*THE EXAMINING MAGISTRATE FOR NATURE: A
RETROSPECTIVE REVIEW OF CLAUDE BERNARD'S
AN INTRODUCTION TO THE STUDY
OF EXPERIMENTAL MEDICINE¹*

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An experimenter facing natural phenomena is like a spectator watching a dumb show. He is in some sort the examining magistrate for nature; only instead of grappling with men who seek to deceive him by lying confessions or false witness, he is dealing with natural phenomena which for him are persons whose language and customs he does not know, persons living in the midst of circumstances unknown to him, yet persons whose designs he wishes to learn. . . . He observes their actions, their gait, their behavior, and he seeks to disengage their cause by means of various attempts, called experiments. (Bernard, 1865, p. 31)

Inbedding his words unobtrusively near the conclusion of *An Introduction to the Study of Experimental Medicine*, Claude Bernard wrote, "In publishing a simple essay on the principles of scientific medicine, my idea is to be of some use to medicine" (p. 200). To say he was successful would be a hyperbolic understatement. Bernard's *Experimental Medicine* laid the foundation for 20th-century physi-

ology and was the basis for the experimental approach to modern medicine.

Contemporary with the life of Charles Darwin, Bernard's accomplishments were remarkable by any metric. The concepts of mechanism and site of drug action grew out of his research on the effects of curare on striped muscle and his research on carbon monoxide asphyxiation by competition with oxygen on the hemoglobin molecule. Bernard generated physiological knowledge at a startling rate. It was said of him that "He discovered as others breathed" (p. xvii). His work on glycogenesis and the pancreas, on experimentally produced diabetes, and on the function of vasomotor nerves stand as models today. But to observe that Bernard was a creative and prolific physiologist fails to capture the essence of his contribution. Bernard's *Experimental Medicine* provided the conceptual underpinnings of an inductive approach to the experimental analysis of physiological and—I will argue here—behavioral phenomena. Bernard's *Experimental Medicine* provided the cynosure for modern inductive biological thought.

The experimental analysis of behavior has typically had its academic home in the discipline of psychology. Paradoxically, the experimental analysis of behavior shares much more with the tradition of Claude Bernard than it does with those of Wundt or Freud. Indeed, if a 19th-century progenitor of contemporary behavior-analytic theory were to be identified, it would be Bernard and not, as is often claimed, James or Pavlov. James's pragmatism and Pavlov's reflexology were peripheral to the metatheoretical and theoretical foundations of modern behavior-analytic theory and practice.

Claude Bernard's *Experimental Medicine* shares basic features with contemporary behavior-analytic theory that distinguish it from the

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The author is deeply indebted to his friends and colleagues, Kenneth MacCorquodale and Paul Meehl, to whom any useful ideas contained in this review are equally owed. Special appreciation is expressed to Murray Sidman, whose *Tactics of Scientific Research* constituted a centenary celebration of Claude Bernard's Inductive Experimental Method and whose thought has served as a source of intellectual inspiration over the past twenty years.

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mainstream of psychology. The matters I discuss here include the nature of Bernard's subject matter, theoretical and metatheoretical assumptions, theoretical constructs, the inductive approach, reductionistic explanation, and the data base from which he arrived at generalizations. In the course of exploring these issues, it will become clearer why behavior analysis is so often an intellectual discipline without an appropriate academic home.

The phenomena Bernard observed were the subject matter of his science. Pancreatic secretions were not studied to reveal processes or events at some other level of analysis. Pancreatic juices were collected to reveal the functions of pancreatic cells. "Physiology," Bernard wrote, is "the science whose object is to study the phenomena of living beings and to *determine* the material conditions in which they appear" (p. 66; Bernard's italics). To study the phenomena of living beings meant to identify their controlling variables, and no more. His goal was to establish general laws expressing relationships among classes of independent variables and organismic dependent variables. "A scientific law gives us the numerical relation of an effect to its cause, and that is the goal at which science stops" (p. 67). The subject matter of psychology still seems to be controversial, though that of behavior analysis does not. Like Bernard, behavior analysts are concerned with the phenomena they observe; behavior, as Skinner argued (1938, p. 1), is a suitable subject matter in its own right, not because it reveals anything about phenomena at another level of analysis, whether physiological or mental.

The metatheoretical assumptions of Claude Bernard's experimental analysis of physiological phenomena involved a determinism expressed in terms of the objectives of prediction and control.

The whole experimental problem may be reduced to foreseeing and directing phenomena. (p. 57)

Indeed, when an experimenter succeeds in learning the necessary conditions of a phenomenon, he is, in some sense, its master; he can predict its course and its appearance, he can promote or prevent it at will. (p. 66)

The phenomenon must always and nec-

essarily be reproduced at the will of the experimenter. (pp. 67-68)

In *The Behavior of Organisms*, Skinner (1938) argued:

One step in the description of behavior is the demonstration of the relationships that are called reflexes. It leads to considerable power of prediction and control. (p. 10)

[But] we need to go beyond mere observation to a study of functional relationships. We need to establish laws by virtue of which we may predict behavior, and we may do this only by finding variables of which behavior is a function. (p. 8)

Echoing Bernard 95 years later, Sidman (1960) wrote:

The process of discovering the conditions under which a phenomenon occurs constitutes the first step of integration. (p. 33)

When an organism's behavior can repeatedly be manipulated in a quantitatively consistent fashion, the phenomenon in question is a real one and the experimenter has relevant variables well under control. (p. 85)

Bernard argued, with an emotional vociferousness that reminds one of Watson's and Skinner's early writings, that biological phenomena follow general scientific laws in much the same way as inanimate nature follows such laws. "I assert that the word exception is unscientific; and as soon as laws are known, no exception indeed can exist, and this expression, like so many others, merely enables us to speak of things whose causation we do not know" (p. 70).

One of the more striking similarities between Bernard's *Experimental Medicine* and contemporary behavior-analytic theory is the rejection of hypothetical constructs as explanatory devices. In their classic paper on the topic, MacCorquodale and Meehl (1948) defined hypothetical constructs as explanatory terms possessing surplus meaning over and above that contained in the relationship between antecedent operations and the resulting

observable consequences. To make his point, Bernard quoted Newton's account of the concept of gravitational force:

Bodies fall with an accelerated motion whose law we know: that is a fact, that is reality. . . . To picture the phenomenon to our minds, we may say that the bodies fall as if there were a force of attraction toward the centre of the earth, *quasi esset attractio*. But the force of attraction does not exist, we do not see it; it is merely a word used to abbreviate speech. (p. 67)

Later Bernard summarized his view concerning hypothetical constructs succinctly: "We must learn that the words we use to express phenomena whose cause we do not know are nothing in themselves; and that the moment we grant them any value in criticism or discussion, we abandon experience and fall into scholasticism" (p. 187). Skinner (1938, 1953) and other modern behavior analysts have typically argued that hypothetical constructs constitute an explanatory chimera whose heuristic value is illusory. In his 1938 book, Skinner called the constructs "drive" and "emotion" *states* and distinguished them from "will" and "cognition." The former were operationally defined intervening variables. Later, Skinner (1950) more explicitly attacked hypothetical constructs, arguing that they were not only unnecessary but misleading.

Skinner presented his most thorough argument against a hypothetico-deductive approach to an experimental analysis of behavior in his 1950 paper, "Are Theories of Learning Necessary?" Skinner erred in attacking "theory," in that he equated "theory" with "hypothetico-deductive theory." Over the years, several writers have correctly pointed out that Skinner, like every other scientist, has operated on the basis of fundamental theoretical assumption. However, behavior analysts share Claude Bernard's distrust of a hypothetico-deductive approach to science. Referring to hypothetico-deductive theorists, Bernard wrote:

These men start, in fact, from an idea which is based more or less on observation, and which they regard as an absolute truth. They then reason logically and without experimenting, and from deduction to deduction they succeed in building

a system which is logical, but which has no sort of scientific reality. . . . the mind becomes bound and cramped by the results of its own reasoning; it no longer has freedom of action, and so lacks the power to break away from that blind faith in theories which is only scientific superstition. (p. 37)

Indeed, like Sidman, who throughout *Tactics of Scientific Research* used examples of experimental hunches often not borne out by the data, Bernard provided examples of abandoned hypotheses leading to major findings. An especially delightful illustration was his research designed to show that blood sugar was broken down in the capillaries of the lungs. In the course of several unsuccessful attempts to prove his point, he found that the lungs had little to do with sugar degradation, but instead that all animal blood contained sugar even when the animal had not eaten any. Bernard wrote: "I therefore abandoned my hypothesis on the spot, so as to pursue the unexpected result which has since become the fertile origin of a new path for investigation and a mine of discoveries that is not yet exhausted" (pp. 163-164). Make no mistake, Bernard was not encouraging capricious casting about for serendipitous findings. Quite the contrary, he argued for what he called "inductive" or "experimental" reasoning. "When we see a phenomenon which we are not in the habit of seeing, we must always ask ourselves what is it connected with, or putting it differently, what is its *proximate cause*; the answer or the idea, which presents itself to the mind, must then be submitted to experiment" (pp. 152-153; italics added). By "*proximate cause*" Bernard meant a controlling independent variable.

Inductive reasoning was at the heart of the experimental method outlined in *Experimental Medicine*, as it was in *Behavior of Organisms* and *Tactics of Scientific Research*. Inductive reasoning was the antithesis of "doctrine" to Bernard, who wrote: "A doctrine, then, is a theory which we regard as immutable, which we take as a starting point for later deduction, and which we believe we are no longer obliged to submit to experimental verification" (p. 220). If I had not known better, I would have believed that I had seen in one of Skinner's works a paraphrase of the following sentences from Bernard—a paraphrase in which the term

theory replaced Bernard's usage of the term *system*.

We said that experimental medicine is not a new system of medicine, but on the contrary the negation of all systems. In fact, the advent of experimental medicine . . . will be only a regular and logical coordination of facts furnished by experience. (p. 218)

(In a personal communication, B. F. Skinner indicated that he had not read Claude Bernard prior to writing *The Behavior of Organisms*, though L. J. Henderson, who had dominated the Society of Fellows during Skinner's tenure as a Junior Fellow, was a devoted follower of Bernard. Murray Sidman read Bernard's *Experimental Medicine* after writing *Tactics of Scientific Research*.)

Watson and, to some extent, Skinner parted ways with Bernard on the issue of reductionism, but this disagreement says more about radical environmentalism than about fundamental features of behavior-analytic strategy. Bernard wrote:

If we wish to find the exact conditions of vital manifestations in man and the higher animals, we must really look, not at the outer cosmic environment, but rather at the inner organic environment. (p. 98)

We must, moreover, learn that the *intimate particles* of an organism exhibit their vital activity only through a necessary physico-chemical relation with immediate environments which we must also study and know. (p. 63)

Claude Bernard introduced the concept of the "internal environment," a hypothetical notion until research on interoceptive conditioning in the 1960s made it an objective reality. "In a word, vital phenomena are the result of contact between the organic units of the body with the *inner physiological environment*; this is the pivot of all experimental medicine" (p. 76).

Bernard alluded to a reflex concept in discussing the relation of an organism to its environment, though he failed to explicate it.

As a natural phenomenon is only the expression of ratios and relations and connections, at least two bodies are necessary

to its appearance. So we must always consider, first, a body which reacts or which manifests the phenomenon; second, another body which acts and plays the part of the environment in relation to the first. (p. 71)

Life results from contact of the organism with its environment; we can no more understand it through the organism alone than through the environment alone. It is therefore a similar abstraction, that is to say, *a force which appears as if it were outside of matter* [italics added]. (p. 75)

In one of the most important sections of *The Behavior of Organisms*, Skinner wrote:

The environment enters into a description of behavior when it can be shown that a given *part* of behavior can be induced at will. . . . Such a part, or modification of a part, of the environment is traditionally called a *stimulus* and the correlated part of the behavior a *response*. Neither term may be defined as to its essential properties without the other. For the observed relation between them I shall use the term *reflex*. (p. 9)

Bernard, having addressed himself to the abstract concept "life," which preoccupied scholars in mid-19th-century literature, in another place applied the same reasoning to human behavior:

If we wish to judge the actions of another man and to know the motives which make him act, . . . we are . . . forced to interpret and imagine it from the movements that we see and the words that we hear. So we must verify the man's acts, one by another; we consider how he behaves in such and such circumstances, and in short, we turn to the experimental method. (p. 31)

The differences in commitment to reductionistic explanation that separated Watson and, to some extent, Skinner from Bernard have little to do with behavior-analytic strategy. This epiphenomenal commitment was not central to the analytic method. Moreover, I believe that Bernard, if he were alive today, would probably agree with Ernst Mayr (1981) in doubting our ability to reduce the laws of

one science to those of another (i.e., explanatory reductionism). There is nothing about the behavior-analytic strategy that requires a demarcation line to be drawn at the skin, as Skinner (1945) and Schnaitter (1978) have pointed out. Interoceptive discriminative stimuli (cf. Thompson & Pickens, 1971), reinforcing effects of internal states (e.g., drug administration: cf. Thompson & Johanson, 1981; Thompson & Pickens, 1969), and the role of genetic variables as modulators of the degree to which environmental variables can operate, all remind us that the skin is not an important conceptual barrier. Although the environmentalism of Watson and Skinner are understandable, they are not cardinal to behavior-analytic strategy.

The Unity of Sciences movement which gained wide popularity in the 1950s operated on the assumption that explanatory reductionism was an inevitable outcome of more sophisticated research methods. Explanatory reductionism requires that the laws of one science are translatable into the laws of another science, *with no surplus meaning left to be accounted for*. To the best of my knowledge, there are no cases in which all the laws of one science are reducible to those of another. In fact, on a careful reading of Bernard, it becomes clear that he seriously doubted whether it was possible to reduce physiological laws to laws of chemistry and physics.

Though we can succeed in separating living tissues into chemical elements or bodies, still these elementary chemical bodies are not elements for physiologists. . . . it would be impossible to establish any relation between the vital properties of bodies and their chemical composition; because tissues and organs endowed with the most diverse properties are at times indistinguishable from the point of view of their elementary chemical composition. . . . For physiologists, the truly active elements are what we call anatomical or histological units. (p. 73)

Although he, like virtually all modern biological scientists, subscribed to a doctrine of *constitutive reductionism*, he obviously questioned *explanatory reductionism*.

Bernard was concerned, then, with the analysis of physiological phenomena at the level of physiological variables—that is, of anatomical and histological events. He seemed to assume

that infralevel inductive theory was not only appropriate but powerfully effective. In this respect, his strategy was similar to that of contemporary behavior analysis, in that the explanatory terms of behavioral events are largely at the same level—the overt exteroceptive observation level—as the variables manipulated and observed. Though the classes of events of interest differed (myoneural and glandular processes as opposed to integrated molar striped muscle response activities), the infralevel mode of analysis is common to both.

Although there are other similarities between *Experimental Medicine* and modern behavior-analytic thought, a final group of factors I would like to discuss here concerns the data base from which scientific conclusions are to be drawn. Bernard was committed to basing generalizations on data obtained from individual organisms. "A physician, in fact, is by no means physician to living beings in general, not even physician to the human race, but rather, physician to a human individual" (p. 92). Bernard was very skeptical of averaging data across individuals, going so far as to write:

In physiology, we must never make average descriptions of experiments, because the true relations of phenomena disappear in the average. (p. 135)

By destroying the biological character of phenomena, the use of *averages* in physiology and medicine usually gives only apparent accuracy to the results. (p. 134)

If Bernard distrusted averaged data, he outrightly rejected statistical probability estimates.

What a physician needs to know is whether his patient will recover, and only the search for scientific determinism can lead to this knowledge. (p. 138)

If based on statistics, medicine can never be anything but a conjectural science; only by basing itself on experimental determinism can it become a true science. (p. 139)

In a similar vein, Skinner (1938) wrote, "The physician who is trying to determine whether his patient will die before morning can make little use of actuarial tables" (p. 443).

Though *Experimental Medicine* is not an applied work, Bernard addressed the relationship between basic science principles and clin-

ical applications at several junctures, none more pointedly than near the end of the book:

Since there is no such thing as a medical work of art, there is no such thing as a medical artist; physicians calling themselves such injure medical science. . . . In my opinion, then, the inspiration of physicians, who do not rely on experimental science, is mere fantasy; and in the name of science and humanity they should be rebuked and proscribed. (p. 204)

Reading *An Introduction to the Study of Experimental Medicine* is like exploring the intellectual ruminations of a great-grandfather one has known only through family oral history and tin-types. Bernard's jousting with vitalism, like Skinner's struggle with mentalistic metaphors, is part of the deterministic battle over free will. The battlefield was the laboratory and the instrument of combat was the inductive experimental method. The competition for an understanding of behavioral phenomena persists. Like Mark Twain's famous 1897 cable to the Associated Press from London ("The reports of my death are greatly exaggerated"), I am pleased to report that the periodic obituaries by newspaper writers and cognitive psychologists announcing the demise of the experimental analysis of behavior

are premature. Claude Bernard's method is alive and well in the thriving discipline of behavior analysis, which, like its progenitor, strives to play the role of "examining magistrate for nature."

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