Tactics of Scientific Research at 30: Some Personal Reflections

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I first read Sidman's classic, *Tactics of Scientific Research*, as a beginning graduate student at the University of Florida in 1973. I was a recent but enthusiastic convert to radical behaviorism, largely as a function of reading Skinner and taking classes with Hank Pennypacker, Ed Malagodi, Marc Branch, Mark Goldstein, and Richard Willis. At the time I read *Tactics*, my exposure to scientific methodology was limited to undergraduate courses in chemistry, physics, and zoology and two graduate statistics courses. In the undergraduate science courses, later referred to as "hard" or natural sciences in a spurious and somewhat demeaning contrast with "soft" or social sciences, we were so busy memorizing facts and replicating experiments in the obligatory laboratory sections that we seldom considered the role of methodology and philosophy in the development of science. Through the statistics courses, I developed a reasonably thorough understanding of group comparison research designs and inferential statistics but I never questioned the logic of this research approach; after all, the ubiquity of inferential statistics in psychology research (with the perplexing exception of a handful of behavior analysis research articles that I had read) suggested that statistics was one of the touchstones of scientific psychology.

Against this backdrop, *Tactics* was a revelation, actually a series of revelations. The concept of establishing the reliability of an experimental result through direct replication and assessing the generality of research findings via systematic replication seemed to be a remarkably logical alternative to the strategies that characterized inferential statistics. Sidman's advocacy of single subject research strategies and inductive reasoning was not without precedent. Bernard (1865/1957) addressed many of the same issues in *An*

Introduction to the Study of Experimental Medicine, a book that Sidman read only after having written *Tactics* (Thompson, 1984). Moreover, some of the experimental strategies that Sidman championed were becoming increasingly evident in the experiment analysis research of the day (see the *Journal of the Experimental Analysis of Behavior*). It was Sidman, however, who most clearly articulated these strategies and argued for their relevance to the scientific study of behavior. The arguments in *Tactics* were so thorough and persuasive that my fellow graduate students began to refer to it with a mix of affection and reverence as "The Bible." The analogy was not without merit in that we students frequently referred to *Tactics* for guidance in the design and conduct of experiments and for a paradigm with which to understand experimental results.

Tactics addresses a number of issues that are central to experimental methodology and epistemology. Among these are the concepts of reliability and generality, direct and systematic replication, steady state experimental strategies, and inductive reasoning. While each of these concepts were important contributions to behavior analysts and scientific research methodology, it was the manner in which Sidman justified specific methodologies that had the most profound impact on me. His discussion of major concepts is set in a philosophy of science context with emphasis on issues and strategies that are generic to all sciences. Sidman's extrapolations from these larger issues to specific applications and examples in psychology helped me to develop an appreciation for the underlying logic of research methodology. Had I learned research methodology by simply memorizing specific research designs or measurement strategies (often referred to as a "cookbook" approach), I believe that

my ability to derive sometimes creative solutions to unique and unpredictable research methodology problems would have been greatly hampered.

Moreover, Sidman's focus on underlying scientific research strategies gave me a strong sense of the continuity of the scientific enterprise across disciplines and across time within a discipline. This is nicely illustrated by a short anecdote that Sidman relates while discussing the role of replication in science. He describes a paper that he wrote as a graduate student for W. N. Schoenfeld who pointed out that the paper was written from an unusual perspective in that Sidman had emphasized the *differences* between Sidman's work and prior research. Schoenfeld noted that science does not ordinarily advance in that manner leading Sidman to conclude that "it is the job of science to find orderly relations among phenomena, not differences. It would have been more useful if I could have pointed out the similarities between my work and previous experiments" (Sidman, 1960, p. 15). This anecdote leads to the perspective that science is a collaborative enterprise (although often punctuated by competition among individual scientists) with many individuals contributing to a common goal (i.e., knowledge or technology). Those of us who have been fortunate enough to participate in this enterprise (either as researchers, interpreters of research, or appliers of research findings) can take pride in our contribution, however large or small, to this grand enterprise.

Finally, Sidman's focus on the philosophy of science that underlies behavioral research methodology convinced me that I was a scientist first and a behavior analyst second. I believe that the identification of basic principles of behavior and, to a certain degree, the emergence of applied behavior analysis, is an inevitable result of applying scientific research strategies to behavior as a subject matter. But future scientific research may (a) discover as yet unidentified principles of behavior, (b) prove some currently accepted principles to be incorrect (an unlikely event in my estimation), or (c) identify limi-

tations of contemporary principles and propose refinements (note the analogy between Newtonian physics and quantum mechanics). Thus my primary allegiance is to science, whose research strategies and philosophy are the forces driving the evolution of behavioral psychology. I certainly hope that twenty years hence we can look back on contemporary behavior analysis (circa 1990) and shake our heads at how naive we were in 1990 and how far we've progressed.

While I have nothing but praise for *Tactics* as an intellectual tour de force and appreciation for its impact on behavioral psychology, there are two topics that I wish Sidman had addressed in the original version of *Tactics* and numerous topics (only one of which I will mention) that have emerged since the publication of *Tactics* that I wish he would address were he inclined to revise *Tactics*. In the interest of brevity, I will pose them as questions for Dr. Sidman.

First, is the subject matter of the science of behavior sufficiently analogous to the subject matter of other sciences (e.g., chemistry, physics) that we can extrapolate research strategies from these sciences? Or are there unique characteristics of behavior as a subject matter (e.g., its ephemeral nature, the relevance of *historical* controlling variables) that renders behavior a more difficult topic for scientific study than, for example, the study of chemical compounds. If so, what accommodations in scientific strategies are necessary?

Second, do some sciences progress faster than others because the scientists in that field are asking sequences of high yield experimental questions in what Platt (1964) refers to as the method of "strong inference"? How can aspiring scientists decide which experimental questions are truly important and worthy of pursuit? What topics in applied and experimental behavior analyses are most likely to yield major advances?

Finally, with the emergence of applied behavior analysis, a number of people have argued for the use of social validation methods in the selection of target behaviors and the use of consumer sat-

isfaction measures in evaluating the effectiveness and acceptability of behavior change interventions (e.g., Fuqua & Schwade, 1986; Kazdin, 1977; Wolf, 1978). What is the appropriate role for subjective measures and the opinions of non-scientists in an experimental or applied science?

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