt psychology and organismic theory by Wheeler (1929); (d) the discovery that Pavlovian conditioning in humans — supposedly a simple form of learning through association — was actually far more complex than Watson's simple S–R conception had originally suggested (e.g., Hamel, 1919; Schlosberg, 1928); and (e) Lashley's (1929, 1930) failure to confirm important features of the S–R physiological model of learning favored by classical behaviorism.

Skinner tackled the first issue by making two moves. The first was to emphasize the secondary laws of the reflex. Primary or static laws of the reflex were concerned with the relation between some property of the response, such as its magnitude, with some property of the stimulus, such as its intensity. These laws came from reflex physiology. Secondary or dynamic laws of the reflex were concerned with the relation between properties of the response, and nonstimulus variables, such as drive state or fatigue. Skinner dealt with these variables as "third variables," which served to modify the primary laws of the reflex. By formally incorporating third variables, Skinner believed he could account for the source of the variability that was of concern to behavioral researchers, and much of his subsequent research directly inves-

tigated the influence of some of these third variables: drive (deprivation, satiation), conditioning, extinction, discrimination. Coleman (1984, p. 490) suggests Skinner simply "neutralized" the problem of variability by emphasizing third variables. Skinner did not originate this solution, of course. Loeb's work (e.g., Loeb, 1916, pp. 253 ff.), with which Skinner was intimately familiar, has page after page of how tropisms are modified by surrounding circumstances, such as variations in the chemical constituency of the medium of contact, whether the organism had recently been fed, and so on. The logical extension is to apply this same thinking to the notion of the reflex, which Skinner did. As Skinner (1979, pp. 117) later described it, he needed to refer not only to a stimulus and response but to conditions that changed the relation between them. The third variables were observable operations that were outside the organism. Tolman (e.g., 1932) saw the same problem but where Skinner kept the variables outside, Tolman inserted them into the organism, as replacements for, if not mere redefinitions of mental processes. Skinner (1989) later quipped that "that may have been the point at which the experimental analysis of behavior parted company from what would become cognitive psychology" (p. 109).

The second move that addressed the issue of variability in behavior took place in a paper published in 1935, titled "The Generic Nature of the Concepts of Stimulus and Response" (Skinner, 1935a). In this paper, Skinner added another feature to his "system": he simply declared that stimuli and responses "entering into a given correlation are not to be identified with particular instances appearing upon some given occasion but with classes of such instances" (Skinner, 1972, p. 491). Given the appeal to third variables to explain the variability of behavior, it was again a natural extension to appeal to variations in the stimulus (e.g., within the class boundaries) to explain the variability of behavior. Clearly, Skinner would have been familiar with the notion of thresholds for activation from his study of physiology, and it was only a small step to extend the notion of classes of action within ranges of stimuli to the generic sense of stimuli and responses. This move explained away the problem of variability by negating a requirement of exact reproducibility. It was not that behavior was uncaused, but rather that the causes varied within a class, so naturally there were variations in the effect. Exact reproducibility was rendered unimportant so long as the ostensibly underlying behavioral process yielded smooth analytic curves, even though some details in the actual instances of the process might differ.

Two Kinds of Reflexes

The third development during Skinner's post-doctoral years concerns the distinction between two kinds of reflexes, and what would become known as

the operant–respondent distinction. As we have noted above, a deterministic science of behavior was troubled by two issues. The first was how to account for the variability in behavior, and the second was how to account for the apparent spontaneity of behavior. Skinner neutralized the first issue by appealing to two explanatory factors: (a) dynamic/secondary laws concerning "third variables," to complement static/primary laws; and (b) the generic nature of stimuli and responses. The issue of the apparent spontaneity of behavior was a bit more complex. If behavior was to be accommodated according to the reflex tradition, in which a response occurs because a stimulus has elicited it, how can one account for the occurrence of a response in the apparent absence of the requisite eliciting stimulus?

Here Skinner showed the influence of Bridgman and operational analysis. For example, Skinner (1989) mentioned Russell, Loeb, Mach, and Bridgman as "the sources of my theoretical position in *The Behavior of Organisms*" (p. 122), but we may take the broader view that these four individuals influenced Skinner in the whole line of thinking that took place during his graduate student and post-doctoral years, culminating in Skinner (1938). Russell was responsible for general questions of epistemology in terms of the behavior of the scientist, Loeb for the study of the intact organism, Mach for renderings of causal processes in terms of functional relations at a descriptively consistent level, and Bridgman for analyzing the operations that were actually in effect during an experimental observation. Upon analyzing the operations that were in effect when a rat pressed a lever, obtained a food pellet, and then ate it, Skinner realized that the underlying behavioral processes differed from those underlying the ordinary Pavlovian reflex. For Skinner (1935b), the groundwork of which had been laid previously (Skinner, 1932), the classical Pavlovian reflex involved "the substitution of one stimulus for another . . . whereas in [the lever press] there is no substitution of one stimulus" (p. 75). The response in question was still a reflex, at least as Skinner then conceived of the reflex. As a reflex, it represented some correlation between stimulus and response. For Skinner (1935b), the response "must be elicited at least once as an unconditioned 'investigatory' reflex" (p. 68). At issue was what was the stimulus in subsequent occurrences of the response? Skinner solved that problem by designating the eliciting stimulus as the sight of the lever, and the response was to press. Importantly, the response depended on the food pellet, but since the food pellet came after the press, analysis of the process by which the response developed implies the process was of a different type than in say, salivary conditioning, where the food didn't depend on any response on the part of the organism. Skinner's solution was therefore to argue that the operation of presenting the food pellet strengthened an existing reflex (the investigatory reflex of pressing the lever); it did not create a new reflex to a new stimulus (as in evoking salivation to a light correlated with the food pellet).

Konorski and Miller (1937) then challenged Skinner's basis for distinguishing the two kinds of reflexes (e.g., see discussions of the challenge in Rescorla and Solomon, 1967, pp. 152 ff.; Coleman, 1981, pp. 218 ff.). Coleman (1981, p. 220) points out that in light of the challenge, Skinner then abruptly shifted gears. Interestingly, Skinner's arguments to this point had dealt with relations from the point of view of the subject. In other words, Skinner's analyses had laid out what sequence of events the subjects experienced in the preparations. Skinner (1937) proceeded to make the fundamental difference an *operational* one, from the point of view of the experimenter, rather than an *experiential* one, from the point of view of the subject. The experimenter arranged relations in the chamber such that the lever press produced the food pellet, according to the contingency between response and reinforcer. Once the response had occurred and the food pellet had been produced, an observer might infer that the situation reduced to the Pavlovian situation, but because the experimenter had arranged the response–reinforcer contingency it did not.

Skinner was then left with the problem of identifying the source of the first lever press. Originally, Skinner argued it was an investigatory reflex, elicited by the sight of the lever. However, this approach caused problems, as Konorski and Miller (1937) pointed out. Consequently, Skinner decided to abandon his idea that the initial response had been elicited by anything. Rather, he decided that it must occur without any eliciting antecedent stimulus: he asserted it was the "kind of response which occurs spontaneously" (Skinner, 1937, p. 274). This assertion was clearly a way out of the problem, but interestingly it involved an ontological claim — that operants were different — in the absence of any empirical justification. It was simply something plausible that allowed Skinner to explain what he needed to explain. Eventually, Skinner (1987) made the spontaneity and "random" origins of operant behavior a virtue:

> Operant conditioning is a second kind of selection by consequences. It must have evolved in parallel with two other products of the same contingencies of natural selection — a susceptibility to reinforcement by certain kinds of consequences and a supply of behavior less specifically committed to eliciting or releasing stimuli . . . . A second stage may have been the evolution of unconditioned behavior that had no survival value of its own but was available for selection through operant reinforcement. Such behavior would enable the individual to develop a much wider repertoire of behavior appropriate to novel environments. The human infant shows a large repertoire of such uncommitted behavior. (pp. 52, 71)

In other words, organisms that engaged in much uncommitted behavior had some sort of survival advantage over those with less uncommitted behavior, because there was much greater opportunity for consequences to select effective behavior.

At the time Skinner (1937) responded to Konorski and Miller, he had in fact been pursuing a long line of research, examining in particular the effect of the dynamic laws on the operant situation. This research was to be the basis of his first book, *The Behavior of Organisms* (Skinner, 1938). Presumably, many of the data from this research program were available to Skinner in 1937, although he couldn't present them in detail in the reply to Konorski and Miller. Coleman (1981, p. 223) outlined some of the empirical differences between the respondent and operant cases that Skinner said existed (see also Rescorla and Solomon, 1967). Some representative examples are (a) operant response force was more stable than respondent magnitude, which increases during conditioning (Skinner, 1938, p. 239); (b) operant response force is not well correlated with changes in rate (Skinner, 1938, p. 340); (c) operants can be conditioned with a single food pellet, whereas respondents may take several trials (Skinner, 1979, p. 88); and (d) operant latency changes in a different way than does respondent latency during extinction and with changes in drive level (Skinner, 1938, p. 240). This whole approach is very strange to the modern reader, as it does not overlap a great deal with modern concerns, as represented in the typical textbook on respondent and operant behavior. That the field has progressed since these original days is no doubt reassuring to many. In a later autobiographical statement, Skinner (1978) — this time evidently selfconsciously — acknowledged the limitations of his language at the time:

> In my reply [to Konorski and Miller, 1937] I used the term "operant" for the first time and applied "respondent" to the Pavlovian case. 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 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)

Coleman (1981, pp. 224–225) notes a curious irony in this whole story. Skinner began his research activity committed to the theses that all behavior was determined, and that all behavior could be accommodated by the concept of the reflex. He was able to deal with problems of variability by emphasizing the dynamic laws of the reflex, and by the notion of classes of stimuli and responses. However, the concept of determinism also implied something about the origin of a response. In arguing for determinism, Skinner tried to keep the origin of an operant response in the realm of the reflex: an operant started as an investigatory reflex. This move didn't work. As a result, Skinner backtracked, and simply asserted that behavior was orderly, although the first instance of an operant response was spontaneous, random, and from uncommitted behavior. That was exactly the position against which he had initially

crusaded, namely, that the analysis of complex, "voluntary" responses required different principles.

The Enduring Concerns with Epistemology: Verbal Behavior

Showing the influence of Russell, Skinner continued to work periodically on the relation between epistemology and verbal behavior during his postdoctoral years. In fact, Skinner titled one of his important projects "A Sketch for an Epistemology," in which he sought to engage a series of questions, ranging from what knowledge means to what experiences lead scientists to become knowledgeable, all in the Machian tradition. Central to the "Sketch" was an analysis of the functional role of verbal processes in the behavior of the scientist. Notes on this project go back to the early 1930s. This project was never finished as such, but bits and pieces, and one formal article, made their appearance over the years. In his autobiography, Skinner (1979, pp. 116–119) described his conception as it existed on November 17, 1932, of how he would work between the ages of 30 and 60. One element of his campaign would focus on the experimental description of behavior. In another he would work through the conceptual differences between behaviorism and traditional psychology. A third element would address theories of knowledge, emphasizing scientific knowledge. Here he would define epistemological concepts in terms of behavior, and address problems of meaning. A fourth element would deal with nonscientific epistemological questions, such as found in literature. The piece of the "Sketch" that was published was Skinner (1935a), on the generic concepts of stimulus and response, showing the direct link between the way Skinner conceived of the relation between laboratory research and the verbal processes of the scientist.

In another important event in the early 1930s, Alfred North Whitehead challenged Skinner to account for why one might say no black scorpions were falling on the table. This exchange marked the beginning of Skinner's formal excursion into the realm of verbal behavior:

> In 1934, while dining at the Harvard Society of Fellows, I found myself seated next to Professor Alfred North Whitehead. We dropped into a discussion of behaviorism, which was then still very much an "ism," and of which I was a zealous devotee. Here was an opportunity which I could not overlook to strike a blow for the cause, and I began to set forth the principal arguments of behaviorism with enthusiasm. Professor Whitehead was equally in earnest — not in defending his own position, but in trying to understand what I was saying and (I suppose) to discover how I could possibly bring myself to say it. Eventually we took the following stand. He agreed that science might be successful in accounting for human behavior provided one made an exception of *verbal* behavior. Here, he insisted, something else must be at work. He brought the discussion to a close with a friendly challenge: "Let me see you," he said, "account for my behaviour as I sit here saying, 'No black scorpion is falling upon this table.'" The next morning I drew up the outline of the present study. (Skinner, 1957, pp. 456–457)

The resulting book was not finished for over twenty years, but Skinner (1978, p. 122) regarded it as his most important work.

Also in 1934 Skinner was one of four young scientists invited to prepare position papers for the Rockefeller Foundation on how it might spend grant funds over the next 25 years to advance the social sciences (Skinner, 1979, pp. 165 ff.). Although one of the scientists (Hudson Hoagland, whose class in reflex physiology Skinner had taken while he was a graduate student) was interested in supporting physiology, the other two scientists joined Skinner in advocating the study of behavior. True to his developing interests, Skinner listed verbal behavior first, followed by self-instruction, discrimination, thinking, volition, responsibility, social and political motives, emotion, prejudice, functional and organic disorders, psychoanalysis, and criminal behavior. As may be readily seen, the list includes a wide variety of topics, but Skinner's proposal was that they all be analyzed from a consistent behavioral point of view, with verbal behavior at the top of the list.

He discussed the implications of a naturalistic epistemology with Van Quine, also a Junior Fellow and destined to become a world famous empirical philosopher. Importantly, Skinner concluded that the behavior of a scientist at work, with himself as a principal subject, was indeed a matter suitable for an empirical, behavioral analysis. He began an account of verbal behavior, including scientific verbal behavior, in terms of operant behavioral processes affecting the scientist. This was the start of a radical behaviorism. In his autobiography, Skinner (1979) commented on the consequences that selected his scientific practices:

> Was not confirmation the be-all and end-all of science? It was a question concerning my own behavior, and I thought I had an answer: ". . . What is the motivational substitute for thing-confirmation? Pretty important in teaching method to graduate students. Resulting *order* instead of *confirmation*?" My reinforcers were the discovery of uniformities, the ordering of confusing data, the resolution of puzzlement. (p. 282)

Skinner's Pragmatism

Finally, an important aspect of Skinner's approach was what we would now call pragmatism (Moore, 1999). In brief, pragmatism means that the importance of a scientific statement or concept is a function of its consequences: one statement or concept may be regarded as more important, or "truer" than another when it promotes or facilitates more effective action. We noted earlier that Skinner was influenced by Francis Bacon, who talked of using experiments to squeeze and mold nature to reveal its order, and then shape nature as on an anvil for the betterment of the human condition. In the Baconian tradition, knowledge was power, and so was it for Skinner. Skinner was also influenced by Mach, who talked of the adaptive purposes of human

activity: "the ways even of science still lead to the mouth" (Mach, 1886/1959, p. 23). In more modern terms, the pragmatic ideal of science was to predict and control events in a practical sense, to derive reinforcers from nature. Although Bjork (1993, p. 68) suggests Skinner never closely identified with pragmatic philosophy, Moxley (2001, 2002, 2003) has recently written extensively of the relation between Skinner and the pragmatic tradition in American culture, for example, as represented in James, Peirce, and Dewey. The sense of pragmatism discussed here coincides nicely with what we would now describe as the selection of behavior, including the verbal behavior of a scientist, during the lifetime of an individual on the basis of its reinforcing consequences, particularly whether it facilitates effective action.

Skinner himself came to unselfconsciously embrace a pragmatic conception of truth, as evidenced in the following passages:

> Scientific laws also specify or imply responses and their consequences . . . . As a culture produces maxims, laws, grammar, and science, its members find it easier to behave effectively without direct or prolonged contact with the contingencies of reinforcement thus formulated . . . . The point of science . . . is to analyze the contingencies of reinforcement found in nature and to formulate rules or laws which make it unnecessary to be exposed to them in order to behave appropriately. (Skinner, 1969, pp. 141, 166)

> Truth. The truth of a statement of fact is limited by the sources of the behavior of the speaker, the control exerted by the current setting, the effects of similar settings in the past, the effects upon the listener leading to precision or to exaggeration or falsification, and so on. There is no way in which a verbal description of a setting can be absolutely true. A scientific law is derived from possibly many episodes of this sort, but it is similarly limited by the repertoires of the scientists involved. The verbal community of the scientist maintains special sanctions in an effort to guarantee validity and objectivity, but, again, there can be no absolute. No deduction from a rule or law can therefore be absolutely true. Absolute truth can be found, if at all, only in rules derived from rules, and here it is mere tautology. (Skinner, 1974, p. 140)

> Science is a corpus of rules for effective action . . . . [A] proposition is true to the extent that with its help the listener responds effectively to the situation it describes. (Skinner, 1974, pp. 241–242)

The commitment to practical action, through prediction and control, was an integral aspect of the evolution of Skinner's radical behaviorist position, even at its very early stages. Skinner's pragmatism came from Bacon, Mach, Poincaré, and the general practical orientation in which he was raised in the first quarter of the twentieth century (see also Zuriff, 1985, pp. 257 ff.). Skinner's commitment to practical, effective action is evident when he later summarized his position by stating

> It is true that we could trace human behavior not only to the physical conditions which shape and maintain it but also to the causes of those conditions and the causes of those causes, almost *ad infinitum*, but there is no point in going back beyond the point at which effective action can be taken. (Skinner, 1974, p. 216)

Skinner's pragmatism is similar in some ways, but different in others, from the more traditional perspective known in the philosophy of science as "instrumentalism" or "conventionalism." Indeed, Poincaré, who influenced Skinner, is often associated with this traditional instrumentalist perspective. In brief, instrumentalism is the perspective that scientific concepts are merely conventionally accepted symbols, selected perhaps on the basis of their "convenience." On this view, what is important about scientific concepts is whether they are instrumental in mediating explanations of the phenomenon at hand. The assumption that underlies instrumentalism is that scientific concepts do not stand in a one-for-one relation with objects in nature that have permanent identity and structure as reflected in the concept. The similarity between instrumentalism and pragmatism is two-fold. First, both imply elements in an explanation are evaluated in terms of their contribution to the explanation or prediction. Second, both deny the elements in the explanation necessarily exist, with just the characteristics as proposed. The difference is that instrumentalism does not seek the basis for explanatory or predictive contribution of the elements; it simply accepts their contribution but nothing more. In contrast, pragmatism does promote an analysis of the basis of their explanatory value. Thus, the hallmark of pragmatism is effective action beyond verbal prediction. Effective action requires knowledge of factors that exist in space and time and that a researcher can manipulate to produce a given outcome. As did Bacon, Mach, and to some extent Poincaré, Skinner's radical behaviorism, then, subscribes to pragmatism but not instrumentalism (see additional discussion in Moore, 1998, pp. 228–229).

Summary and Conclusions: The Emergence of Radical Behaviorism as Epistemology

In conclusion, we have suggested that radical behaviorism is the unique epistemological position arising from the perspective of B.F. Skinner. It evolved in 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, his interests were stimulated by certain writings of Bacon, Russell, and Watson. After entering graduate school, he was very strongly influenced by Mach, Bridgman, and Poincaré. As Skinner worked on his own research program, both as a graduate student and a post-doctoral fellow, he applied his objective, empirical world view to analyses of his own behavior as a scientist as well as the behavior of his subjects. As the analyses proceeded, he developed a unique perspective on verbal behavior, which again he applied to himself as well as to others. With the unfolding of that view of verbal behavior, the evolution of

radical behaviorism reached its decisive stages. We consider the earliest formal example of the application of Skinner's analysis of verbal behavior to scientific epistemology in Part 3 of this series, concerned with Skinner's (1945) contribution to a symposium on operationism organized by the senior psychologist and Director of the Psychological Laboratory in the Harvard Department, E.G. Boring.

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