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# The Case for Praxics

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Since the early 1900s a variety of names has been proposed for the scientific study of behavior, but none has come into general use. "Praxics," a recent entry, is defensible on several grounds. "Behaviorism," on the other hand, is the name of a school of philosophy. Though praxics has roots in behaviorism, the term "behaviorism" should not be applied to praxics. Confusion between the science and the philosophy has retarded the growth of the science immeasurably. Its growth has also been impeded by its association with psychology, which is still primarily the study of mind. Efforts are underway to establish praxics as an independent field.

*It was not a wholly satisfactory name for a field.*
—B. F. Skinner (1979, p. 331), on "the experimental analysis of behavior"

"Praxics"—a blend of "physics" and "*praxis*" (πρᾶξις), the Greek for "action" or "behavior"—is a term I and others now use for the experimental analysis of behavior and related disciplines. We define it as *the study of behavior.* We call one who studies behavior a "praxist," after "chemist." In this essay I present (a) the case for the use of the terms, (b) the case for drawing a clear distinction between praxics and behaviorism, and (c) the case for the separation of praxics and psychology. Most of the arguments I will make have, it turns out, already been made, and hence much of this essay is historical.

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## TERMS

There have been several attempts to name the study of behavior in general and the experimental analysis of behavior in particular (Epstein, 1984a). Most of these efforts have failed. Two—"ethology" and "praxiology"—have been partially successful in certain domains.

### *Ethology*

The term "ethology" has two different, though closely related, modern usages. It was defined by Lorenz and Tinbergen as the study of instinct (e.g., Tinbergen, 1951). It was a specialty within biology, concerned primarily with innate behavior patterns in non-human animals in their natural habitats. By the 1960s it had evolved into a more comprehensive field, defined as "the biology of behavior" (Eibl-Eibesfeldt, 1970) or "the biological study of behavior" (Tinbergen, 1963). For many years there was little communication between ethologists and experimental psychologists and none to speak of between ethologists and those who worked in the operant tradition. Though exchanges are now common (e.g., Fantino & Logan, 1979; Nevin, 1973), ethology still remains somewhat narrow in its focus: Ethologists still study non-human animals for the most part; they rely almost exclusively on field studies and are skeptical of laboratory research on behavior; they are interested mainly in feral animals and are critical of the use of laboratory-bred or domesticated animals;

and so on.[1] "Ethology" is not an appropriate label for the experimental analysis of behavior or the many other scientific fields that are concerned with the determinants of behavior.

A prior use of the term was somewhat more comprehensive and closer to at least some characterizations of modern psychology. In Book Six of his classic *A System of Logic,* published in 1843, John Stuart Mill proposed "ethology" as a lable for what he hoped would be a new science, "the science of the formation of character." He derived the term from "*ethos*" (ἦθος), the Greek for "character," by which he meant behavior in all of its aspects. He hoped to see the emergence of a comprehensive science to supplement the philosophical psychology of his day, but none emerged, and the term fell into disuse. The original *Oxford English Dictionary* (henceforward, *OED*), which was published in installments between 1884 and 1928, gives Mill's definition, as well as two obsolete definitions from the 17th and 18th centuries: the study of ethics, and the practice of mimicry. The 1933 supplement to the *OED* adds the first biological definition, the obvious precursor to the modern usage: "The branch of Natural History which deals with an animal's actions and habits, its reaction to its environment." The first relevant usage is attributed to two zoologists, Parker and Haswell (1897), who defined it as the study of "the relation of the organism to its environment," which, they said, had also been called "bionomics."

## *Praxiology*

The history of the term "praxiology"—also spelled "praxeology"—is considerably more complicated.[2] F. S. Keller has proposed it from time to time (e.g., 1984) as a name for the experimental analysis of behavior. According to Keller (personal communication, October 1983), he first heard it in a course he had as a graduate student at Harvard in 1928. His instructor, Dwight Chapman, used the term jokingly, and Keller, too, has used it somewhat hesitatingly ever since. It has not been adopted by operant conditioners, nor by biologists. But it has been used fairly widely in other domains—philosophy, education, and economics, in particular—so widely that it is listed in several unabridged dictionaries and dictionaries of philosophy and behavioral science.

The original *OED* contains no such listing, but the 1982 *Supplement* contains a listing 49 lines long. "Praxeology," "praxiology," or "praxology" (in that order) is defined as "The study of such actions as are necessary in order to give practical effect to a theory or technique; the science of human conduct; the science of efficient action." "Praxiologist" is defined only as "one who studies practical activity." The American standard, *Webster's Third New International Dictionary,* published in 1966, defines "praxeology" or "praxiology" (again, in that order) simply as "the study of human action and conduct."

Many specialty dictionaries give similar definitions. Horace B. English's (1928) classic *A Student's Dictionary of Psychological Terms* defines "praxiology" as "Study of the activities or movements or 'deeds' of an organism as a whole; synonymous with behaviorism except in not denying the importance of mental processes." The *Psychiatric Dictionary* (Hinsie & Campbell, 1970) disagrees on the last point: "Praxiology" is "Dunlap's term for the science of behavior, which excludes the study of consciousness and similar non-objective, metaphysical concepts." Wolman's (1973) *Dictionary of Behavioral Science* defines it thus: "1. Psychology viewed as the study of actions, and overt behavior. 2. . . . Any normative science . . . e.g., education, social philosophy, ethics, etc., that sets norms and goals for human actions." The *Dictionary of Philosophy and Religion* (Reese, 1980) refers the reader

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[1] In *Four Saints in Three Acts* Gertrude Stein provided a suitable rejoinder: "Pigeons in the grass, alas."

[2] The English term "praxis" is also common, but it will not be discussed in this essay, since most of its applications have little or no relevance to the issues at hand.

to an entry on Kotarbiński, about whom more will be said below.

Many similar works contain no such entry, including the 1977 *International Encyclopedia of Psychiatry, Psychology, Psychoanalysis, and Neurology* (this is significant, since Wolman was the editor), *The Encyclopedia of Philosophy* (1967), *The Encyclopedia of Psychoanalysis* (1968), *The Dictionary of Psychology and Related Fields* (1971), *The Dictionary of the History of Ideas* (1973), *A Concise Encyclopedia of Psychiatry* (1977), *A Dictionary of Philosophy* (1979), *The Encyclopedia of Psychiatry* (1981), and the *Dictionary of Philosophy* (1983). These omissions notwithstanding, the term clearly has some legitimacy. How did it get it?

A number of scholars assert that the term originated with the London author and physician, Charles A. Mercier, who lived from 1852 to 1919. In his 1911 book, *Conduct and Its Disorders,* he wrote: "Apart from the general advantage . . . of having a systematic knowledge of conduct as a whole; there are certain special advantages to be derived from a study of Praxiology, if I may so term it" (p. viii). Of course, there is nothing new under the sun: Seven years earlier, W. R. Boyce Gibson, a lecturer in philosophy at the University of London, used the term "praxology" for what seems to have been the first and last time (also see Ford [1952], who apparently reinvented the term yet again). On page 190 of Gibson's book *A Philosophical Introduction to Ethics* appear the following rather obscure statements: "The proper propaedeutic for a course in moral philosophy would, in my opinion, consist of a theory of experience (or *philosophical* logic), followed up by a teleological (or *philosophical*) psychology. I say 'theory of experience' instead of theory of 'knowledge' or 'epistemology,' in order to include the theory of action or 'praxology'" (italics original). And von Mises (1944)—who cites neither Gibson nor Mercier—credits the first uses of the term "praxeology" to the French philosopher Espinas in the 1890s (e.g., 1890, 1897) and the Russian economist Slutsky in 1926.

*Kotarbiński.* Praxiology was taken up by—or perhaps reinvented by—individuals in four separate fields. The Polish philosopher Tadeusz Kotarbiński (e.g., 1965) defines it as "the general theory of efficient action"—for "action" read "labor" or "work"—which, he says, derives from or is at least harmonious with such diverse works as Marx's *Capital,* Mill's *Utilitarianism,* Machiavelli's *Il Principe,* and Defoe's *Robinson Crusoe.* Though they didn't use the term, he says, George Herbert Mead, Talcott Parsons, and Georges Hostelet were all concerned with praxiology, which he describes as follows:

> The praxiologist concerns himself with finding the broadest possible generalizations of a technical nature. His objective is the technique of good, efficient work as such, indications and warnings important for all work which is intended to achieve maximum effectiveness. (p. 1)

This use of "praxiology" does not rule out mentalism or teleology, and the field Kotarbiński describes is neither experimental nor data based. Chapter 14 of his 1965 book is entitled "Mental Activity" and reads in part:

> . . . every act includes elements which are mental in nature. This is so because in every act there is contained a free impulse directed towards a certain goal, which means that the agent not only moves but also is conscious of the purpose of his movement—and that consciousness undoubtedly is a mental factor. . . . [Purely] mental actions do exist—mental solution of arithmetic problems; recalling past events; composition of musical works without writing down notes. . . . Hence, one may not only discuss the role of mental events in any action, but also . . . analyze purely mental actions as a special case of action in general. (pp. 175–176)

Kotarbiński's version of praxiology has been applied in education, in essays, for example, by James Perry (e.g., 1971) and E. S. Maccia (e.g., 1966), who argue, understandably, that we need to know more about how to be effective in education. They do not mention advances in any empirical science of behavior.

*Von Mises.* The Austrian-American economist, Ludwig von Mises (e.g., 1944, 1962), who held a faculty appointment at New York University from 1945 until his death in 1973, defined praxeology as the general science of human conduct.

Economics, he said, was a special branch—"the only developed branch" (Gutiérrez, 1971)—of this science. Again, the field he described is not a laboratory science:

> Its scope is human action as such, irrespective of all environmental and incidental circumstances of the concrete acts. It aims at knowledge valid for all instances in which the conditions exactly correspond to those implied by its assumptions and inferences. Whether people exchange commodities and services directly by barter or indirectly by using a medium of exchange is a question of the particular institutional setting which can be answered by history only. But whenever and wherever a medium of exchange is in use, all the laws of monetary theory are valid with regard to the exchanges thus transacted. (von Mises, 1944, p. 529)

It is not, he said, "based on psychology and is not a part of psychology" (von Mises, 1944, p. 531), which he took to be the study of mind. "Praxeology deals with choice and action and with their outcome. Psychology deals with the internal processes determining the various choices in their concreteness" (p. 531).

If praxeology is not empirical, where do its laws come from? Von Mises and his students advanced a series of postulates and theorems of action which were "developed by reason and logic from *a priori* truths" (Bien, 1969, p. 4). "The starting point of praxeology," wrote von Mises (1962), "is a self-evident truth, the cognition of action, that is, the cognition of the fact that there is such a thing as consciously aiming at ends" (pp. 5–6). The laws were still dependent on data, however, even though they supposedly weren't derived from them, which led Gutiérrez (1971) to dismiss the entire enterprise as vacuous. Further examination of these issues would be out of place here. Suffice it to say that von Mises, like Perry and Maccia, seemed unaware that there existed a science of behavior any more sophisticated than the one he was espousing. That should be a matter of some concern to those who are immersed in such a science, a matter to be discussed below.

*Kuo.* "Praxiology" was also adopted—with a passion—by Zing Yang Kuo, an ardent behavioral psychologist who lived from 1898 to 1970. Kuo was trained by Tolman at the University of California at Berkeley, where he became, for a time, a devotee of Watson. He soon became an even more radical behaviorist than Watson himself, and, indeed, he, like Skinner, called his brand of behaviorism "radical behaviorism" (e.g., Kuo, 1967).

In 1923, after five years at Berkeley, he returned to China. By 1930 he had established four laboratories devoted to the study of animal behavior, one financed solely by income from his books in Chinese. He is best known for his strong stand against the concept of instinct (e.g., Kuo, 1921, 1930), as well as for a long series of innovative studies on the behavior of the chick embryo (e.g., 1932, 1933). Unfortunately, political events in China terminated his research career in the 1930s. Between 1936 and 1940 he had neither a job nor a country, and he was not able to publish scientific works again until the 1960s (see the preface of Kuo, 1967). Except for a brief period in 1963, he was denied the opportunity to conduct laboratory research for the last 30 years of his life (Gottlieb, 1972).

In a brilliant essay called "Prolegomena to Praxiology," published in *The Journal of Psychology* in 1937, Kuo made the case for praxiology, and, in effect, for praxics.[3] He attributed the term to Mercier and Dunlap, whom, he said, suggested it as a better name for Watson's behaviorism.[4] But, disturbed by what he called "the half-heartedness of the behavioristic revolt, and its inability to make a decisive break with the tradi-

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[3] A footnote in the Kuo (1937) paper promises a book on the subject, to be called "Principles of Praxiology," but I am aware of no such book. Roback (1937) attributes to Kuo a book published in Chinese in 1935, whose title is translated "The Scope of Praxiology."

[4] Though several sources attribute the term "praxiology" to Knight Dunlap, none that I have read has provided relevant references, and I have been unable to find the term in his writings. And Mercier, of course, proposed the term "praxiology" as a name for the study of behavior several years before Watson presented his seminal paper on behaviorism.

tional psychology" (p. 5), he assigned "praxiology" yet another meaning:

> [Praxiology is] *a branch of biology which deals with the behavior of animals (including man) with special emphasis on its ontogenic and physiological aspects as the chief channels through which causal factors of behavior may be discovered* [italics original]. The ultimate purpose of praxiology is, of course, prediction and control of behavior. But unless its physiology and developmental history are sufficiently known, it will be futile to hope to be able to predict and control behavior. Watson, the founder of behaviorism, has made a great mistake by declaring that the behaviorist can make a thoroughly comprehensive and accurate study of behavior without reference to physiology. . . . [The praxiologist] is firmly convinced that by acquiring adequate data on the nature of stimulus and response and their intricate relations and on the physiology and ontogeny of behavior he can give a thoroughly scientific description of behavior in purely mathematical and physical terms. (pp. 5–6)

*"Praxiology" in perspective.* Though Kuo's program—an independent, biologically-based field, the subject matter of which is the behavior of organisms—is not without merit, the term "praxiology," I submit, is no longer a viable name for it. It has been used too widely since the 1930s and in too many ways. The dominant usage today seems to be Kotarbiński's, which is clearly an inappropriate label for an empirically-based science like the experimental analysis of behavior. The term itself, it could be argued, is unattractive—antique, perhaps, in its sound and spelling. "Praxology," the form which is cognate with "psychology," seems attractive until a listener confuses it with "proctology."

## *Anthroponomy*

Another early behavioral psychologist, Walter S. Hunter, proposed yet another term for the scientific study of human behavior (e.g., Hunter, 1925, 1926, 1930). Hunter, like Watson, was trained at the University of Chicago under Angell and Carr just after the turn of the century. And, like Watson, he wanted psychology to become a natural science. He devoted himself to this end throughout his life (Schlosberg, 1954).

He argued that psychology was tainted with "charlatans" (Hunter, 1925, p. 286) and with "spiritualists, psychic researchers, and Freudians, who object to the petty laboratory problems in terms of which the psychologist outlines his field and who resent deeply and religiously the tendency in modern psychology to eliminate the purposeful activity of mind" (Hunter, 1926, p. 83). The solution, he thought, was a new name for that part of psychology which was truly objective. His seminal paper on the subject appeared in 1925, while he was a faculty member at the University of Kansas:

> Anthroponomy is the science of the laws which govern human action—the science of human nature [cf. J. S. Mill's "ethology," described above]. It seeks by experiment and systematic observation to arrive at an understanding of the factors which determine certain of the observable phenomena of the human individual . . . . The essential difference between psychology and anthroponomy lies in the attitude towards the observations which are made. The former science is derived directly from the Greek philosophers and is concerned only with the study of mind, consciousness, or the psyche . . . . Such a point of view results, as might be expected, in the neglect of all data concerning the human individual which cannot be interpreted as evidence for the existence of some type of psychic process. In other words, scientific observations are not to be valued for their own sake, but solely because of the inferences concerning consciousness which they make possible. . . . [In contrast, the anthroponomist] never raises the question whether or not the observable phenomena express or embody a psychic world beyond them. He studies the large field of observable human nature in order to describe and explain the phenomena there found. (p. 286).

Hunter discussed and dismissed a variety of other terms: "objective psychology," "behaviorism," "science of behavior," "tropology," and "anthropology":

> . . . non-psychic studies have become so numerous . . . that new names for the science are coming into use. Of these Objective Psychology and Behaviorism are the most prominent. The former, however, is defective in that it suggests a sub-division of psychology and in that it contains the obvious contradiction of a non-psychic psychology. . . . Among Americans, Behaviorism has proved a wonderfully apt term. It contains no suggestion of the psychic . . . . It does, however, refer to an "ism" and is not, therefore, well suited to designate an entire science. The term Science of Behavior is too cumbersome, and the Greek equivalent Tropology is too reminiscent of a special problem in the behavior of lower animals [the "tropism"], to make these acceptable names for the science . . . . The term Anthropology,

science of man, escapes these difficulties, but is preempted by a closely related discipline .... At present only one thing is certain, and that is the inappropriateness of the term "psychology." (pp. 290–291)

Hunter died in 1954, his dream for anthroponomy unrealized. In a eulogy which appeared in *The Psychological Review*, J. McV. Hunt (1956) wrote: "... he suggested that the term *anthroponomy* replace psychology .... The amusement aroused by this suggestion he took in good-spirited stride" (p. 214).

### *Behavior Analysis*

The term "behavior analysis" has come to be used by many individuals as a label for both basic research, primarily in operant conditioning, and the technology that has evolved from this research. The term is far from ideal as a label for the study of behavior in general or the experimental analysis of behavior in particular.

The term, first of all, is nomic but not nominal; that is, it is the customary label for what certain people do, but it is not truly a name. It is, rather, a description: Both behavioral engineers and laboratory researchers sometimes *analyze* behavior. Even as a descriptor, the term is not wholly satisfactory, for behavior analysts do far more than analyze, which means to separate a whole into its constituent parts. They manipulate variables, make predictions, construct theories and models, treat problem behavior, shape and maintain behavior, engineer physical environments, and so on.

The term seems to be used more consistently as a label for applied behavior analysts than for basic researchers, who are sometimes called "operant conditioners" or "operant psychologists" but who are usually not labeled anything at all, since no suitable label exists. Basic researchers are, understandably, never called "experimental analysts of behavior."

The term "behavior analysis" is used almost exclusively by behavior analysts themselves and even then only cautiously. It is a kind of family term, used by family members only. The public knows nothing of behavior analysis, though it has heard of every other scientific discipline from anthropology to zoology. And, with few exceptions, the same ignorance may be attributed to funding agencies. One does not identify oneself as a "behavior analyst" on a grant application; doing so would likely be suicidal.

Finally, because the term is so closely linked with the operant tradition, it is more a label for a fraternity than a science. I oversimplify perhaps only slightly by saying that behavior analysts are those people who are active in the Association for Behavior Analysis; they are concerned primarily with operant conditioning in some domain or other. In contrast, behavior *therapists* are those people who are active in the Association for Advancement of Behavior Therapy; they are perhaps slightly more concerned with classical conditioning and, these days, with cognition. There is some overlap between the groups, of course.

### *The Experimental Analysis of Behavior*

In the second volume of Skinner's autobiography, he discusses the origin of a term whose sesquipedalian segments have plagued the palates of even its most devoted disciples evern since. With Fred S. Keller and William N. Schoenfeld, Skinner organized the first conference on the domain of research he had originated nearly two decades earlier. It was held at Indiana University in June, 1946[5]:

We called it a conference on the "experimental analysis of behavior," taking the "experimental analysis" from the subtitle of *The Behavior of Organisms*. It was not a wholly satisfactory name for a field. What should we call ourselves? "Students of behavior"? "Behavior analysts"? And what adjectives could we use to identify our research, our theories, or our organization? "Behaviorism," "behaviorists," and "behavioristic" were not quite right. They were too closely tied to John B. Watson. (Skinner, 1979, p. 331).

Ironically, Hunter had rejected the label "science of behavior" as "cumber-

[5] A photograph of those who attended this meeting appears on page 456 of Volume 5 (1962) of the *Journal of the Experimental Analysis of Behavior*.

some" more than 20 years earlier. Were standards of locution different in 1946?

Against all odds, "the experimental analysis of behavior" has persisted as a label for the activities of those who continue to do research in the tradition of Skinner's *The Behavior of Organisms*. It is far from satisfactory as a label for this domain: It is awkward, and, like "behavior analysis," it is more a description than a name. And, again, researchers in this field do more than analyze. Finally, the term is wholly unsatisfactory as a name for the study of behavior in the broader sense, since it is so closely identified with a single research tradition.

### *Other Terms*

Some related terms deserve at least brief mention. Hunter had rejected the term "objective psychology," which had been proposed by the Russian V. M. Bekhterev and was used from time to time as an umbrella term for non-mentalistic forms of psychology (see Diserens, 1925). Bekhterev eventually switched to "reflexology"; the study of reflexes would, he asserted, following Sechenov (1863/1935), eventually encompass all psychological phenomena (Boring, 1950).

Knight Dunlap (e.g., 1922, 1926) proposed the term "scientific psychology" in much the same spirit that Hunter offered "anthroponomy," although Dunlap did not reject consciousness as a subject matter. He wanted merely to dissociate himself from various "new psychologies" which seemed to be of questionable value, like those based on Freud, phrenology, or psychic research. Dunlap is also credited with inventing the term "psychobiology," which Moore (1923) suggested as the inclusive label for both psychology (which, said Moore, "means and can only mean the science of mind" [p. 235]) and praxiology, the science of behavior.

Finally, J. R. Kantor offered the terms "interbehaviorism," "interbehavioral psychology," and "organismic psychology" as labels for his own extensive philosophical and theoretical contributions (e.g., Kantor, 1924; Kantor & Smith, 1975). Kantor, like Watson and Skinner, was ardently anti-mentalistic, but, unlike other behavioral psychologists, he was not content to say that the subject matter of psychology was behavior, for this meant, he believed, that one might study mere movement or activity apart from an organism's interactions with its environment. "[Only] the treatment of psychological events as fields in which responses or acts of the organism operate in interaction with stimulus objects under specified conditions can supply complete and satisfactory scientific descriptions and interpretations in psychology as in every other science" (Kantor & Smith, 1975, p. 31). Both research on behavior and empirically-derived theories of behavior have typically involved an analysis of such interactions, even though Kantor's point has seldom, if ever, been stated explicitly.

### *Praxics*

If indeed all of these terms are in competition with each other, "praxics" should surely win at this point by process of elimination. It hasn't failed yet, which is more than can be said for most of the other terms. It is too new to have been abused. It can be difficult to pronounce at first, but it proves, with practice, to be much easier than "tachistoscope," "statistical significance," or "the experimental analysis of behavior," and "praxist" is easier still. "Praxics" is simple in form (cf. "cybernetics") and is recognizable on sight as the name of a science, probably because it is only three letters away from "physics." It has a respectable and appropriate Greek root.

Unadorned, the term describes neither an "attitude" (cf. "anthroponomy," "behaviorism," "interbehaviorism," "objective psychology," "scientific psychology," and so on) nor a methodology (cf. "the experimental analysis of behavior" or Kotarbiński's "praxiology"). It simply circumscribes a subject matter, as do the terms "physics," "biology," "epidemiology," "psychology," "otolaryngology," "ornithology," "astronomy," and so on. The labels of scientific domains are labels

for *subject matters.*[6] How odd that a subject matter as pervasive as the behavior of organisms has been so difficult to name.

The study of behavior needs some legitimacy, and I suspect that, like the neonate, it lacks legitimacy for the very simple reason that it lacks a name. A name, after all, can create legitimacy even where none is warranted. There is credibility in a name (*IBM, immunology, microbiology*) and, sometimes, prestige (*Lloyds of London, mathematics, neurology*). "If we lost our stock of labels," said William James, "we should be intellectually lost in the midst of the world." Certainly the lack of a name for the science of behavior has made it hard to communicate with other people. Over the years I have responded to the question, "But what *kind* of psychologist are you?" in at least six different ways, and I have occasionally had to try two or three on the same listener to evoke the slightest sign of comprehension. Unfortunately, that sign has usually been the rather singsong "Ohhh . . ." that you emit when you're trying not to show how much you disapprove of someone. Which brings me to behaviorism.

## PRAXICS AND BEHAVIORISM

If praxics is the study of behavior, what is behaviorism? Do we need both terms?

"Behaviorism," as J. B. Watson (1913) first used the term, was the name of a *movement* in psychology—"a breath of fresh air, clearing away the musty accumulation of the centuries" (R. I. Watson, 1963, p. 401). It was not the name of a subject matter; nor was it the name of a school of philosophy. The term "behaviorist" was more like "suffragette" than "physicist." Watson did not offer "behaviorism" as a label for the science of behavior; rather, he hoped to convince psychologists that *psychology should become the science of behavior.* One need hardly look beyond the titles of his two seminal works on behaviorism to see the point: "Psychology as the Behaviorist Views It" (J. B. Watson, 1913) and *Psychology from the Standpoint of a Behaviorist* (J. B. Watson, 1919). Behaviorism was not intended as an alternative to psychology but rather as a corrective action. Consider the opening sentences of Watson (1913):

> Psychology as the behaviorist views it is a purely objective experimental branch of natural science. Its theoretical goal is the prediction and control of behavior. Introspection forms no essential part of its methods, nor is the scientific value of its data dependent upon the readiness with which they lend themselves to interpretation in terms of consciousness. (p. 158)

Dunlap, Hunter, Weiss, Skinner, and others continued Watson's program of reform. In December of 1928, shortly after he began graduate school, Skinner wrote in a letter: ". . . my fundamental interests lie in Psychology, and I shall probably continue therein, even, if necessary, by making over the entire field to suit myself" (Skinner, 1979, p. 38).

The movement was not entirely successful, but this is a matter which we can sidestep for the moment. It is safe to say that behaviorism as a *movement* in psychology died long ago. There are few behavioristic *reformers* around these days, and psychology is not especially vulnerable to them. That was not the case in Watson's day. He was viewed by many in the field as a savior. He was elected President of the American Psychological Association only two years after he published his 1913 paper, when he was only

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[6] Peters (1962), in the revision of *Brett's History of Psychology,* argues that sciences—and psychology in particular—are not definable by their subject matters. "No doubt," he adds, "there is a quite usual and harmless sense of the term 'subject-matter' in which, in any account of scientific method, petrologists, ornithologists, and astronomers can be said to have different subject matters. . . . [But what] we call psychology is just an amalgam of different questions about human beings [what about other animals?] which have grown out of a variety of traditions of enquiry" (p. 27). Though it is true that chaotic and diverse events may have preceded the formalization of a scientific domain, once formalized it virtually always has a clearly delimited subject matter, specified by its name. For the purposes of this essay, I am content to use the term "subject-matter" in that "quite usual and harmless sense" that allows us to say that the subject matter of petrology is rocks, the subject matter of ornithology is rooks, and the subject matter of psychology is either mind or behavior.

37 years old—an unprecedented rise to prominence in the field. The Association would hardly reward a behavioristic flag-waver with the same kind of recognition today.

But the movement left two distinct products, each of which has grown and, to some extent, flourished: First, it helped to convince many researchers that the behavior of organisms was a legitimate subject matter in its own right. And second, it led to the development of a school of philosophy—consisting mainly of psychologists, not philosophers—called "behaviorism."

*Behaviorism as Philosophy*

A system of philosophy was implicit in Watson's early statements. In delineating a subject matter, he also made many assertions about the nature of consciousness, thought, conscious content, introspection, feelings, perception, free will, the role of heredity in human behavior, and other matters. Like James, Freud, and Skinner, he was a strict determinist. He asserted early in his career, without supporting data, that thought was simply laryngeal movement.[7] On the nature-nurture question, Watson at first stood the middle ground, but he eventually shifted toward thorough-going environmentalism. Note that he could conceivably have delineated the subject matter *without* taking such stands.

Watson never made a clear distinction between the philosophy and the science he espoused. If anything, the distinction got murkier over the years. In his 1930 revision of *Behaviorism*—his last contribution to academe—he used the term "behaviorism" as a label for the movement he had begun, as a synonym for "objective psychology," and as a label for the science of behavior—by this time, he had shifted the emphasis to *human* behavior (see Watson, 1930, p. 2). He continued to take strong stands on a wide range of philosophical issues.

Skinner seems to have been the first to tease apart the science and the philosophy, perhaps because he added so much substance to the science. He has used the term "behaviorism" fairly consistently as the label for a philosophical school: "Behaviorism is not the science of human behavior," he wrote, "it is the philosopy of that science" (Skinner, 1974, p. 3). It is, of course, more than that; it is a school of philosophy in its own right, which has concerned itself with issues such as the nature of mind and consciousness (e.g., Quine, 1976; Wessells, 1982), the nature and importance of feelings (e.g., Russell, 1978), free will and determinism (e.g., Burton, 1980), values (e.g., Graham, 1977; Rottschaefer, 1980), the nature of knowledge (e.g., Russell, 1980), the nature of perception and language (e.g., Faraone, 1983; Natsoulas, 1982), and so on (cf. Zuriff, in press). A philosophy journal called *Behaviorism* was established by Willard F. Day, Jr. in 1973 to encourage discourse on these and other topics.

Behaviorists have made assertions such as: (1) Behavior is orderly and predictable; free will is an illusion. (2) Mind is a superfluous concept. Thoughts are not the causes of behavior. Our concern with mind keeps us from finding out more about the real determinants of behavior. (3) Feelings do not cause behavior. They are unimportant, and our preoccupation with them keeps us from finding practical solutions to our problems. (4) Language is also behavior and can only be understood as such. The formal analysis of language tells us nothing. (5) Perceptual phenomena such as imaging can also be treated as behavioral phenomena, and the laws that govern observable motor behavior may be adequate to explain perceptual behavior.

As Skinner recognized, the laboratory study of behavior proceeds almost entirely independently of such assertions. Whether or not behavior is "determined" in the philosophical sense, one can still search for—and, of course, one will find—order in behavior in the laboratory. One can study and will undoubtedly discover interesting things about be-

[7] He adopted a more sophisticated view of thought in his later works. See Watson (1930) and Watson and McDougall (1928).

havior whether mind exists or not. The presence or absence of consciousness in a given species will not affect one's experimental attack on behavior in that species "by one jot or one tittle" (Watson, 1913, p. 161). Where data and formal theory are relatively sparse—as in the analysis of language or perception—philosophical arguments will naturally be taken more seriously (after all, they're all we've got), but *once the scientific attack on some phenomenon is successful, philosophical arguments fall by the wayside.* This is not to say that data necessarily settle philosophical debates; rather, the data tend to create a substantive understanding of the subject matter which takes on its own life. A century from now, if the behavioral approach to language has been successful, praxists will be debating about the significance of data and about the merits and demerits of various formal, predictive theories of language. The views of the philosopher will be at best of only marginal interest, just as they are at best of only marginal interest to the laboratory chemist. Facts constrain speculation.

Though Skinner seems to have recognized the distinction between behaviorism and praxics, the fact that he is both the most accomplished living behaviorist in the world *and* the most accomplished living praxist has surely caused trouble. And though he has made the distinction, others have not. Rachlin (1970), for example, defines "behaviorist" as "[one who is] engaged in the experimental study of behavior" (p. 2). Like Watson, Rachlin presents the philosophy of behaviorism side by side with laboratory findings in the study of behavior.

### *Behaviorism vs. Praxics*

The term "behaviorism" is by no means obsolete. It is, quite the contrary, essential. But its appropriate use today is as the name for a school of philosophy. Praxics, on the other hand, is a laboratory science, inspired in large part by behaviorism, as well as by the theory of evolution and other advances in the biological sciences. The clear separation of praxics and behaviorism is critical for a number of reasons:

a) *No laboratory science, no matter what its origins, should be constrained by a philosophy.* "To set limits to speculation," said Whitehead, "is treason to the future." As I noted above, data generate limits to speculation, but it is one thing to have one's subject matter constrain one's theories and quite another to be constrained by philosophical dogma. The role that genes play in language acquisition, for example, is an empirical question. The old environmentalist assertion that genes play a trivial role goes beyond the facts currently available.

This is not to say that scientists do not make assumptions. On the contrary, science has always rested on a foundation of assumptions, and the assumptions, once tested, have often proved to be wrong (Burtt, 1954). Early in the century, for example, many physicists abandoned the assumption of determinism in favor of the assumption that the universe is probabilistic; the latter, more conservative assumption was, many said, an adequate basis for scientific progress (e.g., Eddington, 1928).

Perhaps all of the natural sciences were constrained by close ties to formal philosophy in their early stages, but the philosophical origins of a particular science are eventually forgotten by all but the historians. Modern chemistry, for example, has clear origins in ancient Greek philosophy (Hopkins, 1934); it is no accident that one of the alchemist's tools was called the "philosopher's stone." But philosophical assertions that helped give rise to chemistry—for example, the notion that all things strive toward perfection—would seem bizarre or at least irrelevant to modern chemists. The distillation of science from philosophy has occurred even in psychology. For example, modern psychophysics is the offspring of Fechner's obsession with the mind-body problem; his early research was meant, once and for all, to *solve* that ancient problem (Fechner, 1860/1966). Though research of the sort Fechner con-

ducted is still underway (e.g., Mansfield, 1976), the mind-body problem has played no part in it for nearly a century.

The future is clear: The science of behavior will go free of behaviorism.

(b) Behaviorism is truly an "ism," a system of interrelated assertions and beliefs, primarily about mind, free will, and feelings. Praxists can (and do) study behavior no matter what their opinions are on these matters, just as physicists study the material world no matter what their opinions are of metaphysics (the physicists I know are dualists of the most extreme sort). And so it should be. It's hard to see how an individual's ability to discover orderly relationships between variables in the behavioral laboratory could be much affected by his or her views on the mind-body problem. *Yet—because of the confusion between behaviorism and praxics—people who have had doubts about behaviorism have kept away from behavioral laboratories or have stayed there uneasily. The behavioral researcher is perceived as a "disciple," a "believer," a "card-carrying member." The behavioral laboratories have surely lost talented researchers as a result, and the range and quality of its investigations have surely been restricted.* An individual with no particular philosophical bent, or with views that change radically from time to time, or even with views that are antithetical to behaviorism, is capable of making a positive contribution to the science. The laboratory doors should be open to all talented researchers, and the dualists currently therein should not feel guilty.

The fact is that you can be a praxist without being a behaviorist, and vice versa. In addition to the many mentalists in the behavioral laboratory, there are many individuals who are deeply religious. Some of the most prominent behavioral researchers in the country are regular churchgoers, and I know of one accomplished researcher who has more than a passing interest in Eastern mysticism. One can also be a behaviorist, of course, without ever entering the laboratory—Kantor was an example—and one might also, like Skinner, be both praxist and behaviorist.

(c) *Behaviorism is unattractive to the American public, and because it has been so closely identified with the laboratory science, the science has suffered materially.* Praxics has produced profoundly important technologies that have aided millions of people—in pharmacology, medicine, child rearing, education, institutional management, therapy, the treatment of the mentally retarded, business, industry, and so on—but it has not received commensurate support and appreciation from the public. If it is presented to the public as a laboratory science that is independent of any "ism"—the support may finally come.

The study of mind—in the hands of "cognitive science," an amalgam of psychologists, linguists, and computer scientists—has presented itself this way in recent years with profound effect. Skinner recently showed me a copy of the "Report of the Research Briefing Panel on Cognitive Science and Artificial Intelligence," one of several chapters in a new book published by the National Academy Press (Estes et al., 1983). The book was commissioned by the White House and the National Science Foundation to serve as a guide to funding agencies for funding in science. According to the preface, cognitive science was identified as one of a small number of scientific areas that "were likely to return the highest scientific dividends as a result of incremental federal investment. . . ." It was placed on an equal footing with *mathematics, the atmospheric sciences, astronomy and astrophysics, agricultural research, neuroscience, human health effects of hazardous chemical exposure, materials science, chemistry, immunology, solid earth science,* and *computer science.*

*The report could easily signal more money annually for cognitive scientists than the grand total of all awards to operant conditioners since the first conference in 1946.* I know personally of more than forty million dollars in foundation funds that have gone to cognitive psy-

chologists over the past few years. And with money goes prestige, laboratories, jobs, professorships, institutes, discoveries, applications (however ill-conceived), and so on. Support of this magnitude will leave its mark on psychology for many years.

Praxics is hardly worth promoting, of course, unless it is advancing as a science. It has advanced, and it is still advancing, though the number and range of topics that have been explored is disappointing, in large part, I am sure, because of the lack of resources. Drawing a clear distinction between praxics and behaviorism is likely to help.

## PRAXICS AND PSYCHOLOGY

If praxics is to grow and flourish, it must strike out on its own. But rather than make a case at this point for its separation from psychology, I will attempt to recast the problem. To argue that the time for secession has come might imply to some that praxics belonged in psychology departments at some point in the past. *But it never belonged in psychology departments.* Psychologists and praxists have been locked in mortal combat for more than half a century, competing for scarce resources and debating about what the appropriate subject matter of psychology should be. *But the appropriate subject matter of psychology is mind, not behavior.*

### *A Historical Blunder*

You are a geneticist. You make your way through the august halls of the Zoology Building, stop at the office of the Chairman of the Department, and insist on speaking to her. You tell her that your discipline has a lot to contribute to zoology and that you want an office and laboratory space in her department. She is surprised, but intrigued by the force of your arguments. You tell her that problems of classification could be handled in a more sophisticated and objective way by genetic analysis and that, indeed, the proper *subject matter* of zoology is actually genetics. She is on her guard. You tell her that you insist on assuming the chairmanship of the Zoology Department immediately. She throws you out of her office.

This tale seems bizarre until you examine the history of American psychology during the first two decades of the 20th century. For hundreds of years the subject matter of psychology had been mind. The term comes from the Greek "*psyche*" (ψῦχή), which means "breath." It came to mean "spirit" or "soul," the animating principle of life, since the absence of breath was a sign of the absence of life. In English we distinguish between "soul" and "mind," but the terms have often been used interchangeably, and in some languages, no clear distinction is made. In German, for example, the word *Seele* is used for both.

Early users of the term "psychology"—for example, Christian Wolff and David Hartley in the early 1700s—defined it as the study of mind, following a tradition of inquiry that had begun 2000 years earlier with Aristotle's *Peri Psyches* (better known by the Latin title *De Anima*). Psychology had long been in the hands of philosophers only, but, over a period of several decades in the 19th century, the first psychology laboratories were established, and psychology emerged as a science (cf. Epstein, 1981)—a science of *mind.* Fechner's vision of 1850, Wundt's laboratory in Leipzig, Ebbinghaus' classic experiments with nonsense syllables, Mueller's experiments on attention—all were concerned with *mind.* The new "functional psychology" that Angell made popular in America during the first decade of the new century was a new approach to the study of *mind.* Morgan's Canon—which the behaviorists later modified for their own uses—was a prescription for simplicity in theories of *mind* (Epstein, 1984b). The original *OED* gives only one definition of "psychology": "*The science of the nature, functions, and phenomena of the human soul or mind*" (italics added).

Then, in 1905, in walks William

McDougall and, a few years later, John B. Watson. Like our psychotic geneticist, they demand nothing less than that the subject matter of psychology be changed.

Consider McDougall's cogent remarks on the subject in *The Battle of Behaviorism* (Watson & McDougall, 1928):

> I, rather than Dr. Watson, am the Arch-Behaviorist. Up to the end of the last century and beyond it, psychologists did in the main concentrate their attention upon the introspectively observable facts, unduly neglecting the facts of human action or behaviour, and ignoring the need for some adequate theory of behaviour and of character . . . . This neglect is implied in the definition of psychology commonly accepted at that time, namely, the "science of consciousness" . . . . (p. 54)

McDougall proceeds with a discussion of the unsuccessful attempts of J. S. Mill and Charles Mercier to create "ethology" and "praxiology," respectively, and then writes:

> It was at this time that I was beginning to struggle with the fundamentals of psychology. And it seemed to me that both Mill and Mercier were in error; that what was needed was not a new science of behaviour under a new Greek name, *but rather a reform of psychology, consisting of greater attention to the facts of behaviour or conduct* [italics added]. . .. I gave expression to this view in my first book [*Primer of Physiological Psychology,* 1905], by proposing to define psychology as the positive science of conduct. I further defended this definition and expounded the need of this reform in my *Introduction to Social Psychology* (1908). And in 1912 I published my little book entitled *Psychology, the Study of Behavior.* (pp. 57–58)

Watson (1913) continued this bizarre program even more aggressively than McDougall had. Not only should psychology adopt a new subject matter, he said, it should also completely abandon its traditional one:

> The time seems to have come when psychology must discard all reference to consciousness; when it need no longer delude itself into thinking that it is making mental states the object of observation. We have become so enmeshed in speculative questions concerning the elements of mind [and] the nature of conscious content . . . that I, as an experimental student, feel that something is wrong with our premises. . . . I believe we can write a psychology, define it as [the science of behavior], and never go back upon our definition: never use the terms consciousness, mental states, mind, content, introspectively verifiable, imagery, and the like . . . . (pp. 163–166)

No doubt more than one department chairman was outraged by such claims, or, as Watson himself said, with atypical understatement, "It was only natural that . . . criticisms should appear" (Watson, 1930, p. x). But, as Boring (1950) noted, Watson's "vigorous propaganda" (p. 641) was consistent with the prevailing *Zeitgeist* in two important respects: First, even in the 1913 paper, Watson promised practical applications for "the educator, the physician, the jurist and the businessman," and second, many psychologists were disillusioned with the progress psychology had made in the study of mind. "Psychology claimed to be science but it sounded like philosophy and a somewhat quarrelsome philosophy at that" (Boring, 1950, p. 642).[8]

In other words, if our geneticist had only been a little more persuasive and his timing a little better, he might have succeeded—for a time—in taking over the Zoology Department.

## *The Legitimacy of Psychology*

The study of mind held its own during the three decades when behaviorism was a force to reckon with in psychology. With the advent of the computer and the adoption of the information processing metaphor by cognitive psychologists, the study of mind gained considerable prestige in the 1950s and 1960s. The marriage of cognitive psychology with linguistics and computer science has created further legitimacy—not only within the field, but among the public at large. The Freudians and their descendants are still active in the field, and even behavior therapists have become taken with the

[8] Boring never gave an inch, however, to behaviorism. In his preface to the 1929 edition of *A History of Experimental Psychology,* which was reprinted in the 1950 edition, he wrote, "Naturally the words 'experimental psychology' must mean, in my title, what they meant to Wundt and what they meant to nearly all psychologists for fifty or sixty years—that is to say, the psychology of the generalized, human, normal, adult mind, as revealed in the psychological laboratory" (p. x).

cognitive model. Moreover, if current trends in funding are any indication, the immediate future of the study of mind is assured.

I don't happen to find the assertion that mind is a legitimate subject matter compelling, but the vast majority of psychologists do—*even, I suspect, a majority of the members of the existing behavioristic organizations.* No matter what the referent of the term, most people seem to think it's worth studying. So be it. Let them study it! Give psychology to the psychologists!

*The Legitimacy of Praxics*

I ask the reader's indulgence while I overdramatize a simple point: *No one—no university administrator, no government official, no foundation director, no biologist, no psychologist, not even the hard-core cognitivist—would deny that the behavior of organisms is a legitimate subject matter.* Cognitivists have been hostile to behaviorists not because of any doubts about the legitimacy of the subject matter but because of the behavioristic polemic: "Mind is *verboten,* let us take over your field."

How tragic were the consequences of that historical blunder. The study of behavior was denied its place in the sun because Watson and others thought they could elbow their way into a field that was not theirs.

*Arguments for Separation*

Many have shared the dream of a science of behavior—Mill, Mercier, McDougall, Watson, Meyer, Weiss, Kuo, Hunter, Skinner, Schneirla, Hull, Tolman, Guthrie, Lashley, and so on. But only Skinner managed to found a school that has survived, and that school constitutes less than three percent of the membership of the American Psychological Association. The percentage, furthermore, has been declining in recent years.[9] Psychology, in short, has not been reformed, and, as I have argued above, it is probably unreformable.

There is an alternative to the reform movement, which, after all, was only one of the methods proposed for the establishment of a science of behavior. The other is the program suggested by Mill and Mercier so long ago—the establishment of an independent, biologically-based science: "a new science of behaviour under a new Greek name," as McDougall said. The first method hasn't worked. Let's try Plan B.

Establishing a science of behavior outside of the confines of psychology makes sense for a number of reasons:

(a) Psychology, like behaviorism, has a terrible public image, largely deserved. The "charlatans and spirit-seekers" about whom Hunter warned are still with us, and the press is still wary.

(b) "Psychology" is an inappropriate name for the study of behavior.

(c) As I noted above, the concept of mind seems as compelling as ever to most people, and it may simply never go away. If someone believes that the earth is flat, you can point to evidence that it is not. But there is nothing you can point to to convince someone that he or she doesn't have a mind. In any case, it seems unlikely that psychology will be the discipline that debunks mind. A successful debunking can begin only with an effective formulation of the behavior of organisms, and that will be more likely to emerge from a thriving, well-funded, independent science of behavior than from psychology.

(d) Seventy years of debate and struggle has resolved nothing. Further debate will only keep us from moving forward.[10]

(e) They'll be glad to see us go.

(f) A split will likely mean new resources. (A scene toward the end of *Walden Two* comes to mind.) As long as there are only so many offices and positions to go around in a psychology department, we will have trouble growing—indeed, we will have trouble surviving. When, someday, an administrator risks the creation

[9] These statements are based on ratios of recent membership figures of Division 25 of the American Psychological Association, which is devoted to the experimental analysis of behavior, to membership figures of the Association as a whole.

[10] Pennypacker (1984) made the point more dramatically: "Never try to teach a pig to sing. It wastes your time, and it only annoys the pig."

of a department of praxics, the department will have only itself to blame if it does not flourish.

(g) The establishment of an independent science of behavior will allow a realignment with the hard sciences, especially with the various branches of biology that are concerned with the controlling variables of behavior: evolutionary biology, ecology, ethology, physiology and anatomy, behavioral genetics, and so on. Biologists have largely ignored behavioral psychologists simply because behavioral psychologists are psychologists. We should attend to data and theories from *any* field—including economics, sociology, anthropology, psychology, and so on—that will help us advance our understanding of the subject matter, but our primary concern should probably be with biology.

(h) There have been occasional attempts in biology to synthesize information from various disciplines—primarily psychology, anthropology, and various branches of biology—that have concerned themselves with the behavior of organisms; the effort is sometimes called "behavioral biology" (e.g., Konner, 1982).[11] "Sociobiology" (Wilson, 1975) is a well-known, though controversial, variant. The creation of departments of praxics could help bring about this synthesis. The possible benefits to society would be enormous.

### *What about The Battle?*

"I am pigeon-livered," said Hamlet, "and lack gall/ To make oppression bitter." But proposing a split from psychology is not defeatism. The "battle of behaviorism" was to some extent won by the behaviorists long ago: Introspection lost its popularity early in the century and has remained suspect. A concern for objectivity in method and terminology has become part of the fabric of modern psychology, largely due to early behaviorism (Schultz, 1969). According to some, Watsonian behaviorism was so successful that it literally "conquered itself to death. It . . . has become a truism. Virtually every American psychologist, whether he knows it or not, is nowadays a methodological behaviorist" (Bergmann, 1956, p. 270).

Watson was fighting many battles at once, and some were won. But the subject matter itself has never been secured. The concern with objectivity is now applied to the traditional—and appropriate—subject matter of psychology.

## METHODS

Previous proposals for a science of behavior have neglected to include a Methods section, which may account for their fates. I offer a glimpse of activities that are underway toward the establishment of an independent science of behavior, as well as some pertinent historical data.

### *The Praxics Society*

In 1983 Paul T. Andronis, T. V. Layng, and I founded an organization called The Praxics Society. Andronis, who earned his Ph.D. under Israel Goldiamond and who is currently doing post-doctoral work at the University of Chicago, serves as Director.[12] (In the Chicago area, I'm told, the term "praxics" has shown up on exams, which makes it official.) After nearly a year of correspondence and discussion among interested students and faculty members, twenty of the Society's members met in Nashville in May 1984, during the meetings of the Association for Behavior Analysis. The membership consists—by design—mainly of young people, but a number of senior people in the field have been supportive of the concept.

The Society is setting up a *Science*-like journal called, naturally, *Praxics,* which will publish original reports of advances in the several biologically-oriented fields that contribute to our understanding of the behavior of organisms. The Society

[11] A good source of work in this area is the journal *Behavioral and Neural Biology,* which was founded in 1968 as *Communications in Behavioral Biology* and subsequently called simply *Behavioral Biology.*

[12] For further information about The Praxics Society, write to Paul T. Andronis, Director, The Praxics Society, 10226 South Artesian, Chicago, IL 60642.

is also planning a series of conferences and books that will also involve representatives from a number of different disciplines. These activities are meant to set the stage for the achievement of Society's primary goal: to bring about the first Department of Praxics by 1990. Is this hubris? How does one create—at least in name—a new field? Early psychologists no doubt asked precisely the same questions.

*On the Creation of Departments*

Modern academic psychology began as an area of specialization in philosophy departments. The first clear suggestions that psychology might have something in common with the natural sciences were made by Herbart early in the 19th century (e.g., Herbart, 1816, 1824–1825). Kant had insisted that psychology could never be a natural science, but Herbart asserted that psychology could be advanced by using two of the techniques of the natural sciences—observation and mathematics. Experimentation, he thought, was not possible (after all, the subject matter was immaterial). His successor at Göttingen, Hermann Lotze, agreed that experimentation on mind was impossible, but he took the extraordinary step of applying physiological and medical data to psychological issues (e.g., Lotze, 1852), which helped make psychological experimentation inevitable. His students, along with Wundt, Fechner, and others, performed the first such experiments in the 1850s. A new field, substantively different from philosophy, had been created, *but formal programs and departments to accommodate the new field did not immediately spring to life.*

The first formal program in psychology was probably Wundt's *Institute* at Leipzig, which was founded in 1879—*more than twenty years after he conducted his first psychology experiments.* Some landmarks in the United States are also notable: The first graduate degree in psychology was awarded to G. Stanley Hall at Harvard in 1878, but it was awarded by the Department of Philosophy, and Hall's research was done in the laboratory of a physiologist. Hall founded the first psychology journal in America, the *American Journal of Psychology,* in 1887, and the first professional organization, the American Psychological Association, in 1892. The world's first chair in psychology was occupied by James McKeen Cattell in 1887 at the University of Pennsylvania, where Cattell established what was perhaps the first university-sanctioned psychology laboratory in the country the same year (less official laboratories had been established previously at Harvard and Johns Hopkins).

Still, these things take time. For decades many psychology programs in the United States were still part of philosophy departments. At Harvard, the first semblance of a psychology department did not appear until 1913, as the Department of Philosophy and Psychology, still under a bureaucratic entity called the "Division of Philosophy"; before that only a Social Ethics Department existed under that Division. It was not until 1934 that separate departments of psychology and philosophy were created—still under a larger entity called the "Division of Philosophy and Psychology." (Note that the *Divisions,* not the Departments, had the authority to grant Ph.D.s.) Psychology did not truly become a separate entity with the authority to grant its own Ph.D.s until 1939, when the Division system was changed.[13] Skinner himself received his Ph.D. in 1931 from the Department of Philosophy and Psychology, under the authority of the Division of Philosophy.[14]

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[13] This information comes from volumes of the *Harvard University Catalogue* dated from 1912 to 1940.

[14] Skinner's dissertation, which is dated December 19, 1930, reads "A Thesis Presented in Partial Fulfillment of the Requirements for the Degree of Doctor of Philosophy in the Department of Psychology of Harvard University," but the Department of Psychology was not formed until 1934. E. B. Newman (personal communication, June 1984) has suggested that Boring, Skinner's advisor, may have been acting as if a Department of Psychology existed years before the fact. It is notable that Boring became the Department's first chairman.

In England some psychology departments were still part of philosophy departments in the 1950s. Harzem (1984) reported that he was part of the two-man team that planned the last such split in the United Kingdom—the creation of the Department of Psychology at the University of Wales, Bangor—in *1963*. And in Greece, Spain, and other enlightened countries in Europe, psychology departments are still uncommon. As of this writing, the University of Chicago has no psychology department; specialists in experimental psychology are awarded degrees in biology.

The point, I hope, is clear: We all come to preexisting departments, structures, edifices, programs, and so on, which, in some cases, are literally cast in concrete. *But we must never take these divisions and labels for granted; they were created by people, they are not necessarily the best means for promoting either a science or a technology of behavior, and they can be changed.*

## SOME FINAL COMMENTS

In the short time since The Praxics Society was founded, faculty members at three universities have expressed interest in establishing programs in praxics at their universities. That should not be surprising, since many senior faculty members in behavioral psychology have dreamed of such a field for decades, since many junior members are shell-shocked from the attacks of their cognitivist colleagues, and since many graduate students in behavioral psychology are looking forward to rewarding careers as computer programmers. No one doubts that the study of behavior is a legitimate enterprise. The question is simply how to bring such an enterprise fully to life.

Should established behavioral psychologists resign from the American Psychological Association and join the Society? By no means. The attempt to establish an independent science of behavior does not require an exodus from psychology. Many students of behavior are already invested in psychology, and, as usual, some will flourish. Praxics—if there is any merit in the idea—is for the young, for their futures. The Israelites had to wait in the desert for forty years before they could enter the Promised Land—though it was just over the next hill—until those among them who had been slaves had lived out their natural lives. Praxics can and probably should be established without the support and participation of people who have strong ties to psychology.

I can't think of a more fitting ending to this essay than Kuo's (1937) closing words, of which I have taken the liberty of altering only two:

> When I discussed with my colleagues my program for [praxics], I was often told: "Your prospectus looks fine, but it will be beyond the possibility of actual accomplishment, and as long as your ideal cannot be fully realized, we will have to rely on those old psychological concepts for the explanation of behavior." I wish to ask my readers for more indulgence if I relate an ancient Chinese fable about an old farmer. The farmer was known among his neighbors by his nickname, "Mr. Fool." He lived in a house which was right behind a hill. Displeased by the obstruction in front of his house, he started to remove the hill. All his neighbors laughed at him most heartily and called him "Mr. Fool." But despite the laughter and ridicule, Mr. Fool carried on. Once he told his neighbors, "I believe we shall be able to remove this hill. If I cannot finish it in my lifetime, I will make my children, grandchildren, and great-grand-children do it." When he died he stated in his will that he had buried all his fortune under the hill and the only way to get it out was to remove the entire hill. So generation after generation all his children worked feverishly on the hill. And in less than four generations, the old house had gained a clear view of the field. Perhaps this is a true story about some modern fools in science. Be it fact or fable, and fool or no fool, the [praxist] has planned to remove something much larger than a hill.

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