Introduction

Shaping a Profession: Behaviorism in American Psychology

Historians agree that behaviorism was the dominant force in the creation of modern American psychology.[1] Now that psychology has returned to the eclecticism of its earlier years, we can analyze behaviorism's role in American psychology. Yet scholars of behaviorism stand face to face with a paradox. It would appear that we know everything we could possibly want to know about behaviorism, but behaviorism and its role in psychology remain mysterious and enigmatic. We know everything about behaviorism because behaviorists themselves have written numerous accounts of behaviorism in general as well as of various specific aspects of it, because we have a surfeit of secondary accounts of behaviorism and of behaviorist theories, and because we have volumes of critical writing on behaviorism. Even so, behaviorism remains an enigma because its dominance in American psychology blocks our efforts to understand its role and its nature. American psychologists (and many outside the United States or Canada, especially in the English-speaking world) are trained to think behavioristically from their earliest undergraduate years, usually without being made aware, or realizing, that this is the case. A truly committed and highly trained American psychologist who strives to articulate the fundamental elements of his or her research practices will state a set of behaviorist propositions because it is the academic culture of behaviorism that will dictate the seemingly self-evident basis of the psychological enterprise.

Any American psychologist who searches for understanding in a comparison of psychology with other social sciences will have great difficulty in reaching nonbehaviorist territory. Behaviorism was the soil nourishing early American social science. In the late nineteenth

and early twentieth centuries there was a symbiotic relationship between social scientists and the intellectual lay public. The writings of the social scientists were read and understood because they took the unformed opinions of their readers and articulated them. Because Americans characteristically view science pragmatically, many of those readers, as well as the social scientists themselves, used what they read as the basis for programs of remedial social action. Those programs, in their turn, provided material for further analysis for the social scientists and, above all, provided the early institutional basis for the growing social sciences. The essence of behaviorism is the equating of theory with application, understanding with prediction, and the workings of the human mind with social technology. Those same equations formed the foundations of the thought of early American social scientists. We now know enough to say with confidence that psychological behaviorism arose not within psychology itself but within American society from about the 1880s onward. It is also clear that the research practices and the theorizing of American behaviorists until the mid-1950s were driven by the intellectual imperative to create theories that could be used to make socially useful predictions.

A critical analysis of the theories of the leading behaviorists, especially John Broadus Watson (1878–1958), Edwin Ray Guthrie (1886–1959), Edward Chace Tolman (1886–1959), Clark Leonard Hull (1884–1952), and Burrhus Frederick Skinner (1904–1990), would in itself be sufficient to reveal the unquestioned—and unquestioning—hegemony that behaviorism established. However, to restrict our analysis to the "giants" of behaviorism would only emphasize their predominance at the expense of historical truth, because their work represented salient expressions of a much broader worldview characteristic of American would-be social scientists. Our best course, then, is to trace the history of behaviorism from its very beginnings not just in American social science but in the intellectual and social context of that social science. Before we begin that examination, however, it is appropriate to pause here and define what we mean by "behaviorism."

Behaviorist and neobehaviorist theories vary widely among themselves, so widely that some scholars would say that no common features can be discerned.[2] The issue is further complicated by the necessity to make two sets of distinctions, philosophical and psychological. Philosophically, we must distinguish between radical behaviorism,

methodological behaviorism, and logical behaviorism.[3] A radical behaviorist believes that the mental and the physical are identical and that mental events can be fully explicated in a physicalist language. Skinner is usually said to be a radical behaviorist. When we take unpublished material as well as published texts into account, Hull can also be treated as a radical behaviorist. Most of those American psychologists who were actively engaged in empirical research were, until recently, methodological behaviorists. They believed that all psychological constructs should be defined operationally, that is, in terms of the procedures required to induce concrete manifestations of the behaviors functionally related to the constructs under investigation. They refused to discuss the metaphysical implications of their position. Watson, Tolman, and Guthrie fall into this category. Logical behaviorism is a position taken by some philosophers but is not really represented in psychology (although certain passages in Skinner's writings suggest that he could also be treated as a logical behaviorist). A logical behaviorist asserts that all mental language can be translated, without loss of meaning, into physicalist language (the language in question states what behavior is to be expected when a person claims to be or is thought to be experiencing some mental state). Logical behaviorists resemble methodological behaviorists in that they leave open the question of the substantive reality of mental states and resemble radical behaviorists in that they analyze all mental states with equal thoroughness.

Psychologically, we must distinguish between behaviorism and neobehaviorism. Behaviorism as such flourished most strongly in the 1920s. Early behaviorists shared a common set of concerns, in which negative considerations outweighed positive. All denied any intrinsic life to the mind, none believed that the mind was psychology's primary area of study, and all believed that introspection was a futile and misleading way of gathering psychological data. In a positive sense, all were objectivists (that is, they believed that the only real data were those that could be directly observed). The early behaviorists, with some exceptions, all shared the faith that behaviorist doctrine could be directly applied to human beings and that experimentation with humans provided a direct route to knowledge. Almost all also believed that psychological research would have direct social implications. Above all, no behaviorists produced fully worked out, comprehensive, empirically based theories.

Tolman instituted neobehaviorism in the 1920s. Almost all neobehaviorists were animal scientists, and unlike behaviorists, they produced highly sophisticated and, in some cases, comprehensive psychological theories. The major neobehaviorists, at least, shared the behaviorist commitment to social application, but believed that such applications should be mediated through empirically tested theories, whose ultimate derivation was the highly controlled environment of the animal laboratory. Such theories, together with those corollaries that provided the theoretical justification for applications, constituted behavioral science, which enjoyed its heyday in the America of the 1950s and 1960s.

Despite the major differences between the various forms of behaviorism, I believe that all behaviorists shared a set of prior commitments. The first of these commitments concerns the relative value given to theory and its applications. The American historian John C. Burnham expressed it cogently. He once said to me that behaviorists wanted their practical work to have a theoretical basis but the particular theory used did not matter to them. That view is expressed in the opening sentence of one of J. B. Watson's articles: "The theoretical goal of psychology is the prediction and control of behavior."[4] Psychologist Franz Samelson found that sentence puzzling. Conventionally, we think of prediction and control as practical matters separate from (but derived from) theory. He claimed that one could solve the puzzle by assuming that, for Watson, technological imperatives guided the search for theory.[5] Burnham's and Samelson's interpretations are essentially the same. Within that framework of interpretation we can treat Clark L. Hull's ruthlessly instrumental approach to theory building as the fulfillment of Watson's intentions. B. F. Skinner's dismissal of the need for theory represents the high point of the behaviorist enterprise.[6] Of all the major behaviorists, only Edward C. Tolman held himself aloof from that enterprise and, in most of his work, showed himself to be a pure theorist. But even Tolman wanted psychological doctrines to have very direct application to everyday life.[7]

A second feature of behaviorism was a suspicion of, or outright hostility to, philosophical speculation. To some extent this is an integral part of psychology's development. In order to establish itself as a profession, psychology had to differentiate itself from its closest academic neighbors. Initially that separation was incomplete because the

first American psychologists, such as William James (1842–1910), James Mark Baldwin (1861–1934), and G. Stanley Hall (1844–1924), were all trained as philosophers. The second generation of American psychologists rapidly and forcefully distanced themselves from philosophy. Although that movement was very broadly based, with psychologists as diverse as Robert Mearns Yerkes (1876–1956), Edward Lee Thorndike (1874–1949), and Watson in the forefront, it was the promise held out by behaviorism, a promise that became increasingly alluring from the 1920s to the 1960s, that inspired American psychologists to keep themselves aloof from philosophy. In contrast, European psychologists on the whole remained receptive to philosophical influences.

A third defining feature of behaviorism is the acceptance of pragmatic versions of positivism. Since both pragmatism and positivism are philosophical doctrines, a contradiction lies at the heart of the behaviorist enterprise. Philosophy, by decree, is excluded from the behaviorist club; nevertheless, upholders of some philosophical doctrines are honored members. All behaviorists were positivists, and in behaviorism's early and mature periods (the 1920s to the 1950s), all were pragmatists. Behaviorists were positivists because they believed that one could establish the truth by appealing to facts. For them, a fact was some sort of purely physical occurrence. With respect to the substance of theories, they believed that theories were created out of facts, and that the role of theories was to increase the scope and the precision of prediction. Even for Tolman, theory had a strictly pragmatic role. For all the behaviorists, theory construction was a seesaw process whereby one began with crude outgrowths from observations and slowly created one's theory in such a way that one could make more and more precise observations, building those observations into the theory at each stage. No behaviorist ever considered the possibility of taking existing comprehensive theories of the mind and testing or refining them.[8]

In their pragmatic positivism the behaviorists were archetypally American. While American intellectuals have studied and admired European thought, the leading American thinkers have typically tried to cast their own ideas within a common nationally accepted framework. In particular, Americans have always linked theory closely to application, even at the risk of being simpleminded or crude. As behaviorism developed, the chief outside influences upon it were Gestalt

psychology and logical positivism. However, both schools of thought were inspirational to the behaviorists only in a very general way, giving global objectives rather than specific models. In the case of logical positivism, historian of science Laurence Smith showed that Hull, Tolman, and Skinner each created an idiosyncratic version of positivism quite independent of logical positivism.

A fourth defining feature of behaviorism is its materialism. The issues here are clouded because of the behaviorists' dismissal of philosophy and because, in the case of the later behaviorists, they believed that their theoretical approach allowed them to predict and therefore explain behavior without taking any particular philosophical position on the mind/body problem. Nevertheless, a close scrutiny of the writings of the leading behaviorists shows that in various ways they were all materialists. Although Watson started the behaviorist tradition of hedging philosophical bets, he was not willing to state his behaviorist creed openly until he could analyze thought behaviorally. His theory of thinking is clearly materialistic.[9] Guthrie's theory is implicitly materialistic in that he limited his examples to perceptual-motor skills, thereby giving the impression that his analysis could be extended to purely mental events without conceptual change. Hull's belief in biomechanical materialism was the underground force that drove his theorizing.[10] Skinner was notoriously hard to interpret in a clear-cut way. But because he asserted that private events were simply those that occur "within the skin" (that is, mental and physical events were substantively equivalent), and because he claimed that we had to use public criteria to establish the meaning and nature of a mental event, he should, I think, be counted as a materialist.[11]

In writing that psychology's *theoretical* goal was the prediction and control of behavior, Watson succinctly expressed the spirit of his era and behaviorism's fifth defining feature. Even his friend Yerkes, who kept himself aloof from the fervor of behaviorism, was obsessed by social control and social technology. As early American social scientists, both men saw theory as an instrument fitted to achieve beneficial and radical social change. A problem for Watson, as it was for other early behaviorists like Stevenson Smith (1883–1950) and Guthrie, was the large gap between the complex empirical phenomena to be explained and the simple and crude theories. The behaviorists successfully constructed theories capable of supporting their very large ambitions only after the development of inferential statistics in the

1930s and their creation of a form of pseudo-positivism (operationalism). The behaviorists' incorporation of operationalism into their thinking led eventually to the formulation of intervening variables and hypothetical constructs. The motivation behind the development of those constructs was to permit prediction while taking unobservable factors into account (the behaviorists fully accepted the logical positivist symmetry between understanding and prediction). For Hull, the most influential behaviorist theorist, social goals remained paramount, even if they had to be set aside in the interests of creating a believable theoretical structure. More to the point, Hull's approach to theory and the benefits he expected from theory were modeled on the social structures of the American corporate boardroom of his day. Following his excursion into pure research as a young man, Skinner returned to hew the pure Watsonian line. His scorn for theory was matched by an apparent ability to demonstrate that his approach to empirical research yielded limitless practical applications. As so often, Tolman stood on the bank of the behaviorist mainstream. Although he was firmly convinced that behaviorism could eventually yield an applied harvest and although he was a social activist in the private sphere, Tolman was not willing to endanger or dilute the theoretical enterprise by rushing precipitately into the applied field.

The sixth defining feature of behaviorism is very complex and will be discussed again at several points in the book. Although, eventually, behaviorist theories were derived from empirical work in the animal laboratory, the behaviorists' approach to their subjects was distinctively different from that of other animal scientists.[12] First, their data were almost exclusively derived from a very narrow base—the laboratory behavior of two species of rats (*Rattus rattus* and *Rattus norvegicus*) and one species of pigeon (*Columba livia*)—and characterized by a startling absence of comparative observations. Second, the behaviorists, although they eventually gave lip service to the Darwinian theory of evolution, continued to espouse a disguised form of neo-Lamarckism, which underlay many of the theories of learning developed by the major behaviorists in the 1930s and 1940s. Those theories were animated by the belief that, in every important respect, animal behavior was predictable and controllable by factors that could be manipulated in the laboratory. That belief was subtly linked to another, seldom made manifest, that all the crucial aspects of animal behavior were controlled by learning. There is a continuity between that

belief and the belief, so characteristic of the early years of American social science, that much evolutionary change originated in the mind. The conviction that the mind was plastically subject to environmental influences and could also control its own destiny was consistent with the belief, forming the driving force behind the thinking of American reformers and social scientists of that era, that human behavior could be shaped to fit social goals by those who understood the nature of those goals and the means of achieving them. It is abundantly clear that some behaviorists (Skinner is the clearest example) were cast in the same mold as their forerunners. Animals were surrogates for human beings, the laboratory and its apparatus were the analogues of social situations, and the experimenter/theorist was the social controller.

The seventh defining feature of behaviorism is a commitment to an extreme form of utilitarianism whereby both values and personal characteristics were seen in strictly functional and instrumental terms. In effect, the person was treated as the physical locus of a set of abstract but operationally definable attributes whose sole function was to promote adaptation to immediate social circumstances. Consistently, values were defined in terms of a particularly stark instrumentalism. The good was that which aided the person or animal to attain certain physicalistically defined objectives (the gaining of food, the securing of shelter, the maintenance of an optimal level of anxiety, and so on).[13] Right was defined in terms of that which yielded immediate personal advantages (like Jeremy Bentham, the behaviorists handled altruism by saying that frequently it was beneficial to defer immediate gratification). "Ought" referred to that which had to be done in order to adapt. Beauty, if dealt with at all, referred to arrangements (of sounds, color patches, objects in space, etc.) that yielded human gratification. Personal relationships were seen solely in instrumental terms and not as ultimate objects of value. Given that the behaviorists' final goal was to demonstrate that their theories applied to all aspects of human life, it is noteworthy that overt treatment of values, morals, and persons was rare in their writings. Even Skinner, who frequently addressed these issues, did not do so in any seriously systematic way.[14]

Although a particular conceptualization of experimentation is, in my view, one of the defining features of behaviorism, a discussion of that

topic merits separate treatment, both because of its complexity and because the behaviorist approach came to be shared by all American psychologists until very recently. That approach is characterized by certain attitudes toward quantification and the role and nature of experiments that are surprisingly hard to portray but that rigidly determined the conduct of research. Broadly speaking, mensuration was placed at the center of the scientific enterprise. Only that which could be counted or measured was worthy of consideration as a scientific fact. The lust for quantification reached its apogee in Hull's theories but was centrally important in all versions of behaviorism. Along with the high value placed on quantification we find not only a belief that experimentation is the only sure and safe way to garner facts, but also an approach to experimentation that those outside psychology must find curious.

That approach, which pervaded American psychology, was articulated by Watson as follows: "we may say that the goal of psychological study is the *ascertaining of such data and laws that, given the stimulus, psychology can predict what the response will be; or, on the other hand, given the response, it can specify the nature of the effective stimulus*."[15] Watson's statement did not merely place prediction rather than understanding at the center of the scientific enterprise. It gave psychologists a crude but very clear blueprint that continues to control the conduct of much psychological research. If one takes Watson seriously, one has to ask what steps we must follow in order to achieve prediction. First, the stimulus itself and all its effects must be made manifest. Second, all the causes of the response and every feature of the response must be open to inspection. Third, each graded increase or decrease in intensity of the stimulus must be reflected in corresponding levels of response intensity. Given the treatment accorded to prediction, the need not just for quantification but for a particular type of quantification became inevitable. This form of quantification in turn controlled both the approach to experimentation and the role assigned to experimentation relative to other forms of fact-gathering.

The exclusive focus not merely on prediction but on making the whole predictive enterprise manifest necessitated a very clear distinction between causes (independent variables) and effects (dependent variables). The obsession with prediction and control yielded a need to separate out the various independent variables. The variation of

each in turn and the study of their effects on isolated dependent variables were deemed necessary parts of experimental procedure.

Because contemporary Anglo-American psychologists treat what I have called the behaviorist approach to experimentation as natural and as the sole available approach, it is vital to recognize that it was a construction whose history can be traced in some detail. Andrew Winston has shown that the first formal statement of the approach was in the second edition of Robert Sessions Woodworth's (1869–1962) highly influential text *Experimental Psychology* (the first edition was published in 1938 and the second in 1954).[16] Although Woodworth was not a behaviorist, he elevated experimentation to the highest position in the hierarchy of fact-gathering devices. He published his blueprint for the conduct of experiments at the very time when operationism (the doctrine that concepts should be defined in terms of the processes whereby they are made manifest) had seized the imagination of experimental psychologists. A psychological concept was defined operationally for the first time in the second paragraph of Skinner's Ph.D. thesis, presented in 1931.[17] Skinner's espousal of operational definitions was followed a few years later by two influential articles by Harvard psychologist S. S. Stevens.[18] Thereafter, the concept was rapidly incorporated into American psychology.

Woodworth's proposals concerning experimentation, as expanded by the behaviorists, posed many more conceptual problems for psychologists than they would for a physical scientist. Even in the case of very simple animals, inner, unobservable factors control behavior much of the time. American psychologists, with the behaviorists in the vanguard, eventually dealt with the problem by posing and answering the question, "In principle, what characteristics would unobservable psychological events have to possess if they were to be observable?" It was assumed that the unobserved factors intervened between observed stimulus factors and observed response factors. It was also assumed that the nature of inner events could be fully understood if one could tease them apart into conceptually distinct components and define each of those components in terms of the operations required to make each of them demonstrate its specific effects.

Contemporary psychologists, who have been so thoroughly schooled in this overall approach to experimentation, do not appreciate the crippling limitations it imposes on their ability to generate and explain psychological data. Above all, it defeats the objective that psy-

chology sets for itself, the explanation of behavior. I can illustrate what I mean by discussing one of the classical paradigms in experimental psychology, Pavlovian conditioning. First, the investigator makes the decision to limit her observations to one act (for example, if the experimental animals are dogs, the act might be salivation). Second, and crucially, the observations are quantitative (amount of salivation, latency of the response, response amplitude, probability that the response will occur, etc.). Third, the observations are collected under strictly controlled conditions. The measures of response strength are assigned to what is called the dependent variable, while the conditions under which observations are collected are assigned to the independent variable. In a typical experiment following the Pavlovian model, an investigator might plot the increasing strength of a response as a function of successive trials. In different experiments the animals used might differ (e.g., dogs in one, rabbits in another), the response might differ (e.g., salivation in one, the eye-blink response in another), the operational definition of response strength might differ (e.g., latency in one, amplitude in another), and the range of trials might differ (some responses take longer to acquire than others). But investigators typically find that response strength is an S-shaped function of level of practice.

Even today, if one asked most experimental psychologists to explain the result (that is, to state what causes the response curve to follow a certain time course), they would answer by essentially describing the typical result. They would say that response strength grows as a consequence of reinforced practice. In that account, reinforcement is being granted causal status. But the word "reinforcement," at least in these experiments, merely describes the procedure that the experimenter followed. So the statement "Response strength increases as a function of reinforced practice" should be interpreted as "When an experimenter decides to limit his attention to certain responses and to elicit these in conditions that are totally under his control, then what he has decided to call 'response strength' increases as a function of the experimenter-instantiated conditions." The experiment alone tells us nothing about the causal efficacy of reinforcement (that is, what it is about reinforcement that makes it causally effective). Above all, it tells us nothing about what is going on inside the experimental animals.

I am not saying that psychologists ignore causes, mental states, or brain processes. In the field of animal learning, people have speculated

about the processes underlying conditioning from Pavlov's time onward. Those speculations, however, did not emerge solely from experimentation. One can certainly devise experiments to test deductions derived from some theory or to falsify another theory. But experiments do not, in the first instance, produce knowledge.

The approach to experimentation I have outlined borders on the nonscientific. In order to see why, let us consider a piece of research in the field of animal behavior and see how it contrasts with the behaviorist approach. Konrad Lorenz's elucidation of innate releasing mechanisms (IRMs) in lower animals had its origins in his study of egg-rolling in greylag geese.[19] Lorenz's conclusions were based on careful observations, combined with minor experimental manipulations. More to the point, his first step was the development of a model in which he differentiated between the IRM proper and the supporting reflexes, going on to demonstrate that egg-rolling and the supporting reflexes were controlled by quite distinct physiological mechanisms. Having developed his model, he selected a species and a mode of behavior that would allow him to collect the data he needed to verify the reality of the model. Only after he had determined the way greylag geese actually egg-rolled did Lorenz start to experiment. It is vital to note that the role of his experiments was not to discover the nature of egg-rolling (his observations had already done that) but to discover the range of sizes, surface textures, and shapes of objects eliciting the response. The study began where it had ended—with further comparative observations. The function of those was to explain the adaptive role of both egg-rolling and other similar instinctive behaviors. Although thoroughly stereotyped, they are extremely adaptive in natural habitats.

To contrast Lorenz's and the behaviorists' approach, after reading Lorenz you have the feeling that you know what an IRM does and what its adaptive function is. Above all, note that Lorenz discovered that lower animals operate in a machine-like, stereotyped fashion, but by using detailed observations he was also able to demonstrate why, in natural habitats, their behavior seems to be purposive and controlled by human-like factors such as maternal love and solicitude. In a typical behaviorist experiment, the explanation lurks uneasily in the shadows. Besides being vague, behaviorist explanation is circular and deductive. A system of operationalized variables allows one to explain only if one assumes that one is dealing with some sort of mechanism.

In many areas in psychology, it is legitimate to assume that one is dealing with a mechanism (or a system that can be fully understood in machine-like terms). For example, leading researchers in the field of visual perception can explain pattern and form recognition very convincingly by using computer simulation models.[20] A higher-level theory can then, in principle, enable us to incorporate those conclusions into our overall understanding of human beings.

A central point to be grasped is that a great deal of work in contemporary cognitive psychology is devoted to the explanation of how individual minds function. Following psychologist Kurt Danziger's analysis, we can say that cognitive psychologists are trying to create a compromise between the behaviorist and a prebehaviorist psychological paradigm. Danziger maintains that historically, psychology has consisted of a family of paradigms united in a purely nominal sense.[21] Each paradigm had its own way of defining what constituted data, determining what methods should be used to collect data, defining the role and nature of sources of data (that is, idiosyncratic ways of treating minds, persons, or individual dispositional/action systems), dealing with the role and nature of observers, and treating the mind/body problem.

By the 1930s, impelled by impatience with philosophical questions and by their pragmatic weltanschauung, American psychologists had created what Danziger has called the neo-Galtonian model of research. In this approach, the individual was treated merely as the carrier of some variable or variables of interest, and no prior assumptions were made about the mode of action of these variables within the individual. Moreover, the neo-Galtonian model demanded that one should deal with groups, not individuals. The defining feature of the neo-Galtonian model is the use of treatment groups. Experimenters manipulate all individuals in the group in the same way so that the experimenters, rather than the individuals selected for experimentation, become the causal agents. The problem with the neo-Galtonian approach is that it creates sets of purely functional relationships between experimental manipulations and behavioral effects. In a pure neo-Galtonian model, findings consistent with experimental hypotheses would demonstrate only that one's assumptions were logically tenable, not that they were even provisionally true.

Psychologist Tim Rogers has shown us how the behaviorists broke the impasse for their colleagues.[22] The first types of operational defi-

nition proposed by psychologists were thoroughly consistent with the neo-Galtonian model. Psychological concepts were defined in terms of the operations required to make the relevant behavior manifest. Independent variables could then be defined in terms of standardized experimental manipulations and dependent variables in terms of selected behavioral observations. Skinner invented a new type of operationalism whereby a construct was defined in terms not of the operations whereby it made itself manifest but in terms of the operations required to produce it, thereby shifting the focus from nature to the laboratory and from naturally occurring behavior to experimentally induced behavior. A good example of such an operational definition is hunger, typically defined in terms of the procedures followed when reducing the body weight of rats or pigeons to 80 percent of their free-feeding level. As early as 1944, Israel and Goldstein pointed out that Skinner had departed significantly from the original purpose and nature of operational definitions.[23] The main purpose of operational definitions was to inhibit people from engaging in fruitless debates about the true essence of the concepts they were using as working scientists. But defining concepts such as atom, electron, or neutron operationally did not absolve physicists from the obligation to understand how those entities functioned in the natural world. A set of causal explanations derived by experiment must ultimately be assessed against happenings in the world of nature.

Skinner's approach, however, "solved" the problem of the relationship between laboratory-induced and real-life behavior by fiat. His form of operationism, which we can call productive operationism, is most effective when applied to intervening variables, such as hunger or thirst. If we define them operationally, we do not need to appeal to inner states as explanations (hunger, for example, becomes what the experimenter induces, not what the animal feels). For many years, the increasing sophistication and success of the behaviorist experimental procedures blinded psychologists to the logical and empirical flaws inherent in productive operationism.

These flaws emerged particularly strongly in the case of drive theory, the most fruitful application of productive operationism. Drive theory assumed that all operational definitions of the same drive were convergent. It fairly soon became apparent that the assumption was not true. Thus, thirst induced in different ways (by depriving animals of water, by giving animals saline solutions, or by feeding them dry

food, for example) has differing behavioral effects.[24] So animal biologists have turned to the concept of central motive states.[25] Drive theorists limited themselves to states induced in the laboratory, whereas central motive state theorists deal with states occurring in nature or in very simple experimental situations. The assumptions of the theory were simple and, in principle, empirically verifiable (for example, that any given behavioral disposition temporarily "captures" an animal's entire response system and that each disposition has some definable and observable behavioral manifestation). Central motive state theory is robust enough to allow ethologists to make very precise predictions of the behavior of a wide range of species.[26]

In contrast, drive theory encountered a series of embarrassing failures when experimenters tried to use procedures more complex than those used in the laboratories of the 1940s and 1950s or to extend their work to species other than rats or pigeons. Very frequently, instead of motivating their subjects to perform some experimental task, these investigators induced instinctive drift or adjunctive behavior. Those behaviors were later interpreted as displacement activities induced by stress.[27] Behaviorist theorists of animal learning found themselves in difficulty because of the logical flaw in productive operationism. To take the case of hunger drive, in a simple laboratory experiment with a widely used species it seems to be self-evidently true that reducing free-feeding body weight induces hunger and nothing but hunger. But we accept the validity of the behaviorist claim only on the basis of concealed anthropomorphic premises, not on the basis of some empirical check. The implicit argument on which the behaviorist case rests is "If I reduce the body weight or limit the number of daily meals in a human being then I induce hunger. But I have reduced the food intake of my experimental animals. Therefore, I have induced hunger in my experimental animals." The problem is that unless one has independently verified that the procedure has indeed induced hunger, it does not follow that the conclusion is necessarily true. The procedures could have produced other states in addition to hunger (such as frustration), or repeated exposure to the same operation in particular animals could produce increasing tolerance of hunger. Of course, the required independent checks could have been carried out, but behaviorists did not do so.

Behaviorism has certainly had its successes in the field of animal behavior. But it is essential to realize that in the case of the paradigmatic

behaviorist technique (operant conditioning) we have to recognize, on nonbehaviorist grounds, that we are dealing with a system controlled by response feedback. Once that has been established, a wide array of behaviorist techniques is at the disposal of physiological psychologists or psychopharmacologists.[28] In contrast, behaviorism unconstrained by theory has seriously misled animal psychologists.[29]

In the human domain, operational definitions were first applied to the concept of intelligence, but their use rapidly spread to other areas.[30] As in the case of animal work, the overt purpose was to provide psychologists with agreed sets of definitions for their concepts. As in the case of the animal area again, productive operational definitions proliferated. Constructs such as anxiety, nurturance, cognitive dissonance, or need achievement were defined in terms of the operations required to generate instances of them in groups of experimental subjects. Underlying the overt purpose were two major covert aims. The first was a subtle shift whereby the new or "scientific" meanings of the constructs were subordinated to the requirements of the treatment group approach. From the standpoint of common sense, the causal efficacy and experienced qualities of anxiety or cognitive dissonance lie within the individual. When the concepts are redefined in terms of experimental operations, the locus of control is shifted from the individual experiencing the state to the experimenter. At the same time, there is sufficient overlap between the "scientific" and the commonsense meanings to render the findings of psychologists comprehensible to the lay public.

The second concealed purpose was the introduction of what are in effect mechanistic explanations. To take a very simple example, an investigator who defines maternal bonding operationally (in terms of hours spent by mothers with their babies, hours spent vocalizing to their babies, proportion of time spent smiling at their babies, and so on), who defines child/mother love operationally (in terms of numbers of times the child uses a term of endearment, number of occasions per observation session in which the child caresses the mother, and so on), and who finds a functional relationship between the two variables will typically conclude that bonding, as operationally defined, has a causal influence on child/mother love, as operationally defined. Underpinning almost all such work in the fields of personality, abnormal, social, and developmental psychology is adherence to a version of positivism in which the investigative enterprise is supported by a con-

cealed and unarticulated belief in machine-like forces. Bonding, need-achievement, the various forms of depression, or the various forms of anxiety supposedly drive individuals and force them to engage in various behaviors. For example, in the days before codes of ethics proliferated, psychologists had at their disposal an array of techniques for inducing anxiety. It was assumed that those techniques that produced an experimental effect would induce the same behavioral disposition in all the subjects in the treatment group. Mere common sense should have told experimental psychologists that, without some sort of independent check, the conclusion was unwarranted. A given procedure could conceivably have been ineffective for some subjects, induced amusement in others, hostility in yet others, and so on. Many psychologists might reply that intersubject variability would express itself as statistical error, whereas intersubject consistency would express itself in the treatment effect. The problem with that argument is that the treatment effect in this type of work is typically extremely small, so that at best, the induced states account for a very small proportion of the variance.[31] Moreover, one cannot discount the effects of compliance with the perceived aims of the experimenter.

Even if those difficulties are overcome, experimenters in many areas of psychology have to meet the challenge posed by Jan Smedslund, who argues that most psychological research can be interpreted in terms of the psychological language of common sense and that psychologists should relinquish their causal explanations.[32] Smedslund's arguments have considerable force when applied to the "findings" of those working with human beings. To return to my example of maternal bonding, the very use of the term "bonding" automatically implies two or more elements to be bonded. Given that understanding, it follows that bonding must be reciprocal. If a mother has strongly bonded with her child and the child does not love her in return, we ask what it is about the maternal love that has induced the adverse reaction in the child (is the mother dutiful but cold, is the love she proffers smothering rather than nurturing, and so on). Smedslund would argue that in such cases we base our conclusions primarily on a scrutiny of the meaning of the explanatory terms we are using (a part of the meaning of smothering love, for example, is that it induces ambivalence and a need to escape in the child). If we define our terms operationally and then establish that smothering love induces ambivalence and incipient withdrawal, then Smedslund would say that we

have done no more than needlessly confirm what had already been established by a semantic analysis of the language of common sense.

Given the logical problems inherent in the use of productive operational definitions, we can ask why their use became not just widespread in psychology but an integral part of the nature of empirical research in the discipline. The answer lies, I believe, in the beginnings of research practices in the United States in the first two decades of the twentieth century. Psychologists were asked to prove their usefulness in the field of mental testing. The primary difficulty was that the nature of the causal factors, intelligence in particular, was completely unknown. The solution to that difficulty was to devise very crude operational definitions.[33] The other difficulty was that close study of individuals in rigorously controlled research settings would not have allowed American psychologists to meet the obligations imposed on them by society in general and industry in particular. The solution to the problem was, once again, conceptual. American psychologists invented the neo-Galtonian research method, an integral part of which was the concept of the treatment group. In the neo-Galtonian approach, the individual became the interactive locus of a number of independent variables, each of which could be manipulated in isolation from the others. Control, as I have already said, was effectively removed from the individual subject and assigned to the investigator. The investigator, in his turn, became a manipulator because, given the equation of science with technology current both within and outside psychology at the time, knowledge was assumed to be derived from action rather than understanding or contemplation, and only those manipulations that had some likelihood of producing socially useful consequences were considered worthwhile. During the rest of the century, psychologists invented more and more precise and more and more sophisticated techniques of manipulation; elegant statistical techniques, especially the various forms of analysis of variance and the various versions of factor analysis, were at their disposal; and they were able to propound their theories and discuss their empirical findings in the language of logical positivism. But I sense an unease, usually expressed in immoderately defensive statements, about this entire enterprise.[34]

The behaviorism described above was the product of the social and institutional context in which American psychology grew and matured. My examination of it begins in chapter 1 with a brief overview

of the Progressive movement, concentrating on the symbiotic relationship between Progressivism and early American social science. Both the Progressives and the social scientists believed that science should serve the good of society, where good was defined primarily in terms of material comforts and success. Both groups also believed that it was possible to develop social technologies to shape human beings to serve the ends of society, as defined by an elite with access to objective knowledge of the ultimate purposes of society. Those purposes were defined in terms of a conceptually incoherent but ideologically unified and powerful set of doctrines that are best called "evolutionary naturalism."[35] Initially, American intellectuals were Lamarckian, but Lamarckism was slowly replaced by a particular version of neo-Darwinism in which Darwinian language and concepts were subverted by a continuing and implicit retention of Lamarckian notions. A functional theory of causation and an atomization of the person were also characteristic of American social science from this early period. Ultimately the emerging social sciences in America derived their formative characteristics from Progressivism.

The chapter will continue with an account of the connections between the philosophical doctrine of critical realism and behaviorism. The critical realists, such as Edwin Bissell Holt (1873–1946) and Ralph Barton Perry (1876–1933), developed a distinctive theory of mind that denied that minds had some special status in the universe. They claimed that a moment of consciousness was a physical event whose dominant characteristic was its connection in time with other similar physical events. There were therefore no such creatures as conscious agents. The similarities between critical realism and behaviorism are obvious. Moreover, some of the critical realists and those who developed similar doctrines had direct connections with behaviorists. For example, Perry was one of Tolman's teachers, and Guthrie's doctoral thesis was supervised by Edgar Arthur Singer, Jr. (1873–1954), a philosopher whose views were very similar to those of the New Realists. The bulk of the chapter will consist of an account of what Erwin Esper has called "the great war of words": the speculative behaviorism of the 1920s and its critical response. On the whole, the proponents of this behaviorism were marginalized. The leading behaviorist psychologists of this era (apart from Watson, who deserves his own chapter) were Max Frederick Meyer (1873–1967), Albert Paul Weiss (1879–1931), Jacob Robert Kantor (1888–1984), and Walter Samuel

Hunter (1889–1956). Each devised a unique form of behaviorism; all exerted almost no subsequent influence.

Chapter 2 provides an intensive analysis of Watson's influence, starting with an appraisal of his work as an animal psychologist and a search for the origins of his behaviorism in his popular articles and in his correspondence with his friend Robert Yerkes. I will then analyze the text of his 1913 article in *Psychological Review* and discuss Franz Samuelson's analysis of the repsonses to it. By the 1930s Watson's appeal had faded and behaviorism was in retreat throughout the social sciences. Many psychologists felt that behaviorism had failed to live up to its early promise and was about to become a part of psychology's history.

Chapter 3 will establish the background for a study of the neobehaviorist theories that dominated American psychology in the 1950s and 1960s. The chapter will chart the rise of operationism. By describing the connections between operationism, the behaviorist philosophy of experimental design, and the statistical technique of analysis of variance, I will demonstrate how the newly emerging speciality of learning theory emerged and how it shaped neobehaviorism.

Chapter 4 deals with Clark Hull's theory of behavior. Hull created the only theory in the history of psychology that gave a comprehensive and formal explanation for all behavior, whether animal or human, whether individual or social, and whether normal or abnormal. An analysis of Hull's diaries, his unpublished seminar notes, and his correspondence with Kenneth W. Spence (1907–1967) will demonstrate that beneath the published writings lay an unarticulated biomechanistic theory. That theory, in turn, has to be set in the context of an even less articulate set of views about human nature and the nature of society. Hull's views about the nature of scientific theorizing led him to a particularly stark version of instrumentalism, derived at several removes from his forerunners among American social theorists. His lifework is best interpreted as a prolonged metaphor for his beliefs about the workings of society.

Chapter 5 is devoted to Skinner's theory of mind, his philosophy of science, his utopian social philosophy, and his theory of value. Despite the seeming crudity of his prose, Skinner's position in all those cases was sophisticated and, up to a point, defensible (as demonstrated by the size of the secondary literature his views have generated). In the case of his theory of value, Skinner's novel *Walden Two* is the crucial

text. It expresses certain fundamental views about human nature that he imbibed as a young man. Skinner's work is a portrayal, in a seemingly scientific form, of the beliefs about human nature and its place in society held by American intellectuals in the early years of this century.

In chapter 6 I will assess behaviorism's role both as creator and creature of intellectual patterns, societal practices and purposes, and values in America. In order to focus my discussion, I will limit myself to a historical account of the rise and demise of behavior modification, behaviorism's version of psychotherapy. Any full discussion of behavior modification must take account of its scientism, pragmatism, and empiricism. More to the point, I will also deal with the implicit theory of society underlying it. Like their Progressive forebears, the leaders of the behavior modification movement did not merely invent an array of social technologies; they also had a technological view of human nature. Such a view, however, had to compete with others and ultimately fell victim to them.

The chapter will open with an account of the promising developments of the 1920s in the work of such people as Watson, Mary Cover Jones, and William Burnham, moving on to discuss the reasons for the discontinuity between that movement and the emergence of modern behavior modification in the work of Leonard Krasner and others. Their achievements were possible only because the mental health profession had demonstrated that it could deal effectively and cheaply with stress-induced mental illness among service personnel in World War II. During the period of economic growth following the war, the American government funded the creation of a large and complex mental health profession. Given the drive toward large-scale treatments of short duration and proven efficacy, the behavior modifiers eventually became very prominent in the mental health profession. The chapter will close with an account of the impact of the civil rights movement on behavior modification programs in institutions such as mental hospitals. Civil rights lawyers challenged behavior modifiers to demonstrate that their practices were indeed effective, and were often the victors in that contest. They also limited the constraints that the behavior modifiers could apply to their clients. The picture is further complicated by the interaction between the outcomes of the numerous court cases brought by the civil rights movement and the decreasing availability of government funding, starting in the 1970s.

Both resulted in a move to deinstitutionalize the mentally ill, thus robbing the behavior modifiers of much of their clientele. By coincidence, behavior modification's death knell was sounded at the same time as the weight of negative evidence from animal studies brought about the demise of neobehaviorism in academic psychology.

In the 1950s and 1960s, neobehaviorist theorizing and research practices dominated American psychology's subject matter, and that neobehaviorist hegemony forms the subject matter of chapter 7. Neobehaviorists exerted control both intellectually and institutionally. They exercised intellectual control by excluding some parts of psychology (such as perception, thought, and language) from serious consideration and by placing others (especially learning and motivation) in the forefront. The career of Kenneth Spence, Hull's leading acolyte, provides us with the most conspicuous example of institutional control. Besides making formidable contributions to neobehaviorist theory in his own right, he graduated seventy-two doctoral students from Iowa State University. All were carefully schooled in the neobehaviorist habits of thought. Many had exceedingly fruitful careers themselves, and all, with trivial exceptions, remained faithful to the neobehaviorist credo to the end of their careers. Today we still find both behaviorists and neobehaviorists thinly scattered through psychology. However, they lack their former prominence because behaviorism was born in a time of social optimism, rose to its apogee during a period of unprecedented economic prosperity, and collapsed into a group of obscure sects during the current neoconservative era.