

In Response

The Debate About Praxics: Some Comments Meant Especially for Students

Robert Epstein

Boston University and Cambridge Center for Behavioral Studies

Academic debates are, understandably, much like faculty meetings. They are pointless, interminable, and exasperating. We half-listen and half-read. Other people's papers set the occasion for monologues. We accuse our colleagues of misunderstanding and then misunderstand in turn. Scholars are, it would seem, essentially autistic.

Consider the debate about praxics. In 1984, in a paper called "The Case for Praxics" (hereafter, simply "Case"), I proposed the creation of an independent, comprehensive science of behavior, and I offered the term "praxics" as a name for the new science (Epstein, 1984a). I also offered a laundry list of reasons for distinguishing between praxics and behaviorism, which is a school of philosophy, and I offered another list of reasons for allowing praxics (the study of behavior) and psychology (the study of mind) to go their separate ways. I reviewed several earlier proposals along these lines; a forceful proposal by Kuo (1937), entitled "Prolegomena to Praxiology," was especially insightful. Finally, I documented a century of unsuccessful attempts to name a science of behavior.

Here are some of the criticisms that have been leveled against the "Case" paper, along with my comments—I claim the right of reciprocal autism.

The science of behavior, or at least "behavior analysis," is in good shape. A paper by Deitz (1986) asserts that those who fear for the health of the field have presented "no data" (p. 66), and a paper by

Wyatt, Hawkins, and Davis (1986) reports that "behaviorism" is a "vital, growing area of behavioral science" (p. 103). But it is the Deitz paper that contains no data (it also has no references), and the Wyatt et al. figures are misleading. Wyatt et al. note, for example, that in 1984 the Association for Behavior Analysis (ABA) had a membership of 1,946. But, according to the ABA membership office in Michigan (personal communication, January 29, 1987), the current membership of ABA is 892; the figure is inflated briefly at each annual meeting perhaps because it is cheaper to register and join than it is to register without joining. The 1986 post-meeting peak was 1,852, or 94 below the 1984 figure.

Other facts are also disturbing. "Case" spoke about funding patterns, for example, which show modern psychology to be wholly dominated by the study of mind. And for some years I have been tabulating job listings in the *APA Monitor*, which are good indicators of the composition of future psychology departments. The September 1985 listings have been typical for nearly a decade: entry-level, tenure-track jobs in cognitive psychology are listed, many at prestigious schools, including Yale, Stanford, Johns Hopkins, Cornell, and Harvard; no such listings appear in animal learning, behavior analysis, or any related area (there was no position appropriate to my training, for example). In preparing this article, I did a quick count on the December 1986 issue: 55 entry-level professorships include the words "cognitive" or "cognition" in their descriptions.

Finally, in "Case" I noted that behavior analysts (members of Division 25) comprised less than 3 percent of the

I thank S. D. Roberts for commenting on the manuscript. I can be reached at the Cambridge Center for Behavioral Studies, 11 Waterhouse Street, Cambridge, MA 02138.

membership of the American Psychological Association; according to the 1986 *APA Membership Register*, that figure is now well below 2 percent.

But whether or not behavior analysis is flourishing (it isn't) is beside the point. I have not called for the growth of behavior analysis, but rather for the creation of an *independent, multidisciplinary, biologically-based science of behavior*. The misguided efforts of William McDougall and John B. Watson (Epstein, 1984a, 1985b, 1986a, in press-a, in press-b, in press-c) have kept us on the wrong track for more than half a century; no true science of behavior has emerged. We are wont to say that behavior is "multiply determined," but we study only a trivial assortment of variables, and our clinical impact remains small (Lindsley, 1985). Those of you who work in clinical environments (I have done so part-time for the last three years) know the agony of our ineffectiveness. We do not cure; we "modify," trivially. For patients who have suffered a brain injury, or who have endured long-term substance abuse, or who are clinically depressed, or who are "retarded," or "autistic," or "schizophrenic," we can do virtually nothing—or at least nothing that lasts.

A true science of behavior must be *multidisciplinary* (not "interdisciplinary," as Ator [1986] suggests), because behavior is a complex subject matter that requires the *joint efforts* of individuals in many specialties, both to advance our understanding and to devise effective treatments. Behavior is affected profoundly by nutrition, physiology, sleep deprivation (if I had had more than 3 hours of sleep last night, what would I be writing now?), sexual deprivation and trauma, chemical interventions, social phenomena, surgical interventions, physical trauma, anatomical variables, organic disease, hormonal cycles, air temperature, humidity, illumination, airborne chemicals, radiation, electrical stimulation, genes—and, of course, learning history. It is not folly to think that individuals with different specialties can be brought together to build a new

science; it is folly to think that the handful of scientists who now study behavior in almost complete isolation from each other in a dozen different disciplines can advance our understanding significantly.

Last year I gave a talk called "Praxics in the Year 2000" (Epstein, 1986b); I suppose we'd have to dub the talk science fiction at this point. I described a large, diverse, university department (not unlike the physics departments I was exposed to in my undergraduate days) that was devoted to the scientific understanding of many aspects of behavior. Faculty meetings were still exasperating, but at least no one had any doubts about the identity of the subject matter of their science. All aspects of the science were quantitative and formal, as is typical in all natural sciences. Methods and instrumentation were as diverse as the specialties. A glassy-eyed theoretician labored to synthesize information from different specialties, keeping one glassy eye on Stockholm. As is true in all mature sciences, department members had widely differing religions, philosophies, credos, ethics, morals, and politics; the differences kept certain people from becoming friends but not from advancing an understanding of their subject matter.¹ Advances in the basic science stimulated the development of new and effective technologies; with new team-treatment approaches to severe behavior disorders, even schizophrenia seemed to be giving way. The prestige of pure science, driving real and promised applications, and uncluttered by an irrelevant and unattractive credo, secured large-scale funding commensurate with that of other important sciences.

We have wasted our time trying to wrest

¹ Fraley and Vargas (1986) assert that praxics "sounds more like a movement for a political party than a scientific discipline" (p. 56), but, curiously, it is they who insist that analysts of behavior must be card-carrying believers in an ism ("radical behaviorism"), and it is they who would continue to limit the study of behavior to the narrow range of variables and methods typical of the operant approach.

psyche-ology buildings away from psyche-ologists. Behavior is an important subject matter in its own right—and it deserves and can have its own buildings! I grieve when senior members of our field (e.g., Skinner, 1986) urge students to continue fighting Watson's old Battle. Yes, certain *individuals* have managed, now and then, after the second or third ulcer, to make their way in psychology, but the *study of behavior* has not flourished, and it has remained hopelessly isolated from the natural sciences.

I have never advocated that individuals who already have a stake in psychology should abandon their careers; toward the end of "Case," I even suggested that such individuals have no place in the new science. I encourage Deitz (1986) to continue to support the "valuable goals" of the behavior analysts in the APA, for the goals are indeed valuable. I applaud Staats (1986) for his continuing efforts to reinterpret psychological concepts in behavioral terms.² I suggest, however, that both Staats and Deitz would find more funding, more resources, more space, more students, more colleagues, more stimulation, and more sympathy in a Department of Praxics than in a Department of Psychology. (Alas, only the latter exists at the moment.)

"Praxics" is an "awful" name, according to Deitz (1986), and so is "behaviorology," a name for behavior analysis which both Los Horcones (1986) and Fraley and Vargas (1986) seem to claim (it seems safe to say that the term was considered and rejected by others a half century ago). Barry (1986) thinks "praxics" is "simply awkward and limiting in scope" (how is it limiting?), and Gaydos

(1986) objects to "praxics" because it is a near-homologue of "praxis," which, he says, "has such a long history of divergent uses that its presence as the root of 'praxics' may be 'praxics' [sic] undoing" (p. 229).

Well, beauty is in the eye of the beholder, of course (say "statistics" three times fast), and near-homologues and even homologues do not undo each other, except perhaps among young children. "Astrology" does not undo "astronomy," "english" (a spinning motion given to a ball) does not undo "English," "scientologist" does not undo "scientist" (fortunately), and "physic" (a medicine to induce vomiting) does not undo "physics." I was well aware of a variety of modern usages of "praxis" when I wrote "Case" (see p. 102, footnote 2); they are, I believe, of no consequence.³

"Repetition," a professor of mine used to say (whenever he lost his place in his lecture notes), "is the mother of wisdom." So let me repeat a point I have made repeatedly (e.g., Epstein, 1984a, 1985a): "Praxics" is not a name for the experimental analysis of behavior or behavior analysis; it is a name for *the study of behavior*. "No methodology or *weltanschauung* is implied" (Epstein, 1985a, p. 271).

Finally, I have no strong attachment to the word "praxics" per se, but rather I believe there is a pressing need for the new science, and I believe further that the new science needs a name. Since 1985 I have been working with A. Deen, a talented Harvard student, on a paper we call "Five Hundred Names for the Science of

² Staats (1986) is incorrect, however, in his assertion that praxics "as . . . movement may be considered to be in the tradition of the separatism of the disunified science" (p. 233). Quite the contrary. Praxics is an attempt to unify many isolated scientific approaches to the study of behavior. An effective understanding of behavior can never be reached without a multidisciplinary effort; praxics is the personification of such an effort, and Staats offers no comparable alternatives.

³ The Gaydos (1986) paper also includes a rather unkind comment: "The example set by Skinner's careful choice of a name befitting an analysis of language does not appear to have been followed in the choice of the term 'praxics'" (p. 230). But I spent more than two years investigating and considering various names before proposing "praxics," and "Case" included more than 80 references to relevant sources. Since proposing "praxics," I have spent an additional two years systematically generating hundreds of alternatives. As of this writing, "praxics" is still the frontrunner, although some intriguing competitors have emerged.

Behavior." We have researched more than 30 languages and consulted with several distinguished linguists to find suitable names; a computer program has multiplied our efforts almost to the point of absurdity. By the time we are ready to submit the paper for publication (in the summer of 1987, most likely), we hope to have enough names to cover every last member of ABA, and therefore everybody can be happy.

Praxics needs to drag along an ism with it, assert Fraley and Vargas (1986). Similar arguments were made in papers by Leigland (1985) and Malagodi and Branch (1985). I cannot, in the short space allowed me here, do justice to my position on this matter; I stand by the original critique of isms I made in "Case," as well as by my response to Leigland and to Malagodi and Branch (Epstein, 1985a), and I urge the concerned reader to consult these papers.

One of the people who first got me thinking about the need for a new science was Fred S. Keller. As I noted in "Case," Keller has gently asserted the need for a new science for many years, and he has offered "praxiology," a term he heard in 1928, as a name for the science. In a talk in 1984, Keller commented that the only thing that remains to be done for the science of behavior is to "eliminate the ism," and he is right. Behavioral genetics, ethology, behavioral neurology, and behavioral pharmacology are unfettered by formal isms; "behavior analysis" should be similarly unencumbered, and a new, comprehensive, multidisciplinary science must be pure, unencumbered science. Let the philosophers philosophize. And let's make sure we give them something important to write about.

I will now embarrass myself by sharing with you some recent New Year's resolutions. Please don't hold me to them; that will only make me nervous, and then I will surely break them.

1) *After I submit this paper, I will not participate in the debate about praxics any further.* Rather,

2a) *I will continue to try to help bring about the creation of a comprehensive sci-*

ence of behavior: by organizing and helping others to organize projects that promote such a science; by contributing time, money, and resources to such projects; and by speaking positively, especially to students, about the merits of the concept.

2b) *Until a better term emerges, I will continue, frequently, to call the new science "praxics," following the dictum "use it or lose it."* (I squeeze the term into almost everything I write, and I even slipped "praxist" into *Nature* in 1984 [Epstein, 1984b].)

3) *I will continue to support the growth of the Cambridge Center for Behavioral Studies, an advanced studies institute and library devoted to "advancing the study of behavior and its application in the solution of practical problems and the prevention and relief of human suffering."* By providing new resources, visibility, and funding for the many scholars, scientists, and practitioners who are concerned with the study of behavior and behavior change, the Center will, I believe, help set the stage for the development of a comprehensive science.

4) *I will continue, through research, to try to advance the scientific understanding of behavior,* and I will make greater use of formal and quantitative methods than I have in the past.

Half my age ago, I sat under a palm tree reading with rapture a thin little book by a woman who claimed that the methods of science had been applied with some success to the study of behavior and that new, humane technologies of behavior change were emerging.⁴ The world was certainly in trouble, and I had long dreamed, as we all do, of a world free of the threat of nuclear war, free of hunger, free of pollution, and free of needless suffering. Science, it seemed to me, was our only hope for realizing such a dream, and I eventually decided to devote 10 years of my life toward furthering that realization. My 10 years were up a few weeks ago, but I have decided to carry on. If you would like to help, drop me a line.

⁴ She served as president of ABA a couple of years ago.

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