The Behavior Analyst 1995, 18, 225-236 No. 2 (Fall)

Jacques Loeb, B. F. Skinner, and the Legacy of Prediction and Control

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The biologist Jacques Loeb is an important figure in the history of behavior analysis. Between 1890 and 1915, Loeb championed an approach to experimental biology that would later exert substantial influence on the work of B. F. Skinner and behavior analysis. This paper examines some of these sources of influence, with a particular emphasis on Loeb's firm commitment to prediction and control as fundamental goals of an experimental life science, and how these goals were extended and broadened by Skinner. Both Loeb and Skinner adopted a pragmatic approach to science that put practical control of their subject matter above formal theory testing, both based their research programs on analyses of reproducible units involving the intact organism, and both strongly endorsed technological applications of basic laboratory science. For Loeb, but especially for Skinner, control came to mean something more than mere experimental or technological control for its own sake; it became synonomous with scientific understanding. This view follows from (a) the successful working model of science Loeb and Skinner inherited from Ernst Mach, in which science is viewed as human social activity, and effective practical action is taken as the basis of scientific knowledge, and (b) Skinner's analysis of scientific activity, situated in the world of direct experience and related to practices arranged by scientific verbal communities. From this perspective, prediction and control are human acts that arise from and are maintained by social circumstances in which such acts meet with effective consequences.

Key words: prediction and control, historical analysis, epistemology, Jacques Loeb, B. F. Skinner

For roughly two decades around the turn of this century, there existed an approach to experimental biology with important historical and conceptual links to the subsequent work of B. F. Skinner and the field of behavior analysis. Jacques Loeb was the major intellectual figure and public spokesperson for this viewpoint, which came to be identified as much with engineering as with mainstream biology. This approach was characterized by a firm commitment to experimental control, by analytic units that respected the integrity of the organism as a whole, and by an emphasis on technological applications. Between 1890 and 1915, Loeb's program stimulated a good deal of valuable research, spawned some important technological advances, and influenced some of the most notable and controversial life scientists of the 20th century, including John B. Watson and W. J. Crozier. Crozier was mentor to both Skinner and Gregory Pincus, the inventor of the birth control pill.[1]

Loeb is known to many through his work on tropisms—orientation in relation to a source of stimulation. By traditional accounts, Loeb pursued this work as a thoroughgoing mechanist, seeking to explain tropisms by reducing them to physiochemical building blocks (Fleming, 1964; Goudge, 1967; Palmer, 1929). It is normally in this light that Loeb's contributions to 20th century behaviorism are viewed (e.g., Moxley, 1992). With tropisms, Loeb provided a deterministic account of simple movements that did not appeal

An earlier version of this paper was presented at the convention of the Association for Behavior Analysis, May 1994. I am grateful to D. A. Dewsbury for providing background material on Loeb and to M. N. Branch, D. A. Dewsbury, E. F. Malagodi, H. S. Pennypacker, D. A. Stafford, M. Vaidya, E. A. Vargas, and especially E. K. Morris, for valuable comments and feedback on earlier versions of the paper.

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[1] Much of the material on Loeb included in this paper is from an excellent biography by Pauly (1987), reviewed in this journal by Logue (1988).

to hypothetical inner causes; this paralleled the stimulus–response approach of early behaviorism. As Loeb (1907) boldly proclaimed, "My aim was to analyze the behavior of animals from a physio-chemical point of view and to substitute the methods of modern science for the anthropomorphisms of the metaphysician" (p. 152).

It is not surprising that such a strong commitment to scientific determinism earned Loeb a place in the history of behaviorism. According to Pauly (1987), however, the relations between Loeb and early behaviorism identified in traditional accounts touch only indirectly on Loeb's more significant contributions to 20th century scientific understanding; these accounts miscast his impact on contemporary biology and psychology. The engineering approach Loeb came to represent, although superficially related to subsequent developments in early behaviorism, was most closely aligned with the brand of behaviorism later identified with Skinner. Both Loeb and Skinner adopted a hands-on approach to science that put practical control of one's subject matter above formal theory testing. Both also based their respective scientific programs on analyses of reproducible units involving the intact organism, and were wary of appeals to nonmanipulable causes. Finally, both Loeb and Skinner actively promoted technological applications of basic science. What united these various themes into a coherent approach was an unwavering emphasis on prediction and control as the fundamental goals of a scientific system.

The present paper has two main objectives. The first is historical—to trace the development of Loeb's engineering approach to biological problems, emphasizing its points of contact with Skinner's work. This should not only provide a greater appreciation for Loeb's approach to science (an interesting and important topic in its own right) but should also help to place Skinner's views within a broader historical framework. The present paper thus joins with Thompson's (1984) retrospective review of Bernard's work in physiology and Marr's (1985) retrospective review of Mach's work in physics in tracing some of the historical roots of behavior analysis. To avoid a "Whiggish" view of history, in which past facts are tailored to fit present circumstances, direct quotes are used wherever possible, and differences as well as similarities between Loeb and Skinner are noted.

Perhaps the greatest difference between the approaches taken by Loeb and Skinner concerns the relative importance placed on epistemological issues, specifically on how prediction and control relate to scientific knowledge. To some extent for Loeb, but to a far greater extent for Skinner, control came to mean something more than merely experimental or technological control for its own sake; it came to define scientific understanding. As Skinner (1947) asserted, "The experimental psychologist is fundamentally interested in *accounting for* behavior, or *explaining* behavior, or in a very broad sense *understanding* behavior" (1947, p. 26, italics in original). A second objective of the paper, then, is epistemological—concerned broadly with the nature and goals of science, and, more specifically, with the conditions under which prediction and control may serve as criteria for claiming that something is known or understood.

ORIGINS OF THE ENGINEERING IDEAL

Born and educated in Germany, Loeb came to the U.S. in 1891, where he held, over the years, academic positions at Bryn Mawr College, the University of Chicago (where he had personal contact with, and apparently much influence on, an impressionable young Watson; see Herrnstein, 1972), the University of California at Berkeley, and Rockefeller Institute for Medical Research, where he served as head of the Division of General Physiology until his death in 1924.

Loeb's move from Europe was the culmination of many years of growing isolation from the European physiological community, whose activities Loeb believed were too narrowly focused on problems of concern only to medicine, which drew firm lines between normal and pathological functioning. Loeb's move to the U.S. brought opportunities for him to participate in the new and active field of experimental biology, rich in possibilities and sufficiently broad in scope to include a wide range of intellectual pursuits. At the time of Loeb's move, the U.S. was also in the midst of rapid technological growth, fueled by advances in basic science. Loeb's work during the peak years of what Pauly (1987) has called "the engineering ideal" (1890–1915) intersected these two domains, as Loeb came to view his scientific activities in terms of their practical applications. In a letter to the physicist Ernst Mach in 1890, Loeb discussed his work in terms of its potential for transforming biology into "a technology of living substance" (Pauly, 1987, p. 4).

In his day, Loeb was known as a creative and tireless researcher, as well as an outspoken participant in academic and social debates. Loeb was widely known not only for his work on tropisms but also for controversial research on artificial parthenogenesis: the development of an unfertilized egg through physiochemical means. Loeb's research on these two topics was at the center of controversies concerning the role of science in transforming nature. His work on tropisms helped shape his hands-on engineering approach to biological problems; his work on artificial parthenogenesis allowed him to apply that approach to a fundamental question in biology—the nature and origins of life. Although he later shied away from such broad issues in favor of more clearly defined empirical problems, Loeb's engineering approach came "to symbolize both the appeal and the temptation of open-ended experimentation among biologists in America" (Pauly, 1987, p. 5).

The broad outline of Loeb's engineering approach was inspired by Ernst Mach, who served as an intellectual mentor as well as a kind of personal hero to Loeb. Although the two men never met, Loeb and Mach corresponded for over a decade just prior to the turn of this century. Loeb was first drawn to Mach's work in psychophysics, but Mach's most important influence on Loeb was philosophical. Loeb embraced Mach's unique brand of scientific positivism, in which science is viewed as human social activity and effective practical action is taken as the basis of scientific knowledge. For Mach, explanations appealing either to hypothetical inner causes or to basic building blocks achieved through reductionistic analyses were resisted in favor of economical expressions of functional relations. The most efficient route to such functional relations was through experimental control, which Loeb, after Mach, came to equate with explanation. Thus, to *control* a phenomenon—to specify the conditions responsible for producing it—was to *explain* that phenomenon, and vice versa.

These basic themes will have a familiar ring to those acquainted with Skinner's writings. Like Loeb, Skinner's general orientation to science owes much to Mach, including his eschewal of hypothetical causal entities, his emphasis on functional relations and experimental control, his antireductionistic stance, his treatment of analytic units, and his views on the relations between basic science and technology, among others. Given the relatively direct influences of Mach on Skinner (see Chiesa, 1992; Marr, 1985), it may seem superfluous to trace those influences through Loeb. As one who applied Mach's positivism to the behavior of living, intact organisms, however, Loeb's work holds special relevance for behavior analysis.

LOEB'S INFLUENCE ON SKINNER

Prompted by his teacher, "Bugsy" Morrill, Skinner came into contact with

Loeb's book, *Comparative Physiology of the Brain and Comparative Psychology* (1900), as an undergraduate at Hamilton College. He became better acquainted with Loeb's views through his contact with W. J. Crozier, head of the short-lived Department of Physiology at Harvard, in whose laboratory Skinner worked as a graduate student and postdoctoral fellow. Crozier was a devoted follower of Loeb, and much of the work undertaken in Crozier's laboratory was inspired by—indeed, was an empirical justification for—Loeb's views on the nature and goals of science. Crozier was a strong advocate of within-subject research methodology, with its emphasis on experimental rather than statistical control (see Kazdin, 1978, p. 93). As Herrnstein (1972) noted, "The line of behaviorist descent as regards actual research passes more conspicuously from Loeb *via Crozier* to Skinner, than *via Watson*" (p. 46, italics in original).

Although Skinner was intrigued by the tropisms studied by Crozier and his students—his first empirical publication was a paper on geotropisms in ants (Barnes & Skinner, 1930)—he found them too narrow in scope to account for most of the facts a thoroughgoing science of behavior would be called upon to address. "I did not take warmly to tropisms . . . the stimulating environment I cared about could seldom be described as a field of force or behavior simply as orientation or movement" (Skinner, 1979, pp. 45–46). Skinner appeared to be more impressed with Loeb's experimental strategies than with his specific subject matter:

> General Physiology [the approach founded by Loeb and championed by Crozier] dealt with overall quantitative laws. It was a methodology rather than a subject matter, and almost any data would serve if studied with the right methods. . . . An emphasis on method suited me, for I had my subject matter and was looking for ways of dealing with it. (Skinner, 1979, p. 45)

The Organism as a Whole

Reflexes were closer than tropisms to what Skinner deemed relevant to a comprehensive account of behavior. But, dissatisfied with what he regarded as premature theorizing about the nervous system, Skinner adopted a view closer to Loeb's "whole organism" approach (see Loeb, 1916) than to the physiological-neurological approaches popular with the reflexologists of that period. "I began to think of reflexes as behavior rather than, with Pavlov, as 'the activity of the cerebral cortex' or, with Sherrington, as 'the integrative action of the nervous system' " (Skinner, 1979, pp. 45–46). As Skinner (1956) put it,

> It had been said of Loeb, and might have been said of Crozier, that he "resented the nervous system." Whether this was true or not, the fact was that both these men talked about animal behavior without mentioning the nervous system and with surprising success. So far as I was concerned, they cancelled out the physiological theorizing of Pavlov and Sherrington and thus clarified what remained of the work of these men as the beginnings of an independent science of behavior. (p. 223)

By "independent science of behavior" Skinner was not advocating a "black box," nor was he implying that physiological processes were unimportant; rather he meant that inferences about the nervous system were unlikely to be fruitful until the behavioral facts were in order. Skinner believed Pavlov, Watson, and others had moved too quickly from their observations to their supposed underlying neurological bases. For Skinner, this amounted to little more than substituting one class of hypothetical terms (brain activity) for another (mental activity), with no corresponding gain in experimental rigor. Until physiological events could enter into effective action via the direct prediction and control of behavior, they would remain part of what Skinner (1938) called "the conceptual nervous system" and what Loeb called "the mysticism of the ganglion cells" (Loeb, 1890/1905, p. 114).

Analytic Units and Functional Relations

In calling for an independent science, Skinner was making a case for studying Loeb's whole organism in its own right and on its own terms. As behavioral, as opposed to physiological, facts, reflexes were defined functionally as correlations between directly observable events. As Skinner (1938) put it,

> So defined, a reflex [including what he would later call an operant] is not, of course, a theory. It is a fact. It is an analytic unit, which makes an investigation of behavior possible. . . . Many traditional difficulties are avoided by holding the definition at an operational level. I do not go beyond the observation of a correlation of stimulus and response. (pp. 9–10)

Loeb's work on tropisms in the 1890s was based on a similar approach to the definition of analytic units. As Pauly (1987) put it,

> Such a change in motion [an animal's movement] was the fundamental unit of behavior. The significant aspects of the behavior depended only upon . . . variables that could be controlled by the experimenter; and in the last analysis behavior was the result of these controlling variables. (p. 39)

The observed correlation of stimulus and response in tropisms and in reflexes defined a functional relation in Mach's tradition. *Functional* in this context can be understood not only in the biological sense of adaptive functioning but also in the mathematical sense of $y = f(x)$ (cf. Hineline, 1992). This latter usage is exemplified in Skinner's (1931) equation, $R = f(S, A)$, where response probability (R) is said to be a function of current stimulus conditions (S) and other variables (A). (The latter, which Skinner called "third variables," were conceptualized as conditioning history, emotion, motivation, or other conditions of the experiment that altered the relations between stimuli and responses.) The primary goals of a science of behavior were to map out these functions.

This general strategy was a natural outgrowth of Crozier's research program, an attempt to realize Loeb's dream of a mathematical approach to behavior (see Coleman, 1984; Day, 1980; Herrnstein, 1972). For Loeb, and especially for Crozier, this meant more than simply quantified observations: It meant empirically derived functions relating behavior to environmental changes. As Loeb (1912) stated, "For all 'explanation' consists solely in the presentation of a phenomenon as an unequivocal *function* of the variables by which it is determined" (p. 58, emphasis added).

Redefining *functional* in mathematical terms was important for Loeb, who for many years struggled with progressive evolutionists, for whom *functional* was understood in a very narrow biological sense. Implicit in popular evolutionary accounts of that period was the now-discredited notion that developmental changes (both biological and cultural) achieved some function ideally adapted to the surrounding world. The goal of science, according to this view, was to reveal the adaptive significance of changes in progress toward some ideal state of development. Although he accepted the facts of evolution, Loeb avoided post hoc appeals to adaptive functioning and the implication that evolution was synonymous with progress. Moreover, Loeb viewed the passive description and classification of historical facts as distinct from the active observation and experimentation that characterized his program.

Loeb's disagreements with the progressive evolutionists prompted his participation in a lively scientific debate between the preformationists, who believed that development proceeded along preexisting pathways determined solely by genetic makeup, and the epigeneticists, who believed that development was a gradual process of differentiation subject to nongenetic factors in the cell environment (see Gould, 1977, pp. 197–202). Although it was the preformationists who appealed to evolutionary processes, epigenesis is actually much closer to contemporary views of developmental biology with its emphasis on dynamic interactions between intracellular and intercellular processes (Oyama, 1985; Purves & Lichtman, 1985; see reviews by Midgley & Morris, 1992, and Provine, 1988, respectively). Loeb found epigenesis to be more congenial to his engineering outlook, because it dealt directly with the influence of external variables in modulating cell development, factors that were implicit in his later work on artificial parthenogenesis.

Hypothetical Processes and Private Events

In focusing their analyses on reproducible units involving the intact organism, Loeb and Skinner avoided appeals to hypothetical processes, because the subject matter was amenable to direct control and manipulation. As Skinner (1956) noted,

> When we have achieved a practical control over the organism, theories of behavior lose their point. In representing and managing relevant variables, a conceptual model is useless; we come to grips with behavior itself. When behavior shows order and consistency, we are much less likely to be concerned with physiological or mentalistic causes. (p. 231)

Loeb, too, argued explicitly for accessibility and practical control over the subject matter, using as a criterion whether an engineer could ever

> make use of these causes in the physical world. "Instinct" and "will" in animals, as causes which determine movements, stand upon the same plane as the supernatural powers of theologians, which are also said to determine motions, but upon which an engineer could not well rely. . . . Wherever I have thus far investigated the cause of such "voluntary" or "instinctive" movements in animals, I have without exception discovered such circumstances at work as are known in inanimate nature as determining movements. By the help of these causes it is possible to control the "voluntary" movements of a living animal just as securely and unequivocally as the engineer has been able to control the movements in inanimate nature. (Loeb, 1890/1905, p. 107)

This emphasis on the practical control over one's subject matter was part of a more general operational approach adapted from Mach's positivism. Although genuinely operational, Skinner's approach differed fundamentally from the operationism practiced by the more traditional "methodological behaviorism," which restricted its analyses to publicly verifiable events, taking interobserver agreement as the criterion of scientific validity (Skinner, 1945). For Skinner, scientific concepts were deemed meaningful or true if they worked—if they led to effective, practical action. From this standpoint, publicly observable events are favored in science not because they are more scientific or more physical than events within an organism's skin, but rather, for strategic reasons, because they are more accessible and hence more readily controlled and understood.

It might then be said of Loeb, as of Skinner, that hypothetical terms (including both mentalistic and physiological terms) were resisted on pragmatic grounds, because such terms were beyond the reach of experimental control and thus provide unsatisfactory explanations. Even when Loeb's attention was drawn to more molecular chemical preparations, these were merely attempts to bring aspects of an organism's physiology under experimental control (see Loeb, 1890/1905, pp. 576–623). Thus, for Loeb, as for Skinner, the boundaries between external (public) and internal (private) events were defined by the limits of experimental control. As Skinner (1953) put it,

> The line between public and private is not fixed. The boundary shifts with every discovery of a technique for making private events public. . . . The problem of privacy may, therefore, eventually be solved by technical advances. (p. 282)

The Natural and the Artificial

A related theme evident in the work of Loeb and Skinner was the shift in focus from the natural to the artificial, from patterns discovered in the world outside the laboratory to those discovered through contrived arrangements inside the laboratory. From an engineering standpoint, the laboratory is more than just a simplified version of the real world; it is part of that world. As MacCorquodale (1970) put it, "In the laboratory, variables are made to act 'one at a time,' for all practical purposes. The real world simply puts the environment back together again" (p. 98). But the ways in which the real world puts it back together represent only a small fraction of what is possible. In the laboratory, one can study not only the way things *are,* but the way things *can be* (cf. Malagodi, 1986, p. 16). As Skinner (1971) pointed out,

> A physicist does not confine himself to the temperatures which occur accidentally in the world at large, he produces a continuous series of temperatures over a very wide range. The behavioral scientist does not confine himself to the schedules of reinforcement which happen to occur in nature, he constructs a great variety of schedules, some of which might never arise by accident. (p. 155)

Loeb (1912) expressed a similar view on the relation between natural and artifical environments:

> What the biologist calls the natural environment of an animal is from a physical point of view a rather rigid combination of definite forces. It is obvious that by a purposeful and systematic variation of these and other forces in the laboratory, results must be obtainable which do not appear in the natural environment. . . . It was perhaps not the least important of Darwin's services to science that the boldness of his conceptions gave to the experimental biologist courage to enter upon the attempt of controlling at will the life phenomena of animals, and of bringing about effects which cannot be expected in nature. (p. 195)

Experimental control achieved in the artificial conditions of the laboratory or in planned interventions in nonlaboratory settings thus takes on special significance, not only as a key to analyzing nature but also of transforming it—by experimenting with novel forms that may give rise to effective practical applications. As Skinner asserted, "There is no virtue in the accidental nature of an accident. A culture evolves as new practices appear and undergo selection, and we cannot wait for them to turn up by chance" (1971, p. 155).

Productive Knowledge

This general experimental approach to problems of concern to society gave rise to technological applications. "By its very nature an experimental analysis of behavior spawns a technology because it points to conditions which can be changed to change behavior" (Skinner, 1983, p. 412). Indeed, the technology of behavior that has grown out of a laboratory-based experimental analysis has an impressive record of practical accomplishments with impact on human health and welfare, ranging from education and medicine to industry and public policy. The work of Loeb and his followers, too, had far-reaching technological spin-offs, including in vitro fertilization, oral contraception, and the artificial production of mutations (see Pauly, 1987, chap. 8).

Although important, such technological accomplishments were not viewed as justifications for one's program of research or conceptual viewpoint. For Loeb and Skinner, technology was more than simply extrapolation of laboratory findings to real-life affairs; it characterized an open-ended approach to science that was grounded in experimentation, transformation, and change. In the history of science, Loeb and Skinner belong to a tradition dating to Francis Bacon that was concerned with *productive* knowledge, gained actively through observation and experiment, rather than with *contemplative* knowledge, gained passively through classification and description (see Smith, 1992). The latter seeks to uncover the natural structure of the world or its underlying essences (cf. Palmer & Donahoe, 1992); the former seeks to understand the world by changing it. As Skinner (1953) stated,

> Science is not concerned with contemplation. When we have discovered the laws which govern a part of the world about us, and when we have organized these laws into a system, we are then ready to deal effectively with that part of the world. By predicting the occurrence of an event we are able to prepare for it. By arranging conditions in ways specified by the laws of a system, we not only predict, we control: we "cause" an event to occur or to assume certain characteristics. (p. 14)

As stated forcefully by Loeb a half-century earlier,

> We cannot allow any barrier to stand in the path of our complete control and thereby understanding of the life phenomena. I believe that anyone will reach the same view who considers the *control* of natural phenomena as the essential problem of scientific research. (Loeb, 1903, in Pauly, 1987, p. 114, italics in original)

In this context, the term *control* clearly refers to something more than either technological or experimental control for its own sake. It refers to something more akin to scientific understanding. Control is an epistemological anchor, which provides solid empirical grounds for claiming that something is known or understood. In other words, scientific action is effective, and thereby successful, to the extent that the subject matter can be controlled. As Loeb (1907) put it, "I laid emphasis on the fact that it is necessary to control the animal's reactions before explaining them, as only the control of the reactions offers a sufficient test for the correctness of our analysis" (p. 152). In a similar vein, Skinner (1953) stated,

> When we discover an independent variable which can be controlled, we discover a means of controlling the behavior which is a function of it. This fact is important for theoretical purposes. Proving the validity of a functional relation by an actual demonstration of the effect of one variable upon another is the heart of experimental science. (p. 227)

From this standpoint, the shaping or "bending" of nature in the production of novel effects is "the surest test of knowledge" (Smith, 1992). Prediction and control, then, are merely economical ways of expressing the pragmatic basis of scientific knowledge that Loeb and Skinner inherited from Bacon and Mach (see Morris, 1992).

PREDICTION, CONTROL, AND UNDERSTANDING

For at least the first three decades of this century, the life sciences were heavily pragmatic, both intellectually, in the philosophical pragmatism of Dewey and James, and technologically, in the service of scientific products to real-world affairs. In asserting the importance of prediction and control, Loeb and Skinner were expressing a viewpoint that was prevalent in both science and the culture at large. Why, then, in this scientific and cultural context, were the views of Loeb and Skinner so controversial?

The reasons are probably many, but one can speculate that some of the controversy surrounding their views was related to the narrowness with which prediction and control are normally understood. Although most may agree that direct prediction and control are important in the practical application of scientific principles to real-world problems, not all agreed with Loeb and Skinner, then or now, that prediction and control are synonymous with scientific understanding. Understanding the natural world through scientific principles appears to involve something more than mere prediction and control.

There are different senses, however, in which prediction and control imply understanding (see Morris, 1991, 1992). There is a shallow sense of understanding via control that comes from mere *demonstrations* of control, narrowly focused on the production of some effect without regard to how it works. There is a trivial sense in which this kind of arbitrary control yields understanding, but it falls short of the deeper understanding that results from the actual *discovery* of functional relations gained from a thoroughgoing analysis. This latter sense is a stronger sense in which control implies understanding, and is what experimental and conceptual analyses are really all about (see also Hayes, 1978).

This deeper sense of understanding is too often missed by critics (e.g., Schwartz, Schuldenfrei, & Lacey, 1978), who frequently equate the broad discovery-oriented goals of science with the narrower demonstration-oriented goals of engineering. For his part, Loeb did little to dissuade such a view. As Loeb revealed in a letter to Mach in 1890, "The idea is now hovering before me that man himself can act as a creator even in living nature, forming it eventually according to his will" (Pauly, 1987, p. 51). Similarly, in discussing his work on artificial parthenogenesis with a journalist of a popular magazine, Loeb expressed a desire

> to go to the bottom of things. I wanted to take life in my hands and play with it. . . . I wanted to handle it in my laboratory as I would any other chemical reaction—to start it, stop it, vary it, study it under every condition, to direct it at my will! (Loeb, 1902, in Pauly, 1987, p. 102)

Such bold statements invited comparisons to legendary technologists such as Faust and Frankenstein and further distanced Loeb's views from the biological mainstream. Although Loeb did at times equate control with scientific explanation, control for Loeb was usually in the service of technology rather than of understanding. Scientific understanding was a by-product of one's control over nature; it was not the primary focus. Loeb was more interested in demonstrating the power of a technological approach to life science than in examining its many implications. The goal of an experimental science, according to Loeb, was to control its subject matter; anything beyond that was regarded as distracting philosophical speculation.

Skinner's views, too, suffered from simplistic caricatures of a technologist narrowly focused on direct manipulation and control of behavior. In contrast to Loeb, however, Skinner was concerned with scientific understanding from the outset of his scientific career. Skinner's dissertation was concerned with the discovery of functional relations, in the tradition of Mach, and their elaboration into a more general theoretical system, as laid out in *The Behavior of Organisms* (1938). By the late 1940s, Skinner was explicitly calling for a theory of behavior, although one quite different from the more common logical-deductive theories that came to dominate psychology in the middle of this century (e.g., Hull, 1943).

By 1945, Skinner had distanced his views from more conventional varieties of behaviorism and had developed more fully some of the implications of a thoroughgoing pragmatic approach to scientific understanding. Skinner also called for an analysis of scientific knowledge itself through a careful examination of the verbal practices of the scientific community, which he later treated more fully in *Verbal Behavior* (1957). Scientific knowledge, according to this viewpoint, is rooted in the practical circumstances of everyday life, differing from nonscientific knowledge primarily in the special practices of its verbal communities that guide effective scientific behavior. Such practices are attempts to maintain proper control by the subject matter, partly by eliminating or weakening control by irrelevant (nonscientific) factors (see Skinner, 1974, p. 235). (What is deemed relevant and scientific, of course, changes from time to time and from verbal community to verbal community; cf. Kuhn, 1962.) The methods and concepts of a science, in other words, serve to maintain a particular type of contact with the world, which influences the kinds of questions that are asked and the kinds of evidence deemed sufficient in claiming that something is known or understood. Scientific facts or concepts can be considered true if they work; if they allow the scientist to successfully act on, and thereby better understand, the subject matter. Prediction and control are simply efficient means for achieving such successful action. For Skinner, scientific understanding became truly synonymous with effective prediction and control.

The kind of control over nature that eventually gives rise to scientific understanding is itself a natural behavioral process that is important in the survival of the individual and the culture to which it belongs. As Skinner (1974) put it,

> That an organism should act to control the world around it is as characteristic of life as breathing or reproduction. A person acts upon the environment, and what he achieves is essential to his survival and the survival of the species. Science and technology are merely manifestations of this

essential feature of human behavior. Understanding, prediction, and explanation, as well as technological applications, exemplify the control of nature. They do not express an "attitude of domination" or a "philosophy of control." They are the inevitable results of certain behavioral processes. (pp. 189–190)

SUMMARY AND CONCLUSIONS

The objective of this paper has been to uncover certain historical connections between Loeb and Skinner. Although Skinner's subject matter differed notably from Loeb's, his overall scientific outlook appears to owe much to Loeb, including his functional specification of analytic units, his emphasis on practical rather than statistical control, his rejection of hypothetical causal entities, his stance against reductionism, and his enthusiastic support for technological applications of laboratory science. Most important, however, Loeb's biology forced a consideration of prediction and control as the fundamental goals of an experimental life science. Skinner built upon Loeb's emphasis on prediction and control, both as a basis for an experimental science of behavior and for technological applications of that science. Skinner broadened and extended prediction and control to the realm of complex human behavior, including the circumstances giving rise to and maintaining scientific activity. This provided the beginnings of a scientific account of scientific behavior, which made it possible to treat prediction and control (or more precisely, predicting and controlling) as patterns of behavior to be explained rather than as scientific ideals to be exalted. Situating scientific activity within the world of direct experience made it possible to approach scientific understanding as a human act related to the social circumstances of its occurrence.

The same type of analysis can also be brought to bear on the historical development of a science. The history of a science is a history not of ideas but of practices, specifically of verbal practices characteristic of a particular scientific discipline. As Skinner stated,

> if the history of ideas seems to show the development of human thought, it is not because, for example, romanticism leads to classicism and vice versa, but because the practices characteristic of one ism eventually produce conditions under which a different pattern of behavior is generated and for a time maintained. (Skinner, 1974, pp. 145–146)

These practices are transmitted through rules—not formal rules of logic, but practical "rules for effective action" (Skinner, 1974, p. 235). Such rules relate not only to the shared methodological commitments that result in the standardized procedures characteristic of a science but also to the shared epistemological commitments that determine the kind of questions that are asked and the kind of evidence that is sought in claiming that something is known or understood. It is here, in the epistemological assumptions that guide a science, that Loeb and Skinner made their greatest contribution, and where their legacy looms largest. Their most radical departure from traditional approaches was their shared emphasis on prediction and control as goals sufficient for an experimental life science.

The approach taken by Loeb and Skinner raises a number of important questions concerning the nature and goals of science and the relations between science and technology, questions that are of more than mere historical interest. Are prediction and control the best or the only routes to scientific knowledge? Under what conditions does control imply understanding? Is it possible to control something without understanding it? Is it possible to understand something without controlling it? Perhaps Skinner's greatest insight here was his suggestion that such questions would be answered not in the contemplative world of ideas but in the practical world of everyday human activity. Bringing scientific principles to bear on the behavior of historians as well as scientists will not only reveal important facts about the social circumstances surrounding history and science but will also provide a basis upon which practical rules for doing better history and better science will one day emerge. It will then be possible to speak not only of a *history of science* but of a *science of history.*

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