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*The Cult of Empiricism in Psychology, and Beyond**

STEPHEN TOULMIN

DAVID E. LEARY

At some stage in its development, any field of intellectual discussion or scientific speculation may reach a point at which it begins to generate large numbers of “empirical” questions, that is, questions whose answers must refer to carefully documented observations, or even to controlled experiments. In physics, this happened most strikingly in the course of the seventeenth century; in biology, the comparable stage was not reached until around 1770, rising to its peak in the course of the nineteenth century (Toulmin, 1972; Toulmin & Goodfield, 1962); whereas in psychology, it has become customary—though a trifle arbitrary—to argue that this happened just one hundred years ago, with the establishment of Wilhelm Wundt’s pioneer psychological laboratory in Leipzig in 1879.

There need be no serious objection to saying this, on one condition. If we speak of Wundt and 1879 as defining the moment at which scientific psychology became a genuinely “empirical” science, we must take care to talk about Wilhelm Wundt himself, as he existed historically in late nineteenth-century Germany, and not

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about the Wundtian heritage that has developed during the subsequent hundred years, particularly in the United States. For, in some significant respects, the intellectual constraints that American psychologists have tended to place upon themselves since Wundt—in the name of “empiricism”—have been far more rigorous and exacting than any which Wundt himself intended, and also far more rigorous and exacting than was ever the case in other fledgling sciences. Neither in physics nor in biology did the introduction of experimental procedures lead scientists to cut their diplomatic relations with the larger philosophical debates out of which their newly defined “empirical” questions had emerged and from which those questions had acquired their original meaning. On the contrary: in both sciences, an elaborate analytical and theoretical debate continued for most of a century after the onset of this new “empirical” phase of investigation. These debates were concerned with two groups of theoretical issues carried over from the preceding period. On the one hand, there were conceptual discussions about the fundamental nature of matter and force (or life and adaptation), discussions that had already been under way before the beginnings of the new empirical physics and biology; and on the other hand, there were discussions about the intellectual relevance of the new empirical investigations to the larger theoretical issues from which they had developed. These latter discussions were aimed, in particular, at clarifying the question, “What is an empirical science of physics or biology, and what should it become?”

Wundt’s Dual Program for Psychology and Its Bifurcation

If we look at Wundt’s own case, we find just the same sort of thing happening once again in psychology. The establishment of Wundt’s new laboratory was not intended to inaugurate a separate, autonomous field of experimental psychology, independent of all other subjects (Leary, 1979; Métraux, 1980). Rather, Wundt saw it as contributing toward *one* legitimate research program, among others. In Wundt’s own hands, psychological issues initially retained their earlier connections with the logic and epistemology of Immanuel Kant, the physiology of Hermann von Helmholtz, and the psychophysics of Gustav Theodor Fechner (Wundt, 1862, 1863; Richards, 1980; Woodward, 1982); and even after he had defined his narrower research program for the experimental study of “the manifold of consciousness” (Wundt, 1874), he never suggested that this should constitute the entirety of psychology, still less that the results obtained from this new program could be used to build a psychological science free of all more general intellectual connections, either with philosophical arguments or with contemporary theoretical issues in neighboring fields of science. Indeed, Wundt (1880–1883, 1886) went to considerable effort to develop his own systems of logic (including epistemology) and ethics—not to mention his own system of philosophy (1889)—each of which he saw as

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intimately related to, and dependent upon, psychology. Conversely, psychology itself, in his scheme, was *and should be* susceptible to philosophical analysis. The depth of Wundt's belief in this two-way interdependence between psychology and philosophy was perhaps best illustrated in his (1913) vehement opposition to the proposed separation of psychology and philosophy in German universities (see Ash, 1980). According to Wundt, neither empirical observation (or experiment) nor rational analysis alone could constitute true, complete science. Neither psychology nor philosophy could fulfill its task without the other. In taking this stand, Wundt showed that he was clearly aware of the historical dependence of psychology—even the new experimental psychology—upon the discipline of philosophy. (On the indebtedness of the so-called “new psychology” to philosophy, see Leary, 1978, 1979, 1980a, 1980b, 1982.)

To put the point in a word, Wundt was not a “positivist,” though he did share with Ernst Mach (and many others) a view of experience as the primary “given” from which the different natural sciences arrive at their respective subject matters by various distinctive modes of abstraction. Although he was cautious in his metaphysical speculation, he did not shrink from the discussion of the nature of his subject matter, which he construed as consciousness, or the mind. Besides formulating a rather dynamic view of the mind as “actuality,” he also pointed out the practical (as well as intellectual) necessity for a philosophical doctrine about the relation between mind and body: methodological decisions—i.e., day-to-day empirical procedures—are dependent upon such a doctrine. For himself, he preferred and argued for a psychophysical parallelism, at least on a pragmatic level; ultimately, this parallelism reflected Wundt's double-aspect monism (Blumenthal, 1980; Richards, 1980). Still, the point is that Wundt, the “physiological psychologist,” argued on essentially philosophical grounds for an autonomous psychology, i.e., a nonreductionistic psychology; and he saw no other way to argue the point, pro or con.

In addition to his belief in the integral relationship between psychology and philosophy—and in addition to his belief that psychologists should continue to address the epistemological, ethical, and ontological issues that had given rise to psychological science in the first place—Wundt held yet another supposition that helped to define his research program. We are referring to the distinction he made between different aspects of mental life, i.e., between those that do and those that do not lend themselves to investigation by experimental methods. His view was that purely experimental methods were appropriate only to a restricted range of mental activities and phenomena. In particular, he excluded from their scope just about all those aspects of mental life that are nowadays classified under the heading of “higher mental functions.” These other aspects of mental life could be brought within the grasp of a scientific psychology, but only if they were handled in quite different ways. So Wundt conceived a kind of historico-anthropological method for investigating the “higher” modes of mental functioning (see van Hoorn & Verhave, 1980; Leary, 1979). This is best exemplified in Wundt's own later volumes on

Völkerpsychologie (1900–1920), a title that is perhaps best translated as “cultural psychology.”

In Wundt's conception, the subject matter of this nonexperimental cultural psychology included such phenomena as language, myth, custom, social structure, law, culture itself, and history, all of which reflect the higher-level working of the human mind (in contrast to basic sensation and perception, for example). Wundt believed, in brief, that the rigorous demands of experimental control, and the equally compelling demand for phenomenal authenticity, could not be simultaneously met in the study of these higher-level phenomena. To take a single example: in the study of language, one must either forfeit control of experimental subjects' prior experience (and hence, idiosyncratic associations) with natural language or set up a strictly controlled study of previously nonexperienced nonsense language, which is, of course, no real language at all. The same sort of dilemma can be posed for the study of social-group processes, and so on. In each case, Wundt felt, experimentation is doomed to failure. What he proposed in its stead was a careful, historical, and cross-cultural investigation of the various products of higher mental functioning. In addition to mere data-gathering, he argued for the extensive use of rational and comparative analysis as the optimal method for this kind of psychology. (In many ways his analytic method was analogous to reductive analysis of the Kantian sort. Wundt asked, in essence, what must be the nature of the human mind for it to produce these kinds of phenomena.) Although the actual execution of some parts of his program for cultural psychology have been subjected to criticism, it is noteworthy that Wundt's study of language has recently been acknowledged as a significant forerunner of contemporary psycholinguistics (Blumenthal, 1970).

All in all, then, the real Wundt had a more complex and richly stocked mind than one might guess from reading various histories of psychology (e.g., Boring, 1950) or the polemics of Wundt's critics at Würzburg and elsewhere. (For recent revisions of Boring's account of Wundt, see Blumenthal, 1975, 1979; Danziger, 1979, 1980a, 1980b, 1980c; Leary, 1979; Mischel, 1970; Woodward, 1982.) This fact needs to be borne in mind when we consider the influence Wundt had on psychology in various countries, particularly as regards the two branches of his dual program for psychology. In Germany, the United States, Russia, Britain, and even xenophobic France, the work of Wundt was of real importance, but the existing intellectual traditions in each of these countries led psychologists to select out rather different things from the Wundtian corpus for use in their own locales. For the most part, they selected out his experimental psychology (and then, only a strange facsimile of it), leaving his cultural psychology to have an impact, as it did, only on other disciplines, such as linguistics, sociology, anthropology, and even psychoanalysis. (An interesting exception evolved in Russia, where the work of Lev Vygotsky and Alexander Luria has represented both sides of the Wundtian heritage: the experimental and the cultural. See Toulmin, 1978.)

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In the United States, which will be the locus of our concern, this bifurcation of Wundt's psychological program was carried yet another step further. Virtually de facto, in its transatlantic migration the new experimental psychology was divorced from the philosophical context that had originally nurtured its existence. At the same time, it was transplanted into a pragmatic soil that, with each tilling, abided overt philosophical nurturance less and less. Although some Americans, such as William James, agreed with Wundt in espousing a fundamental relation between psychology and philosophy, as a matter of fact their philosophy was such that it tended to discourage (or disguise) the free exercise of the philosophic imagination. And many others simply denied the need for any philosophy—or even any preliminary *thinking*—at all. Edward B. Titchener was not of this persuasion, but his positivistic rendering of Wundt's psychology did much to instigate the movement toward a reductionistic (albeit introspective) empiricism in American psychology (see Danziger, 1979; Leahey, 1981; Tweney & Yachanin, 1980). It was, however, Titchener's friend (and systematic opponent), John B. Watson, who best symbolized the trend toward reductionistic empiricism in the United States, especially as it became aligned with the development of so-called "objective" psychology. As this trend enveloped more and more American psychologists, empiricism ceased to be the legitimate source of creative innovations that it had been in physics two hundred years before and in biology one hundred years before. Instead it became what we shall call a cult.

The Temporary Divorce of Psychology from Philosophy in the United States

Interestingly, Watson's famous behaviorist manifesto (1913) appeared in the same year as Wundt's equally impassioned defense of the alliance between the "new psychology" and philosophy. To be sure, Watson's proclamation was not simply a reaction against Wundt, or even against the derivatives of Wundt's psychology in America. In fact, from an historical point of view, Watson's manifesto was actually only one of a much larger set of factors that contributed to the movement of American psychology toward a simplistic form of empiricism. Many of these were operative well before and after 1913. Although we cannot review them all, we can at least discuss them briefly under several general headings.

First of all, as we said, the national setting itself made a difference in the assimilation and shaping of the new psychological science. Broadly speaking, the practical character of the American temperament, blending at that point in history with the progressivist movement toward political and social reform (Goldman, 1955; Wiebe, 1967), provided a context within which American psychologists were called upon, and willingly offered, to apply their new-found methods to the solu-

tion of real-life problems. Even formerly intransigent "pure" psychologists such as the immigrant Hugo Münsterberg (1908, 1909a, 1909b, 1913, 1914) were converted to, and developed, the various fields of applied psychology. This trend toward the practical drew American psychologists away from the former intellectual concerns of their field, so much so by the 1920s that it has been argued (O'Donnell, 1979a) that the first (1929) edition of E. G. Boring's classic history text was written as a defensive reaction against the threat of total encroachment (even within academia) by "professional" psychology. At any rate, it is clear that Watson's promise of an "objective" science of psychology that would lead (in short order) to the prediction and control of behavior was precisely what the reformers, politicians, and businessmen-trustees wanted to hear (Burnham, 1968; O'Donnell, 1979b). The fit between perceived social needs and the goals of applied, and particularly behavioristic, psychology was fortunate, to say the least.

Within this context, it is not surprising that the apparently successful applications of psychology accelerated the trend away from a conceptually rich interdisciplinary psychology (see Crennan & Kingsbury, 1923). Indeed, even some of the less valid applications, as in the army intelligence testing program during World War I (Samelson, 1979), passed for major achievements which buttressed the legitimacy of the new profession. In order to understand the emergence of the cult of empiricism, it is necessary to acknowledge the imagined as well as the real accomplishments of the first generation of applied psychologists.

Meanwhile, at the same time that psychology was moving away from philosophy under its own momentum, American philosophy itself reinforced psychology's trajectory. Although this is rarely appreciated, it was *not* generally the case (Harvard being a notable exception) that psychologists had to fight their way out of philosophy departments, and thus away from their past associations with philosophy. As often as not, philosophers were in the vanguard of the movement calling for an autonomous psychology. Sometimes crying "psychologism," they argued that the alliance between psychology and philosophy had to be broken, for the sake of philosophy as much as psychology. There were clearly economic motives involved in this campaign, but there were also intellectual reasons (Leary, 1979). Increasingly, around the turn of the century, philosophers became aware that their goals, methods, and interests were not always coextensive with those of psychologists. As a result, in 1901 they founded the American Philosophical Association as a splinter group from the American Psychological Association, which had served as a professional organization for both psychologists and philosophers since its inception in 1892. Even after 1901, the psychologists and philosophers continued for a time to meet together. (In many cases, of course, the psychologist and philosopher was one and the same person.) But over the next decade the connections dwindled. Still, the point is that the numerous debates that were held in the early 1900s regarding the proper affiliation of psychology—whether it should maintain its traditional relation to philosophy or become one of the natural sciences—helped to set the scene for

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Watson's later proclamation (see Smith, 1981). Watson was no more the first to call for psychology's independence from philosophy than he was the first to call for objective methods and the extirpation of subjectivist terminology from psychology.

In fact, a select group of philosophers foreshadowed and influenced subsequent developments in this latter respect. Building upon the earlier pragmatism of C. S. Peirce and William James, neorealists such as Edwin B. Holt and Ralph Barton Perry called for an objectivistic interpretation of sensation, perception, consciousness, cognition, behavior, purpose, and so on (see Holt et al., 1910, 1912). Their philosophical orientation had a very profound influence, for example, upon the thinking of Edward C. Tolman (see Smith, 1982). Although their stance was not entirely antimetaphysical, their works (and those of other neorealist philosophers) contained antispeculative undertones. Contemporaneously, another philosopher, E. A. Singer (1911), anticipated the main emphases of Watsonian behaviorism, directly influencing the career choice of Edwin Guthrie (Smith, 1981, pp. 34–35). It is probable that acquaintance with the works of such philosophers predisposed some of the more philosophically inclined psychologists toward objective modes of analysis. Together with the later development of critical realism and neopragmatism, their views would have reinforced the movement toward an empiricistic psychology.

Of course, some American psychologists simply had no taste for philosophical discourse. Among these were certainly Watson and Edward L. Thorndike, whose unhappy experiences with John Dewey and Josiah Royce, respectively, had left unpleasant associations (Smith, 1981). Following in the footsteps of Charles Darwin, George J. Romanes, and C. Lloyd Morgan—and together with Robert M. Yerkes, among others—they turned away from traditional introspective, human psychology and did pioneer research in the area of animal psychology. This research clearly influenced their later approach to human psychology, serving as a natural prologue to their articulation of a behavioral point of view. In addition, Watson was influenced by the objectivistic biology of Jacques Loeb, the zoological research of H. S. Jennings, and (after 1913) the reflexology of Ivan Pavlov and Vladimir Bekhterev (Watson, 1936), much as B. F. Skinner was later influenced by William J. Crozier's approach to the study of physiological "conduct" (Skinner, 1979, pp. 44–46). This shift of allegiance from philosophy to the biological sciences did not mean, of course, that they—or any other "objective" psychologists—avoided taking philosophical stances in their work: it was (and is) impossible to avoid such stances. But in their attempts to get out of the orbit of philosophical discourse, these psychologists produced an antiphilosophical rhetoric that came to typify behavioral psychology in the United States. Although many other psychologists were less drastically disaffiliated from philosophy than they, this rhetoric, wedded to the slowly evolving modern psychology-journal style, operated as something of a self-fulfilling prophecy. When psychologists and philosophers began to find themselves in separate departments, speaking different languages and publishing in different

journals, the long-time public marriage of their disciplines was temporarily over. Psychologists had heard the "empiricist message," and they proceeded to emphasize empirical work at the expense of psychological theory (see Bruner & Allport, 1940).

Watson's Empiricistic Program for Psychology and Its Constrictive Influence

Although his so-called "classical" behaviorism did not take over American psychology as instantaneously and completely as sometimes thought (Samelson, 1981), Watson's psychology strongly contributed to the development of the cult of empiricism in American psychology. If the inspiration of his creed came from animal psychology and objectivistic biology, Watson's ultimate model was derived from physics. His resolution was to make psychology as close to experimental physics as he knew how, banishing all subjective appeals to introspectable data and focusing exclusively on public, observable reactions to arbitrary stimuli. Despite his insistence on objective techniques of observation, it is interesting to note that his consequent use of basic terms such as "stimulus" and "response" was anything but precise and consistent, as he himself admitted (Watson, 1919, pp. 9-15). In fact, Watson's continual reliance on mechanical analogies in his published works served as a sort of verbal camouflage for the fact that the empiricism he espoused was more of an idealized prescription than a real description of his own work. This is partly what we mean when we speak of empiricism becoming the object of a cult: it was more praised in omission than honored by commission. This point would be driven further home if we could take the time to analyze Watson's basically ad hoc, metaphorical, extrapolative use of the notion of the reflex and Pavlovian conditioning. Under present constraints, however, we shall simply invite the reader to consider Watson's views (1924, Chap. 12) on the reduction of personality to reflexology, and try to find an adequate evidential basis for his claims.

The postulation of objective technique and the focus (however fuzzy) on stimulus-response relations led naturally enough to the other major characteristics of Watsonian behaviorism—its peripheralism, its emphasis on learning, and its environmentalism (Koch, 1961; 1964, pp. 7-9). Even at the "periphery" of the organism, however, basic activity is not always so easily observed. As a consequence, Watson's assertion that certain "inner" responses (such as thinking) are effectively "represented" by various muscular movements (such as those of the larynx) was far less influential than his attempts to correlate external "stimuli" with subsequent overt physical "responses" (Watson, 1919, Chap. 9; 1924, Chap. 10). Thus, in practice, the classical behaviorist program led toward a radical shrinking of the range of topics for psychological investigation *and theorizing*, despite Watson's avowed intention to account for a broad spectrum of psychological phenomena, including thinking.

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This constriction of the subject matter of psychology was furthered by the behaviorist emphasis on learning and on the influence of the environment. Coming from a background in animal psychology, Watson wanted to focus on the adaptation of the organism to its environment; and because he held certain utopian ideals about the improvement of the social order, he was also interested in the possibility of the prosocial, behavioral adaptation of the organism to an environment manipulated according to human design (see Morawski, 1982). It is not surprising, then, that Watson's psychology became almost entirely a psychology of acquired adaptive responding. But as behaviorism exerted more and more influence on the psychological scene, this emphasis on learning, and on the impact of the environment, became rather too preemptive, and psychologists began to overlook even the basic processes underlying their vaunted empiricism: for several decades, sensation and perception were methodologically taken for granted but theoretically and empirically ignored by the majority of American psychologists. Perhaps nothing illustrates so well the development of a naive, cult-like empiricism as this basically knowing-nothing attitude toward the fundamental processes underlying the acquisition of empirical knowledge.

New Links between Psychology and Philosophy

For some time now, we have been pointing our fingers at Watson's psychology. But, of course, classical behaviorism is something of the past, even if its legacy is not. Not all the evils of subsequent times can be laid directly at its door. The orthodoxy that controlled American psychology from the 1930s until (approximately) the mid-1950s was not Watsonian but "neobehaviorist"; and there were not one, but several forms of this orthodoxy (see Koch, 1959, 1964, 1976). It was under the aegis of these revisionist versions of behaviorism that the cult of empiricism achieved its most complete formulation and its practical dominion over American psychology. What is particularly remarkable, in view of the putative banishment of philosophy from psychology, is the way in which this formulation and domination was effected with the complicity of a certain kind of philosophy. By the 1930s, psychologists had apparently gained enough confidence in their new experimental discipline that they began to listen to philosophers once again, and they liked what they heard: for they listened to a group of philosophers—by and large, the Viennese logical positivists—who were speaking a dialect closely related to their own objectivistic language. That they were attracted to the neopositivist philosophers was natural enough. Given their common empiricist heritage, the behaviorists and logical positivists had a good deal in common. In fact, through the earlier influence of Watson on Bertrand Russell, and Russell's impact on the logical positivists, there was an actual historical link between behaviorism and logical positivism (Smith, 1981). It is not surprising, then, that logical positivism pro-

vided support for the major orientations of the newly developing versions of behaviorism.

Indeed, it is only a slight caricature to represent neobehaviorism as the product of the remarriage of psychology, in the guise of behaviorism, and philosophy, in the guise of logical positivism (Koch, 1961; 1964, p. 12). Although there were internal trends within behaviorism itself, as well as philosophical and methodological trends indigenous to the United States, that led toward the redefinition of the behaviorist program, the confluence of behaviorism and logical positivism constituted the major factor in the development of the orthodoxy that ruled American psychology for at least twenty years, and still remains a tangible, if somewhat less reputable, directive force. For a marriage of limited fertility, and a marriage in which one of the partners (logical positivism) retreated from its vows almost from the beginning, this is truly amazing. In fact, it is difficult to explain the abiding hold of the neopositivist philosophy of science without reference to the cult-like belief that the marriage inspired.

The first step toward understanding this resilient belief in the complex of assumptions propagated by the behaviorist-positivist alliance can be taken by examining the context within which the belief was generated in the first place. By the late 1920s and early 1930s, it had become apparent that the promise of classical behaviorism remained unfulfilled. Rather than solving all the intellectual problems of the discipline and curing the various practical problems of the world, behaviorism had failed even to silence the cacophony of voices within psychology. Although the general objectivistic orientation of behaviorism had gained a wide range of adherents, other "schools of psychology" had continued to develop throughout the 1920s (see Heidebreder, 1933; Murchison, 1926, 1930; Woodworth, 1931). Objectivism in data collection was one thing; agreement about specific modes of objectivism, and about the theoretical implications of "objective" data, was something else. What was needed was some sort of "decision procedure" (see Koch, 1959, p. 371ff.; 1964, p. 9ff.), by which psychologists could reach agreement not only about the specifics of methodological procedure, but also about the appropriateness and definition of theoretical terms and the proper conduct of theoretical discourse. In the midst of apparent conceptual anarchy, even those who had banished speculative, metaphysical theory from psychology decided that psychologists had to adopt some sort of technique for reaching consensus about the meaning of their empirical results.

Within this context, logical positivism satisfied the needs of the moment. First of all, it passed inspection by being as virulently antimetaphysical as the most dogmatic form of behaviorism. At the foundation of the logical positivist program was the belief that all statements are either analytic (and thus tautological), empirical (and thus verified by observation), or meaningless (Carnap, 1932/1959a). As a result, according to the original logical positivist creed, theoretical statements were either logical tautologies, propositions reducible to empirical content, or nonsense. Any theories exceeding (even if not contradicting) the restrictive domain of verified

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observation were consigned to the latter category. This included carefully conjectural considerations of fundamental issues as well as out-and-out metaphysical speculations about the nature of the mind, organism, behavior, and so on—the kind of speculations that typified early (as well as later) theoretical discourse in the natural and biological sciences. Thus, the “new philosophy,” which had modeled itself on a particular conception of science, legitimized (we might have said “sanctified”) the empiricism of “objective” psychology. Psychologists were only too eager to pledge allegiance to this new code since it seemed to provide a program by which they could finally achieve scientific—not to mention philosophical—respectability. It had become only too embarrassing for them to contemplate the empirical state of affairs in their less than unified and progressive discipline.

In corroborating the behaviorists' emphasis on objectivism, logical positivism offered a specific criterion by which the meaning of terms could be empirically defined. “Physicalism,” as the logical positivists defined it, entailed the formulation of a data-base language in which all terms could be defined by reference to intersubjectively observable, physical objects and events. This criterion formalized the behaviorists' own objectivistic leaning and made it prescriptive for all the sciences. As a result, it brought psychology (at least potentially) within the fold of so-called “unified science.” One of the major logical positivists, Rudolf Carnap (1932/1959b), applied the physicalist criterion directly to psychology, admitting that the successful adoption of such a criterion would depend upon the open-mindedness of psychologists who might well have an “emotional resistance” to the prospect of a physicalistic psychology.

The neobehaviorists, of course, had no such emotional reaction. The message of physicalism seemed to corroborate not only their own unsophisticated objectivism, but also the recent proclamation of operationalism by the Harvard physicist Percy W. Bridgman (1927). This operationalism had a substantial impact on American psychologists. Early on, it was reflected in the methodological orientation of E. G. Boring (1933); later, it was given influential formulation by S. S. Stevens (1935a, 1935b, 1939); and under the joint inspiration of neorealist philosophy, it led to the tradition of “intervening variables” begun by E. C. Tolman (1935, 1936) and relied upon by Clark Hull (1943a). Strengthened and justified by its fusion with the meaning-criterion of early logical positivist philosophy of science, the influence of the operationalist approach to the definition of theoretical terms can be seen in American psychology to this very day, despite the criticism and recantations of the former promoters of orthodox logical positivism (Koch, 1964, 1976).

Indeed, by the mid-1930s, Carnap (1936–1937) and other neopositivist philosophers of science (e.g., Hempel, 1935/1949) had begun to realize that their early interpretation of the criterion of meaning, especially as applied to psychology, was unrealistic, and they continued to liberalize the logical positivist position—almost beyond recognition—over the subsequent decades (see Hempel, 1950; Toulmin,

1969, 1977). But as Sigmund Koch (1964, pp. 21–25) has noted, this liberalization was hardly noticed, much less imitated, by the neobehaviorists and their successors, who held (and continue to hold) on to earlier operationalistic conceptions in a manner that can only be described as wish-fulfilling (or fetishistic). As Smith (1981) has pointed out, several of the leading logical positivists have admitted that “progress in psychology has been impeded by the influence of the earlier strict versions of the meaning criterion” (p. 36; see Carnap, 1956, p. 70; Feigl, 1951, pp. 201, 204–205).

The logical positivist creed also offered the neobehaviorists a view of scientific laws that helped psychologists resolve their uncertainty about the structure of theoretical discourse. The logical positivist prescription for a theory appeared in different variations (see Suppe, 1977), but all these variations required that a theory be capable of being “rationally reconstructed” into a deductive form. Working from the top down, scientific theories (in this general conception) must provide a set of statements from which the pertinent empirical phenomena can be deduced. In practical terms, scientific theories should be composed of fundamental laws or hypotheses that are asserted as postulates. From these, lower-level theorems should be deducible by the strict application of the rules of logic and/or mathematics. These theorems should then be testable by experiment (in the course of which scientists should use physicalist [translate: operational] modes of definition). In summary, proper scientific theory should constitute a hypothetico-deductive system.

As is well known, Clark Hull (1943b, 1951, 1952) became the major advocate of this form of theorizing in psychology. Indeed, he made it the supreme goal of psychological science. Without a hypothetico-deductive system of theory, he was convinced, psychology would be no science at all. He believed this, we should point out, despite the evidence of history which attests to an almost unending panoply of “scientific” forms. If Hull used Newtonian science as the prototype of a hypothetico-deductive system, he might just as well have used it, in another of its aspects, as the prototype of an empirico-inductive discipline (see Guerlac, 1965; Schofield, 1970, esp. Chap. 1). His acceptance of the logical positivist prescription of hypothetico-deductive systematization must be seen, on the historical evidence, as the acceptance of a corroboration of a personal viewpoint, not some timeless or necessary truth. Hull’s own bias for logical, hierarchical explanations preceded his contact with logical positivism; indeed—unlike many of the psychologists who inherited his hypothetico-deductive zeal—he did not in his writings appeal to logical positivism for authority, nor is it clear that he was a keen student of the position (Smith, in press).

Through his station as a major teacher and leader at the Yale Institute of Human Relations, as well as through his influential publications, the hypothetico-deductive model achieved near hegemony in the heyday of neobehaviorism. Only when his

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own drastic revisions (Hull, 1951, 1952) of his own hypothetico-deductive system (Hull, 1943b) were published did his espousal of a radically logical, hierarchically structured, unified theory of psychology come to be seen as vastly premature at best (Koch, 1954). But in many ways Hull's philosophy of science endures, in somewhat attenuated form, to the present time—in the many minisystem models that have taken the place of his grand theory, in the prescribed format style for dissertations and journal articles, in the introductory chapters of psychology textbooks, and thus in the general lore of the field. And again, these conceptual straitjackets persist despite all the reactions, and all the developments, in the philosophy of science over the past decades.

There were, of course, some neobehaviorists who escaped the influence of the logical positivists. B. F. Skinner, for instance, took his inspiration from the older positivism of Ernst Mach (see Skinner, 1979, pp. 66–67). But this only means that his is an even sparer form of empiricism. Trying to banish not only metaphysics but also theory from his psychology, Skinner has relied on the most naive form of empiricism imaginable, assuming that the "facts" will speak for themselves. This is perhaps the ultimate expression of the cult of empiricism—of faith in the data. Ironically, such naive empiricism, precisely *because* it disavows its dependence upon the theoretical realm, is all the more likely to be a vehicle of unexamined metaphysical assumptions about the nature of the data, the organism, and the world.

In concluding this discussion of neobehaviorism, it is only proper to acknowledge that not all psychologists in America between the 1930s and 1950s were neobehaviorists. Yet it seems entirely safe to say that the neobehaviorists, particularly Hull, set the dominant tone for this period. Additionally, the general characteristics we have outlined—the antimetaphysical temper, the operationalist approach, and the hypothetico-deductive ideal—governed the activity of the majority of psychologists throughout this period, whether they were behaviorists or not. Clearly, there were exceptions. Egon Brunswik (1943, 1947), for example, rejected the hypothetico-deductive ideal, even though he accepted the other basic tenets of neobehaviorism. But the exceptions proved the rule. Although Brunswik was highly respected, his critique was neither understood nor accepted. Even nonbehaviorists, such as Kurt Lewin (1943), rejected Brunswik's criticism of the dominant prescription. Yet from an historical perspective we can see once again that the dominant prescription was just that: a prescription. Since the publication of the multivolumed *Psychology: A Study of a Science* (Koch, 1959–1963), it has been clear that much of the theorizing that had been going on in psychology had not conformed to the requirements of hypothetico-deductive systematization (see Koch, 1959). Nor for that matter had psychologists really avoided all metaphysical commitments; nor had they always utilized—or even believed—in the lore about operationalism! But this acknowledgment was rather tardy. During the "age of theory" (Koch, 1959), psychologists had not so readily confessed their sins against the god of Empiricism.

The Cult of Empiricism and Its Results

With all the foregoing as historical background, we can now (finally) explore several features of what we are here calling the "cult" of empiricism among American psychologists. Although we have already suggested a number of reasons for referring to a cult of empiricism, we should once and for all justify our deliberate choice of a prejudicial word. To recapitulate and expand our argument: among experimental psychologists in the United States from roughly the 1920s on, a commitment to "empiricism" rapidly became something which (like the doctrine of the Trinity) it was more important to accept than to understand. These experimental psychologists came to regard theoretical questions that were not tied directly to the analysis of "controlled experiments" as self-discrediting: self-discrediting as arguments, since their nonempirical origins were sufficient to demonstrate their emptiness; and self-discrediting even as speech acts, since any psychologist who spent time on such irrelevant matters would be talking evident nonsense and so be professionally unsound. And this was something that had never happened, either in physics after 1650, or in biology after 1780. Newton and Leibniz, Bichat and Müller, Helmholtz and Bernard had all had broad and highly philosophical interests.

Stepping back from our earlier historical survey, we want now to present a conceptual overview and analysis of this cult of empiricism. We shall argue that this cult rested on three distinguishable but connected strands of argument. These had to do (1) with the nature and purpose of "scientific" observations and, more specifically, with the purpose of "controlling" those observations; (2) with the need to limit theoretical hypotheses and constructions to those arising out of, and securely supported by, the results of "controlled observations"; and (3) with the prize that could seemingly be won only by confining oneself to this particular empirical model, viz., universality (which would be expressed, for most of the neobehaviorists, as some sort of law in the upper reaches of a hypothetico-deductive system). Only those who stuck to narrowly controlled experimental procedures (it was believed) could hope to formulate theories guaranteed to apply to all human beings (not to mention animals), regardless of all the diversities associated with cultural variations and historical changes.

Once these three claims are set out separately in this way, it becomes clear why one must speak of this as a cult of "empiricism" rather than of "experimental method." Since the time of Francis Bacon—long before the advent of logical positivism—items 2 and 3, in particular, have been essential elements within the tradition of *philosophical empiricism*. Empiricist philosophers have repeatedly claimed that controlled observations alone, as prescribed under item 2, provide the indispensable means to achieve universalizable results, as specified in item 3. The empiricist formula for establishing universal generalizations has been, precisely, to confine theoretical speculation to the results of controlled observation. Furthermore, while we can distinguish these three elements in the program of American

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experimental psychology in retrospect, they were originally conceived as three aspects of a single program, and in actual practice they worked together. According to this program, as we have seen, it was assumed that psychology could make progress as a science only by taking three closely related steps. First, psychologists had to cut themselves loose from all the verbiage of earlier philosophical speculation; next, they had to create for themselves a theoretical tabula rasa—an empty field or flat conceptual space, awaiting the erection of some vast and disinfected scientific emporium; and, finally, they had to throw up new “logical constructions” de novo, as building material became available from controlled experiments.

In the last fifty years, the discussion among psychologists about “theory construction” (to use a phrase that is unknown in physics and biology) has gone through two successive phases, both of which have been influenced by arguments within neopositivist philosophy of science and inductive logic. In the first place, as we have seen, American experimental psychologists followed the recipes for theory construction put forward by such philosophers as Rudolf Carnap and C. G. Hempel in their debates, e.g., about the legitimacy of using “intervening variables” in psychological explanations and theories. What was called “theory construction” had thus, in actual fact, nothing to do with the older traditions of scientific theorizing as developed by physicists and biologists. Instead, it represented a kind of “logical construction” of more complex propositions out of simpler ones, according to models set out by Gottlob Frege, Bertrand Russell, and their successors in the program of twentieth-century symbolic logic (Toulmin, 1969).

For the time being, it escaped the notice of working psychologists that the positivist account of natural science was a formal logicians’ fiction and had only a very limited application to real-life science. It could not be applied to contemporary physics: Carnap himself (1950), for instance, refused to address questions about quantum mechanics, on the ground that the quantum theory had never been “formulated according to the rigorous standards of modern logic” (p. 243). And it gave an impoverished account of biology: Carl Hempel (1965), for instance, dismissed Darwin’s theory of evolution as not really being “scientific” at all, at least by the formal standards of logical empiricism (p. 370). Since they allied themselves with a particularly narrow and dogmatic school of philosophers, then, it is not surprising if the psychologists’ own results were themselves narrowly dogmatic. In the extreme case, only those theoretical statements that could be “logically constructed” out of observational measurements alone were regarded as “well-founded” in experience; and it became the ambition of some, following Hull’s example, to leap directly to a theory of psychological phenomena as fully axiomatic as Newton’s theory of dynamics in the *Principia*.

Subsequently, from the mid-1950s on, many psychologists became disappointed at the scanty results achieved as a consequence of all this “theory construction” and “axiomatization.” They also wanted to escape from the dominance of the neobehaviorists and began to espouse pluralism in the methods and concepts of

psychology. This was, in its own way, a promising enough move. If their pluralism had been grounded in a proper understanding of the relations between the methodologies of the different branches of psychology—like the understanding that validates, for example, the division of authority in biology between cell biology, general physiology, developmental biology, and evolution theory—all might yet have been well. But that would have meant confronting the problems of psychology head on, and analyzing them in their own terms—i.e., generating a truly *theoretical* account of those problems. Most psychologists were not yet ready to do so. Instead, they were content to go on as before, taking as their guide and authority the outdated words of logicians and philosophers of science, rather than the digested fruit of their own experience. Then, under the influence of T. S. Kuhn's *Structure of Scientific Revolutions* (1962), the move toward pluralism quickly lapsed into a shallow relativism—"You choose your paradigm, and leave me to choose mine." This second phase is, of course, still with us.

Meanwhile, the other two elements in the empiricist program have also generated lasting difficulties of their own. These requirements were (to recall) the necessity to perform only "controlled" experiments and the desirability of achieving "universality" in the resulting generalizations. On the one hand, the recipe for "controlling" psychological experiments was commonly interpreted as requiring the elimination of all external, situational cues by which experimental subjects might be led to attribute "meaning" to the stimuli that were the subject matter of experiments. On the other hand, the goal of "universality" was typically construed as requiring psychologists to begin by abstracting from all possible historical epochs and cultures, with the intention of eliminating all the local or temporary (and so presumably *nonuniversalizable*) influences that are the subject matter of "cultural psychology," or *Völkerpsychologie*.

The first of these demands explains the widespread emphasis, found in much American experimental psychology since the 1920s, on the need for experimental arrays of stimuli or attributes to be purely *arbitrary*—i.e., thought up ad hoc by the experimenter. Only such arbitrary experimental material was supposedly controllable enough to yield "objective" results. Only in this way, it seemed, could experiments be insulated from the extraneous influence of current preexisting "meanings" and associations. Unfortunately, this demand for "culture-free" observations had the effect of destroying the entire "sense" of the material under investigation. In particular, it prevented experimental psychologists from considering how any given body of experimental material related to the larger-scale human enterprises from which it drew its significance in actual practice. A classic illustration is provided by Clark Hull's (1920) study of concept formation, which utilized arbitrary Chinese characters as experimental stimuli. More recent is the account given by W. C. Holz and N. H. Azrin (1966) in their survey article, "Conditioning Human Verbal Behavior." This article attempted to provide an exhaustive account of recent research on the subject, and the authors set out to achieve true "objectivity" by dint of

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rejecting all observations that had not been totally shielded from all extraneous contextual cues. The result of insisting on this demand for absolute objectivity was striking and drastic: Holz and Azrin found themselves with nothing to report aside from experiments on meaningless vocalizations—e.g., the learning of nonsense syllables, the utterances of lunatics, and techniques for improving subjects' capacity to articulate sibilants!

As for the second demand (that experimental psychology should guarantee its own universality by abstracting in advance from all possible cultural differences), this requirement has tended, in practice, to blind experimental psychologists to actual "cultural universals," and these are now proving to be of considerable significance. In this respect, indeed, the novel methods of investigation of actual constancies in human nature developed by the crosscultural psychologists—e.g., Brent Berlin and Paul Kay (1969) and Eleanor Rosch (1973, 1977, 1978) in their studies on color perception and categorization (see Mervis & Rosch, 1981, for a review)—represent a major break with the assumptions underlying much American experimental psychology since the 1920s. If many of the people involved in this novel crosscultural work have come to experimental psychology from cultural anthropology or animal-behavior studies, that is no accident. For the new field of "crosscultural psychology" successfully straddles Wundt's dividing line between "experimental psychology" and "cultural psychology." And this gives us a way of moving not only beyond the narrower limits set by Wundt's American successors but also outside the more generous limits set by Wilhelm Wundt himself.

Beyond Wundt and Beyond the Cult of Empiricism: Toward a More Unified and Expansive Conception of Psychology

Wilhelm Wundt (as we have seen) understood cultural psychology, on the one hand, and experimental psychology, on the other, to be concerned with phenomena that could be distinguished and separated *in advance*. Some human mental activities and functions lent themselves, in Wundt's eyes, to experimental study, while others lent themselves rather to historico-cultural investigation; and, once this division had been clearly made, there was no way of going back on it. By contrast, the crosscultural psychologists of the 1970s and 1980s see experimental psychology itself as having something to learn from the results of anthropological studies. Instead of *abstracting* from all possible cultures and so losing the significance of all "meanings," the crosscultural psychologists seek to *generalize* from all actual cultures and so to discover what "meanings" are in fact universal, and so, generalizable. Eleanor Rosch's (1973) work on "salient colors," following Berlin and Kay's (1969) pioneer work in the linguistics of color, is an excellent example. What it illustrates is that by making a preliminary digression into *Völkerpsychologie*, we are not banishing ourselves from experimental psychology in perpetuity. On the con-

trary, we may subsequently be able to come back out of *Völkerpsychologie* into experimental psychology and enrich it with new discoveries of kinds that were precluded both by Wundt's dichotomization of psychology and by the traditional methodology of experimental psychology in America.

To spell this point out more clearly: the experimental psychology that has lent itself to an honest marriage with cultural anthropology, and so helped to create the crosscultural psychology of the present day, is not the dogmatic, positivistic psychology of the 1920s to 1950s. It is not, in other words, the psychology that had condemned itself to incoherence and meaninglessness as a result of its own chosen methods and assumptions. Rather, it is the more modest experimentalism that had originated in the psychophysics of Weber and Fechner and in the sensory psychology (the so-called "physiological optics") of Hermann von Helmholtz, before being taken up and developed by Wundt and his contemporaries. For in psychology, as in physics and biology earlier, a sufficiently unpretentious experimentalism allied to clearheaded analytical arguments (philosophical, conceptual, or theoretical, call them what you will) was quite capable of keeping out of trouble. And the experimental procedures of (say) a Helmholtz can usefully be taken into the field and employed for the purpose of crosscultural inquiries, in New Guinea or Nigeria as well as in New Hampshire or Nebraska.

In this respect at least, we have at last reached a point at which Wundt's two separate research programs for psychology are beginning to come together. It is reasonable to hope that, from now on, those who are concerned with the study of the cultural aspects of higher mental functioning will, increasingly, take with them into the field methods of inquiry developed by the experimental psychologists. Conversely, we can hope and expect that the outcomes of their cultural investigations will, increasingly, feed back illuminating concepts and generalizations suitable for study within the experimental laboratory. If this occurs, the dividing line between the two classes of mental functions and activities that lend themselves to investigation by experimental and historico-cultural methods, respectively, will be seen to be a good deal more fuzzy and blurred than Wundt originally assumed.

Even more subversive, from the standpoint of hardline experimental methodology, is another conclusion to which the new crosscultural psychology has recently been leading. To quote an observation by Eleanor Rosch (1978):

When a context is not specified in an experiment, people [i.e., experimental subjects] must contribute their own context. Presumably . . . in the absence of specified context, subjects assume what they consider the normal context or situation for the occurrence of that object [i.e., the situation or stimulus under experimental investigation] (pp. 42-43).

If these observations are correct, then all the precautions that hardheaded American experimental psychologists have relied on since the 1920s in order to ensure the "objectivity" of experimental material by insulating it against extraneous associa-

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tions have been in vain. For the only effect of these precautions has been to drive experimental subjects back to their unanalyzed assumptions about "the normal contexts" and the actual patterns of events in daily life that lead us to impute "meanings" to objects. In short, the emergence of crosscultural psychology makes it not merely *possible* to bring back into experimental psychology material from which it has been cut off for quite some time: it makes it *compulsory* to do so.

At the same time, with reference to the residual dogmatic empiricism that is the legacy of the neobehaviorist era, we can hope and expect that an increased freedom of thought will typify future psychology. We are not thinking here of a lazy, undisciplined, relativistic approach to psychological theory. Empiricism has its own legitimate role, as does the quest for universality. The search for empirically grounded theories of broad scope has characterized science in all its many forms. But there needs to be room for the creative play of the scientific imagination. History abounds with instances of important empirical observations that were made only after—and on account of—the formulation of significant theoretical insights. To cite one example alone: John Hughlings Jackson's important discoveries regarding the neurophysiological correlates of aphasia were dependent upon Jackson's belief in Herbert Spencer's theory of evolutionary associationism (Young, 1970, Chap. 6). In addition, there needs to be room for serious thinking about the fundamental (including metaphysical) issues that underlie the field of psychology. Even Ernst Mach (1896), the arch-positivist, insisted that great scientific investigations could be "carried out only by a [person] who is inspired by a great and philosophically most profound view of the world" (p. 240). In recent years, in some of the "harder" areas of psychology (e.g., Eccles, 1953; Pribram, 1971; Sperry, 1969), serious reflection of the sort we are suggesting has begun to take place. This is a trend that deserves to continue.

One last comment: it is not only *thinking* that suffered because of American psychology's cult of empiricism. As we have tried to indicate, empiricism itself suffered because of the rigid, experimental fetters that were placed upon it. Surely there should be room too for a retreat, at least by some, from *experimental* empiricism. All science, and probably all speculation, originates at a *more basic level of empiricism*: the level of experience. In a discipline that is often confused about its subject matter, it is not a bad idea to return to basic experience from time to time. The natural taxonomy that arises therefrom is much more likely to provide a useful framework for experimental work than the artificial taxonomies that structure so much of the field today.

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