

# Comparative Psychology as the Praxist Views It

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The plight of modern comparative psychology is rooted in part in the destructive effect that early behaviorism had on the field. Early in the 20th century, Mercier, Dunlap, Kuo, and others proposed the creation of a new, multidisciplinary science devoted to the study of behavior. Watson derailed this effort by insisting that psychology should adopt behavior as its subject matter and that it should abandon the study of mind. Watson's proposal isolated the study of behavior from the biological sciences and led to an incessant and unproductive battle between behaviorists and cognitivists, in which the latter have emerged the victors. Because comparative psychology has remained for the most part the comparative study of animal behavior, it has suffered greatly both by the field's isolation from biology and by the emergence of a strong cognitive psychology. The comparative study of mind will undoubtedly flourish in modern psychology, but the comparative study of behavior should be part of a new, comprehensive, multidisciplinary science of behavior, along the lines suggested by Kuo. Efforts are underway to establish such a science.

A brief history of comparative psychology and ethology by Gilbert Gottlieb (1979) makes a rather provocative statement: "If psychologists had remained faithful to their early intellectual tradition, in which biological thinking was central, there would have been no need and no place for the discipline of ethology" (p. 163). By the 1930s many psychologists had abandoned cross-species comparisons in favor of studies of the white rat, and ethology, said Gottlieb, arose as a corrective. Moreover, the concept of instinct had been all but abandoned.

Others (e.g., Kalat, 1983; Snowdon, 1983) share Gottlieb's view that comparative psychology went awry—perhaps to the point of bringing about its own demise (Lockard, 1971; Snowdon, 1983)—because it lost its biological orientation. How we are to deal with this is not clear, but, understandably, most comparative psychologists want to keep comparative psychology going: by reorganizing (Scott, 1973), by learning from and responding to sociobiology (Chiszar & Carpen, 1980; Snowdon, 1983), by yet another realignment with ethology (Adler, 1980; Chiszar & Carpen, 1980; Glickman, 1980; Kalat, 1983; Snowdon, 1983), by biologizing (Gottlieb, 1979; Hodos & Campbell, 1969), by not biologizing (Flanagan, 1980), by "marriage" to related disciplines (Demarest, 1980), and so on.

I propose that we cast these issues into a new light. I agree that early in the century serious mistakes were made, but these were not the mistakes that Gottlieb identified. The mistakes to which I am referring preceded the early crises of comparative psychology and, I believe, set the stage for these crises.

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I agree with E. O. Wilson's (1975) unsavory suggestion that comparative psychology and ethology should be "cannibalized," but along completely different lines than the ones he suggested and for completely different reasons than the ones he gave.

My rationale has to do with some early events in the history of psychology that are rarely discussed these days but that still affect us profoundly. In researching these matters several years ago, I was pleased to find that most of the arguments I am about to make were made quite eloquently in 1937 by one of the most talented comparative psychologists who ever lived—Zing Yang Kuo.

I believe that the current predicament of comparative psychology can be best understood by reexamining behaviorism and its place in early psychology.

## Psychology

Before the advent of behaviorism, psychology was and always had been the study of mind, or, as the *Oxford English Dictionary* (OED) put it, "the science of the nature, functions, and phenomena of the human soul or mind" ("behavior" is not mentioned in the original OED), or, as Boring (1950) stated, the study of "the generalized, human, normal, adult mind." The word "psychology" comes from the Greek *psyche*, which originally meant breath but came to mean soul or spirit, because the absence of breath was a sign of the absence of life. All early psychological studies, without exception, were studies of mental phenomena: Wundt's and Fechner's early studies of psychophysical phenomena, Mueller's studies of attention, Ebbinghaus's classic studies of memory, Romanes's anecdotal studies of animal intelligence, Mills's developmental studies, Washburn's (1908) text on animal mind—all were concerned with the study of mind. Even Morgan's canon, which the behaviorists later pilfered for their own purposes, was a call for conservatism in theories of mind.

I labor this point because, with the birth of the field so far behind us, and with the modern field having only re-

cently—and rather noisily—emerged from the shadow of behaviorism, we tend to forget how clear the mission of psychology used to be. Psychology began as the study of mind, and comparative psychology began as the comparative study of mind.

Due in part to the coalitions that have been formed over the last few decades between psychologists, computer scientists, philosophers, and linguists, mainstream academic psychology has become, once again, the study of mind. Or perhaps I should say merely that today it is *obvious* that psychology is concerned with its original subject matter. I submit that mind has always been the primary concern of most psychologists. Behaviorism was a conspicuous cloak—certainly, the public, for many years, identified American psychology with behaviorism—but a quick tally of journals suggests to me that behaviorists were always a small minority in the field.

To deny that modern psychology is the study of mind seems to me to be sheer folly. For the last 5 years, the American Psychological Association *Monitor* has listed nearly one hundred ads a year for entry-level jobs in cognitive psychology; the December 1986 issue alone listed more than fifty. Only one or two each year have appeared for assistant professorships in behavior or animal behavior. I am personally aware of more than \$40 million in private foundation funds that have gone to cognitive psychologists—for example, to support centers for cognitive studies at M.I.T., Carnegie Mellon, and other universities where, on occasion, funds have supported as many as 40 postdoctoral fellows in a single year at a single university.

The trend is not likely to stop while any of the current readers of this journal are still in academe. A recent, high-level report undertaken for the White House by the National Academy of Sciences and the National Science Foundation singled out only 11 areas in all of science as being worthy of “incremental federal support” in years to come. Most of these fields, like materials science, chemistry, immunology, astronomy and astrophysics, computer science, and neuroscience, would not surprise you. But one of the 11 was cognitive science (Estes et al., 1983).

Comparative psychology, perhaps to its credit, has always been a bit behind the times. But—if it is to survive in psychology—it, too, I predict, will soon find its roots. In that sense I agree with Mason's (1980) rather straightforward statements in the *American Psychologist*: The study of mind, wrote Mason, is “the program which comparative psychology originally set for itself, and which remains a principal pursuit. . . . Comparative psychology is about the evolution of mental processes, and the various forms and aspects of minding in different animal species” (p. 964). “*Wanting and knowing*,” he continues, “are major interrelated themes in the evolution of minding” (p. 966). He concludes that since sociobiology is not concerned with mind, mind is the comparative psychologist's best protection against cannibalization. I agree completely, but for reasons that I will set forth below.

Mason's position is shared by many prominent psychologists. George Miller (1983) has praised Premack's (1983) recent work (e.g., Premack, 1983; Premack & Woodruff, 1978) as “a gratifying enrichment of the empirical resources

available to cognitive psychology” (p. 152) and credits him with “resuscitating comparative psychology” (p. 153). Griffin (1976, 1978), Gallup (1975, 1979), Savage-Rumbaugh (1981), Terrace (1984), Roitblat (1982), Sober (1983), and many others have called for the return of the comparative study of mind. The return seems inevitable.

### The Study of Behavior

Etymology notwithstanding, many current papers in comparative psychology are not concerned with mind. They are concerned with behavior as behavior—in behavior for its own sake—in what the organism does as something worth studying in its own right.

Some, like Hodos and Campbell's (1969) article on the *scala naturae*, or J. P. Scott's (1973) review in the *Annals of the New York Academy*, or Snowdon's (1983) excellent article in the *Annual Review of Psychology*, seem to be completely oblivious to the mentalistic origins of comparative psychology. Others, like Gottlieb's (1984) reply to Hodos and Campbell (1969), or Kalat's (1983) recent article on evolutionary thinking, seem to accept the fact that comparative psychology *used to be* concerned with mind but somehow came to be concerned with behavior more or less exclusively. Still others, like Demarest's (1983) scholarly article on continuity, are unclear about whether comparative psychology is concerned with mind or behavior. But, certainly, some of the towering figures in the area, like Schneirla and Lehrman, were concerned primarily with the study of behavior per se.

Clearly, comparative psychology, like the rest of the field, got sidetracked for a while from its original mission. How did this happen?

Inspired in part by the myriad of observations that were prompted by the theory of evolution, the late 1800s and early 1900s brought many calls for the formation of a new science, one devoted to the study of behavior. As early as 1843, John Stuart Mill suggested such a science, which, interestingly enough, he called “ethology” (modern ethology has no relation; Lorenz borrowed the term from his teacher, the German Heinroth). An accumulation of what McDougall (in Watson & McDougall, 1928) called “the facts of behavior” led to similar proposals by Mercier (1911) and Gibson (1904) in England, by Espinas (1890, 1897) in France, and by Sechenov, Bechterev, and Pavlov in the Soviet Union. Two zoologists, Parker and Haswell, suggested such a science in a textbook they published in 1897; they suggested the terms “bionomics,” and, once again, “ethology.” Each of the many proposals for the new science was accompanied by a new name. None of the names was ever widely adopted; some have resurfaced several times (Epstein, 1984).

Had these proposals been followed—had a new, comprehensive, biologically-based science of behavior been established early in this century—by now it would surely have become one of the most prestigious, most highly funded, and most beneficial fields in all of science. A new field, relatively unconstrained by dogma or methodology, intent simply on the scientific understanding of the many variables that determine behavior—on the effects of neural and chemical interventions, sleep and sleep deprivation, conditioning, evolution-

ary history, genes, nutrition, anatomy, physiology, and so on—would have been theory-rich, like kindred natural sciences, and would have yielded a myriad of practical applications.

But the science did not materialize. As I have documented in a series of recent papers (Epstein, 1984, 1985a, 1985b, in press-a, in press-b), two powerful personalities—William McDougall and John B. Watson—put the new science on a steep and thorny road.

In his little book, *Principles of Physiological Psychology* (1905), McDougall made a curious suggestion; namely, that psychology should adopt behavior as its subject matter. He repeated this suggestion in several subsequent books, but it was largely ignored. In 1913, however, a more flamboyant figure, John Broadus Watson, made this curious suggestion again, and, this time, he turned it into a full-fledged movement for the reform of psychology. That movement, appropriately, had an *ism* in its name: *behaviorism*. Watson even went so far as to forbid the study of psychology's traditional (and etymological) subject matter.

The suggestion was more than curious; it was absurd. It should have been ignored, as McDougall's statements had been. But Watson's personal style and his promises of practical applications made the impossible happen. He repeated his manifesto the next year in a book entitled *Behavior: An Introduction to Comparative Psychology* (so much for the comparative study of mind!), and, less than a year later, in 1915, at age 37, he was elected President of the American Psychological Association.

At least in some circles, psychology now had a new image. It was to be a natural, empirical science of behavior. But the old subject matter never went away, and neither did the old, very negative, public image, in spite of valiant efforts by Hunter, Dunlap, Hull, Tolman, Weiss, Skinner, Schneirla, Lehrman, and many others. Students of behavior have entered psychology for more than 70 years only to find that it is not a natural, empirical science of behavior, that funding, jobs, and resources are scarce, and that nonpsychologists are more likely to ask them about ESP or the id than about the latest advances in the science of behavior.

Incoming students of behavior continue to receive inadequate training, the vast majority of which is usually in traditional psychology. They lack training in the many biological disciplines that are concerned with behavior—behavioral genetics, ecology, ethology, behavioral pharmacology, behavioral neurophysiology, and so on—and, most important, they lack training in mathematics. The importance of mathematics to the advance of science is self-evident to anyone with training in the history of science. Even Herbart, in one of the very first proposals for a scientific psychology, published in German in 1822 and in English in 1877, made this point forcefully.<sup>1</sup> It is, in my opinion, undeniable, and we must stop weaving our way around this issue.

Watson tried to reform a field that did not want reforming. Though behaviorism failed in its mission to oust the old subject matter, Watson's soldiers refused, against overwhelming odds, to surrender. (I apologize for the military metaphor, but I am not the first to use it: Consider King's *Behaviorism: A Battle Line!* [1930] and Watson and McDougall's *The Battle*

*of Behaviorism* [1928].) Over the years, Watson's original tenets were repeated and elaborated until behaviorism emerged as an intricate philosophy—a complex set of inter-related assumptions and assertions, all bent on explaining *why students of behavior should be allowed to take over psychology departments*.

Had the movement succeeded, this elaboration would never have occurred; instead, we would now have a flourishing science.

One of the most poignant calls for the formation of such a science came from Zing Yang Kuo, the ardent behaviorist whose scathing attacks on instinct (Kuo, 1921, 1930) and whose innovative research on the behavior of the chick embryo (e.g., 1932, 1933) made him widely known at a young age. In 1937, in *The Journal of Psychology*, Kuo published a brilliant article called "Prolegomena to Praxiology," which was inspired, he said, by "the half-heartedness of the behavioristic revolt, and its inability to make a decisive break with the traditional psychology" (p. 5).<sup>2</sup>

Kuo called the new science "praxiology," following Dunlap, who, in turn, had borrowed the term from Mercier, whom I mentioned earlier. It is, he said,

*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. (pp. 5-6)*

The outcome of the science, said Kuo, will be "a thoroughly scientific description of behavior in purely mathematical and physical terms" (p. 6). How tragic, as Gottlieb (1972) pointed out, that a man of Kuo's talent—a brilliant laboratory researcher, a man who understood the importance of a developmental approach, of physiology, of biology, and of mathematics—how tragic that political events in China kept him out of science for more than 40 years. What a loss for us all.

### Predicament and Solution

Our mistake, years ago, was not in abandoning biological thinking, as Gottlieb (1979) suggested. *Our mistake was in isolating the study of behavior from biology in the first place.* Comparative psychologists—most of whom are not really psychologists in either the traditional or the modern sense—have suffered acutely from this error.

In 1983, with Paul T. Andronis and T. V. Layng of The University of Chicago—notably, the only major university in the country with no psychology department—I helped to found The Praxics Society,<sup>3</sup> the purpose of which is to establish a new multidisciplinary field devoted to the study of behavior in all its aspects. Among other things, the Society is

<sup>1</sup> Herbart (1822) claims that he first presented this position in 1806.

<sup>2</sup> His position was elaborated further in a book in Chinese, which, thanks to Gilbert Gottlieb, I recently received as a gift from Kuo's widow in Hong Kong. A translation is in progress.

<sup>3</sup> "Praxics," a blend of "physics" and *praxis*, the Greek word for behavior, is a term I and others now use for the study of behavior. "Praxist," after "chemist," is the practitioner.

working toward the establishment of a *Science*-like magazine that will serve the needs of the many scientific disciplines that are concerned with the scientific understanding of behavior. Every existing program and department in every university was started by an effort of this sort; the new field is no pipe dream.

There is no more important subject matter on earth than the behavior of organisms. Society needs to be taught this, and it also needs to provide generous support for a comprehensive, multidisciplinary effort to understand behavior. Watson put the science of behavior on a steep and thorny road. The time has come to get it back on track.

I can think of no more fitting an ending for this essay than the closing words of Kuo's (1937) own proposal for praxiology. I have altered only two words.

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. . . ." I wish to ask my readers for more indulgence if I relate an ancient Chinese fable about an old farmer. . . . He lived in a house which was right behind a hill. Displeased by the obstruction in front of his house, he started to remove this 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 that 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. (pp. 21-22)

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