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## Defining Psychology: Is It Worth the Trouble?



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Henrique's thoughtful effort (this issue, pp. 1207–1221) to define psychology suffers from at least three shortcomings: (a) "psychology" is almost certainly an inherently fuzzy concept that resists precise definition; (b) attempts to define psychology are likely to hamper rather than foster consilience across scientific disciplines; and (c) Henrique incorrectly diagnoses the cause of the scientist–practitioner gap and hence offers an incorrect prescription. The sources of this gap lie not in intractable definitional disputes, but in fundamentally different approaches to acquiring knowledge. © 2004 Wiley Periodicals, Inc. *J Clin Psychol* 60: 1249–1253, 2004.

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Several years ago a professor who teaches psychology at a large university had to ask his assistant . . . to take over the introductory psychology course for a short time. The assistant was challenged by the opportunity and planned an ambitious series of lectures. But he made a mistake. He decided to open with a short definition of his subject. When the professor got back to his classroom two weeks later he found his conscientious assistant still struggling to define psychology. (Miller, 1966, p. 15)

As Henrique notes in his thoughtful and thought-provoking essay "Psychology Defined" (this issue, pp. 1207–1221), the field of psychology is in disarray. The fragmentation of the American Psychological Association (APA) into 53 divisions, some of which are further divided into sections, is emblematic of our disunity. This observation is, of course, not new. Over four decades ago, Cronbach (1957) described the typical APA convention in terms that may strike many of us today as charitable:

The scene resembles that of a circus, but a circus grander and more bustling than any Barnum ever envisioned—a veritable week-long diet of excitement and pink lemonade . . . This 18-ring display of energies and talents gives plentiful evidence that psychology is going places. But whither? (p. 671)

Henrique further points out that the increasingly worrisome scientist–practitioner gap (Fox, 1996; Tavis, 2003) is yet another manifestation of psychology's lack of disciplinary coherence.

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Henriques' central thesis is that the "absence of a clearly defined subject matter has been a key to psychology's problems" (this issue, p. 1211). As a corollary, he posits that this absence has contributed to the scientist-practitioner gap and that "a precise definition will open the pathway for a much more harmonious dialogue" between researchers and clinicians (this issue, p. 1209).

But I am doubtful on both counts. In the brief space available, I will focus on three significant shortcomings of Henriques' analysis. I will argue that although Henriques' attempt to decrease the intensity of internecine warfare roiling our profession is laudable, it is unlikely to succeed.

### Can Psychology Be Defined?

For starters, I take issue with Henriques' contention that sciences more advanced than psychology, such as biology, have resolved their definitional quandaries. According to Henriques, "Biology is a unified discipline precisely because it has a clear, well-established definition (the science of life) . . ." (this issue, pp. 1209-1210) as well as a circumscribed subject matter and overarching theoretical framework.

But in fact, biologists have never really settled the issue of how to define their subject matter, namely life. For example, biologists have yet to achieve consensus on whether a virus, a prion, a day-old embryo in a Petri dish, or even a self-replicating computer program, are alive (Angier, 2001). As Medawar and Medawar (1983) observed:

A great many nonbiologists believe that animated and contentious discussions of the definition of "life" are a principal preoccupation of institutes and university departments of biology. In reality, the subject is not mentioned at all, except to disparage . . . people who believe that an agreed-on definition of life will lead to a better comprehension of biology . . . A hunger for definitions is often a manifestation of a deep-seated belief . . . that all words have an inner meaning that patient reflection and research will make clear . . . indeed, amateurs will sometimes [ask]: "What is the true meaning of the word 'life'? There is no true meaning. There is a *usage* that serves the purposes of working biologists well enough. (pp. 66-67; emphasis in original)

If life itself has not been defined clearly, then the "science of life" (Henriques, 2004, p. 1210) similarly has not been defined clearly. Yet biologists do not lose sleep over this definitional ambiguity; they merely go about their business without worrying about it (Lilienfeld & Marino, 1999). Similarly, pharmacologists do not lose sleep over the fact that the word "drug" has no precise definition (Gorenstein, 1984). For example, is caffeine a drug? What about nicotine? Definitive answers are elusive and perhaps impossible.

I suspect strongly that "psychology," much like many other mental concepts, is an inherently fuzzy concept that resists precise definition (see also Reber, 1995). It seems likely that the remarkably diverse subdisciplines comprising psychology are linked loosely by family resemblance, similarity to an exemplar, underlying conceptual similarity (see Komatsu, 1992; Murphy & Medin, 1985; and Rosch, 1978, for reviews of these models), or perhaps a combination of all three. As a consequence, efforts to define psychology precisely, such as those of Henriques, are no more likely to succeed than are efforts to define precisely many other natural language concepts, including life (Medawar & Medawar, 1983), drug (Gorenstein, 1984), mental disorder (Kirmayer & Young, 1999; Lilienfeld & Marino, 1995, 1999; but see Wakefield, 1999, for a different view), and species (Levin, 1979). None of these concepts has proven amenable to a strict "classical" definition (see Komatsu, 1992) consisting of singly necessary and jointly sufficient features.

### Will Defining Psychology Bring About Greater Unity Among the Sciences?

Even putting aside questions of whether psychology can be defined precisely, it is unlikely that Henrique's effort to strictly delimit psychology from other scientific domains will bring about the long-sought consilience (Wilson, 1998) between psychology and disciplines both above and below it in Comte's (1830–1842) hierarchy of the sciences. To the contrary, such definitional attempts may paradoxically encourage turf warfare among disciplines adjoining psychology, such as sociology, anthropology, ethology, neuroscience, and economics. That's because such attempts are likely to embroil psychologists—and psychology departments—in prolonged and ultimately futile disputes over whether a given scientist's research area “really” falls within the domain of psychology rather than another substantive area (e.g., neuroscience).

Wilson (1998) noted that the social sciences are “easily shackled by tribal loyalty” (p. 199) and that they “stress precision in words within their specialty but seldom speak the same technical language from one specialty to the next” (p. 198). Far more important than a precise definition of psychology is a concerted effort to bridge the vertical gaps across disciplines that provide differing levels of explanation for behavior.

### The Scientist–Practitioner Gap and Its Origins

Henriques contends that the principal source of the scientist–practitioner gap is the failure to define psychology. Furthermore, he maintains that this failure underlies many clinicians' reluctance to embrace cognitive-behavior therapy (this issue, p. 1214), which as Henriques correctly observes, is a conceptually muddled oxymoron. Henriques also attributes the increasing exodus of research psychologists from APA into more scientifically oriented organizations, such as the American Psychological Society, to the same source. Nevertheless, I believe that Henriques' analysis misses the mark.

The principal origin of the scientist–practitioner gap lies not in the absence of a precise definition of psychology, but in the markedly different approaches of researchers and certain clinicians to acquiring knowledge. McHugh (1994; see also Wood, Nezworski, Lilienfeld, & Garb, 2003) argued persuasively that much of the split between science and practice in psychology and psychiatry could be traced to the sharp difference between two epistemic attitudes: *empiricism* and *romanticism*. Empiricists believe that questions regarding human nature are best settled by scientific evidence, whereas romantics believe that such questions are best settled by intuition. Although research evidence and clinical intuition sometimes yield identical answers, when they diverge acrimonious splits often ensue. Although Meehl's (1954) classic “little book” on clinical versus actuarial prediction demonstrated that scientific evidence almost always trumps or at worst matches subjective clinical judgment (see Grove, Zald, Lebow, Snitz, & Nelson, 2000, for an updated quantitative review), many practitioners from a romantic tradition continue to rely on clinical judgment even when well-validated statistical formulas are available.

There is ample reason to believe that the scientist–practitioner gap is traceable largely to a more fundamental rift between the empirical and romantic traditions. In a survey of 407 members of professional psychological organizations, Nunez, Poole, and Memon (2003) found that the majority of clinical psychologists believe that “alternative ways of knowing, for which the scientific method is irrelevant” should be “valued and supported in the practice of clinical psychology” (p. 12). In contrast, few nonclinical psychologists hold this view (see also Kimble, 1984). It seems unlikely that a sophisticated integration of behavioral and cognitive perspectives, such as that offered by Henriques (this issue), will bring extreme romantics back into the empirical fold, because most extreme romantics

reject the assertion that scientific evidence (including controlled findings on the efficacy of behavioral and cognitive-behavior therapies) should be the ultimate arbiter of psychological disputes. Nor is it likely to bring many APS members back to APA, because most of the former members feel that they no longer feel at home in an organization that places a relatively low premium on empiricism.

The principal solution to the scientist–practitioner debate lies not in improved definitions of psychology, but in more rigorous education and training of aspiring clinical psychologists in the philosophy of science and scientific methodology (Lilienfeld, Lynn, & Lohr, 2003). More specifically, to narrow the growing scientist–practitioner gulf, we must train future clinical scientists to appreciate the proper places of romanticism and empiricism within science. In the hypothesis generation phase of science, we should feel free to be romantics: we should think big and dream big. But in the hypothesis-testing phase of science, or what Reichenbach (1938) termed the “context of justification,” we must become empiricists and subject our armchair conjectures to the crucible of rigorous scientific tests. Once the deep rift between romantics and empiricists disappears, the seeming need for a precise definition of psychology will disappear along with it.

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