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READING AND WRITING ABOUT RESEARCH METHODS IN BEHAVIOR ANALYSIS: A PERSONAL ACCOUNT OF A REVIEW OF JOHNSTON AND PENNYPACKER'S STRATEGIES AND TACTICS OF BEHAVIORAL RESEARCH *(2ND ED.)*[1] *AND OTHERS*

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Why would anyone want to write a book on research methods in behavior analysis? Better yet, why would anyone want to read one? Let's begin with the second question first.

Why Read a Book on Research Methods in Behavior Analysis?

Historical contexts. Toward the end of my graduate education at Arizona State University (circa 1965), Montrose Wolf suggested that I read Murray Sidman's *Tactics of Scientific Research* (1960) (hereafter, *Tactics*). At the time, I was working with Harold Coppock on shock-escape conditioning preparations, in which standard practice was to observe responses on single trials and average the data across subjects. Coppock was interested in mathematically modeling acquisition functions (e.g., Coppock & Osborne, 1965). I think Wolf thought that reading *Tactics* would expand my horizons, and he was right. I don't recall a particular "aha" experience at that time, but I do remember feeling that here was a clear description of the scientific experience in behavior analysis. The book made sense because it put words to my own experiences, which, at that time, seemed as much contingency shaped as they were rule governed. The conversion in worldview that I underwent to become a behavior analyst was catalyzed by *Tactics*. It was and is a great book. But in saying so, I need to put the experience further in context.

At the beginning (1962), the PhD program at Arizona State was experimental analytic. At that time how could it be otherwise? With few exceptions (e.g., Fuller, 1949), there was little applied behavior analysis: There were neither texts in behavior modification nor a *Journal of Applied Behavior Analysis*; Ayllon and Michael (1959) provided an isolated example of applied work. At Arizona State, Art Staats had just begun his work in reading (e.g., Staats, 1965). As students we discussed a preprint of Dickey (Wolf, Risley, & Mees, 1964) in Jack Michael's experimental analysis class, the textbook for which was the then-current volume (Vol. 4) of *JEAB*. Several of my student colleagues were in the field attempting endeavors similar to those employed in Dickey's study. Thus, *Tactics* fit like a master key into many of the current locks. As a key, it opened experimental analysis because it provided reasons for our own responses: reasons for controlling the experimental environment more closely, reasons for repeating an experiment, reasons for running a subject for a longer period of time, reasons why variability should be thought of as imposed rather than intrinsic, and so on. Moreover, it provided labels for the procedures we were reading about in *JEAB*. Contextually, *Tactics* made sense because there were extant locks for its keys. Less metaphorically, *Tactics* provided concept labels for which I had many exemplars. Thus, the concepts and their labels in *Tactics* were instantly meaningful.

A little later, as part of the preparation for comprehensive exams, I read Claude Bernard's *An Introduction to the Study of Experimental Medicine* (1878/1957). There seemed to be a great similarity between *Tactics* and this book, and both were so sensible as to be readily incorporated into a student's inchoate worldview of behavior analysis.

Current contexts. Both books were read rather late in my student career, and both were read under the aversive establishing opera-

[1] Johnston, J. M., & Pennypacker, H. S. (1993). *Strategies and tactics of behavioral research* (2nd ed.). Hillsdale, NJ: Erlbaum.

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tion of impending comprehensive examinations. Confession time: From that time until I agreed to this review, I had not read another research methods book. I accepted the task of this review because I was flattered to be asked, because I thought I could write a decent one, and because (an aversive establishing operation again) the deadline imposed by acceptance of the project would be sufficient (perhaps necessary) for me to read the second edition of Johnston and Pennypacker's *Strategies and Tactics of Behavioral Research* (1993) (hereafter, *Strategies and Tactics*), the book which this review is mostly about. I had always wanted to read the first edition (Johnston & Pennypacker, 1980) but never had. Why not?

I don't know how similar my experience is to that of others, but I have found that when I am engaged in research on an ongoing basis, I spend little time reading about how to conduct that research. As Johnston and Pennypacker (1993) indicate, it is not vital to memorize names for research designs, nor even to know that there are standard ones frequently used. Nor does it seem necessary to know what someone else says about research methods in one's area in general. The contingencies involved in research seem to shape the required repertoires: Reading the relevant literature and analyzing and interpreting data are shaped through the social contingencies of a graduate education and a teaching career both of which include endless discussions with colleagues and students. In these experiences there are multiple acceptances and rejections of one's responses.

For me, reading about research methods involves aversive control. One reads a book on that topic to tell others about it, to teach from it, to decide whether to set the conditions for students to learn from it, or because doing so satisfies some other social contingency (e.g., a reviewer points out that you use the word *probability* in an inappropriate manner). Thus, until this review, a further occasion to read about research methods in behavior analysis never arose. Understanding the necessity for an aversive establishing operation, I accepted the offer by emitting a controlling response (saying "yes") over responses to be controlled (reading this book and others; writing this review). This then is the current context, important because of what I may or may not say about *Strategies and Tactics* and because there is an implicit sense of relief of responsibility for my actions: Historical and current contexts made me do this. From the foregoing then, one reason to read a book on research methods is because one must: for someone else such as a teacher who will examine you; for students awaiting pearls of wisdom; or for you, the reader, wanting to know whether or not to read this book.

Putatively, though, there is a more important reason to read books on research methods: to learn to conduct research. Is this second reason taken seriously by anyone? Perhaps it is by students. However, the rule governance of behavior that may arise from reading about research is generally insufficient to actually conduct research that is well thought out. Yet, as teachers we often behave as though it were. We assign books on research methods, and, if our course descriptions can be believed, the experience is supposed to equate with doing (or compensate for not yet having done) research. We participate in this charade owing to the lack of time and resources that are required for the extensive mentoring (shaping) that comes from actually conducting research in collaboration with all of one's students.

In some ways I have misdescribed my prior reading about research methods, for I have read a number of books on the history of science and scientific investigation (e.g., Beveridge, 1950; Kuhn, 1962; Taubes, 1993; Watson, 1968). Still, these are not research methods books per se; rather, they are books about the experiences of scientists (and others) from many areas who have behaved scientifically or who at least believe that they have done so.[2] As revealed by these stories, it is not unusual for scientists to make errors that with hindsight are informative. In addition these books contain personal anecdotes that make them interesting and that make them easily remembered. Why should this be? One answer is that the personal anecdotes humanize the scientific process and uncover it for the social process that it is. As a

[2] One cannot always be sure about this: Taubes's recent account of the chapter in American science related to "cold fusion" should be must reading for all graduate students in behavior analysis (Taubes, 1993). A similar argument can be made for Shane's discussion of facilitated communication (Shane, 1993).

social process, science is not easily molded into books of strategies and tactics (Hull, 1988). To attempt such an endeavor is daunting. Johnston and Pennypacker, however, are undaunted, and their attempt invites our assessment.

The Positive Reactions

Clearly, *Strategies and Tactics* is a book primarily for behavior analysts. In many ways it provides the same satisfying experience that *Tactics* provided 30 years ago, but it does so in a different way. Essentially Johnston and Pennypacker attempt to make explicit the rationale supporting the special features of behavior analysis. Many topics that we take for granted about behavior analysis in general and the scientific enterprise within behavior analysis in particular are *not* taken for granted in this book. The book is unique in this respect. Here is a sampling of these topics:

Our dependent variables. The authors start with the very bases of their subject matter and leave no stones unturned. They expect the careful reader to exit the book with no lingering uncertainties regarding the topics that are presented—an admirable objective. Is the objective met? Probably not, but this book comes close, and the reader is grateful for the attempt. As an example, consider our casual use of particular dependent variables. Why response rate? or duration of responding? or latency to respond? The authors explain that units of measurement must "refer to a fixed amount of a quantity . . . be defined independently of what is being measured, and . . . be defined in a standard way within and across disciplines" (p. 95). In turn, quantities can occur on dimensions that help to distinguish the properties of objects and events. To make valid distinctions among objects and events is an aim of scientific measurement. Our typical dependent variables have the attributes of dimensional measures: They have location in time (latency), temporal extent (duration), and repeatability (countability). When you finish the measurement chapter, you not only know what measures behavior analysts use (something you may have already known), but you also understand why these are the measures (something you may not have thought about).

Vaganotic and idemnotic measures. Johnston and Pennypacker draw a useful distinction between two kinds of measures in psychology. Vaganotic measures are those based upon the variability of a population with respect to some measure such as IQ. For such measures there is no independent physical dimension to which the measures relate. The meaning of any IQ score varies from test to test dependent upon the standardization of the tests themselves. An IQ of 85 on one test may not equate with an IQ of 85 on another. The authors point out that one person's four on a Likert rating scale may be equivalent to another person's six. Vaganotic measures are clearly what most psychologists employ, but not we happy few. We rely on idemnotic measures that have absolute meanings "established independently of what is being measured" (p. 93). The reference here, of course, is to physical standards of measurement. In this move—a significant one—Johnston and Pennypacker bind behavior analysis to the physical and natural sciences. An inch is 2.54 centimeters and a minute is 60 seconds, and frequencies or rates are responses per unit time. Interpretation of dependent variables needn't change from experiment to experiment. It is a feature of our idemnotic measures that response frequencies on a particular parameter of a fixed-ratio schedule of reinforcement can be compared validly within sessions and across sessions, within laboratories and across laboratories, within species and across species.

A dash of cold water along with the above: Saying it's so, we hope, can go some way toward making it so. Perhaps that is the most that can be hoped. *Vaganotic* and *idemnotic* are not to be found in the newly revised *Random House Dictionary.* Given our small representation in psychology perhaps they never will be. Nevertheless, it is convenient to have the terms because they instantly categorize for us—as discriminative stimuli—once we know what they mean, and they do this in the same way that labels such as *ABAB design* and *multiple baseline design* do.

Scientists controlled by their data. The authors provide some nice language with respect to the scientist as behaving organism. The basic essentials of this have been said elsewhere (i.e., Skinner, 1957), but Johnston and Pennypacker bring a different slant. Memorable is their attempt to show that scientific behavior does not occur logically any more than

any other human behavior occurs logically. (The biographical books cited above illustrate this beautifully.) As behavior analysts, we know that science is a combination of both formal (rule governed) and informal (contingency shaped) human behavior. But science is human behavior of a very particular sort—that which is under the control of the data gathered by scientists themselves. It is not until human behavior is under such control that it approximates scientific behavior, for at that time, the data can control further behavior ("Stop this condition; start that one; This is an interesting relationship. This is ready to present," etc.). Moreover, the authors do an exhaustive job of showing how a human behavioral repertoire can be brought to such a point, specifying the necessary historical experiences to produce a behavior analyst.

Generating experimental questions. Nowhere is it more important to have the appropriate repertoire than when asking an experimental question, and the portion of the book dealing with this may be the most distinctive. The authors deal not only with the composition of the question-asker's repertoire but also with the characteristics of experimental questions. Once again, such discussion is enlightening because it serves to distinguish behavior analysis from the rest of psychology and provides the reasons for its distinction. With exceptions, we are not theory builders, particularly not in any formal sense; traditionally, we relate our data to our literature inductively, and even post hoc.

Are there distinctive but general types of experimental questions that behavior analysts might ask? The authors answer yes: open-ended questions. They characterize traditional psychology as tending to ask closed-ended questions framed as testing hypotheses based on differences between means of (often vaganotically measured) responses averaged within groups of subjects. In such work there may be a statement of the status quo commonly called the null hypothesis. Experimental outcomes either lead to the rejection of this hypothesis in favor of a sometimes vaguely stated alternative hypothesis (e.g., not the null set), or the experimental outcome leads to a failure to reject the null hypothesis leaving the status quo intact, albeit unproven. Behavior analysts, on the other hand, are responsive to subtle nuances of their data, and they study chronic behavior–environment relations. "What if" questions are often appropriate. For example, what might happen if every 60 s we illuminated a keylight for an untrained, food-deprived pigeon, turned the keylight off after 5 s and followed these two events immediately with a 3-s presentation of the food hopper? The specific answer, as virtually everyone knows, is to be found in Brown and Jenkins (1968). The general answer is that a different area of research might (and did) result.

The emergence of the experimental question owes to the multiple sources of control over that instance of verbal behavior: graduate training, experimental literature, actual observation of behavior, existing resources such as a laboratory, a granting agency interested in your subject field, experimental contingencies (what happened in a prior experiment), extraexperimental contingencies (professional reputation), and the catchall: personal history. This area of the book personalizes the scientific experience in a valuable way.

What the Reviewers Said About the First Edition

The first edition of *Strategies and Tactics* (1980) was reviewed six times (i.e., Dietz, 1981; Hurst & Buskist, 1983; Kazdin, 1982; Lissitz, 1982; Sulzer-Azaroff, 1983; Trice, 1982),[3] with all but one of these reviews appearing in behavior-analytic journals. The exception was the review by Lissitz (1982) for *Contemporary Psychology.* Lissitz, at the time chair of a Department of Statistics and Evaluation and appointed in both Education and Psychology, might be thought of as the quintessential group designer. Although he opined that the book would not "satisfy the average psychologist who is studying children, the aged, marketing, or organizations to name a few areas," that "it does seem somewhat simplistic and narrow in its viewpoint," and that it was doubtful that it "would be used as a text for undergraduate or graduate courses" (p. 35), a number of points of his review were not dissimilar to those of reviewers in the behavior-analytic literature.

Take Sulzer-Azaroff (1983) as an example. Although in general philosophical agreement

[3] I thank Jim Johnston for this information.

with the behavioral stance of the book, Sulzer-Azaroff was bothered by its "polemical stance," specifically as she saw how Johnston and Pennypacker dealt with inferential statistics. Had she read Lissitz? Or was Lissitz just a mirror for some of Sulzer-Azaroff's feelings? Kazdin (1982) also agreed with this reservation, stating that "between-group research is given a short shrift" and that "perhaps [that methodology] should not be lightly cast aside" (p. 360). Sulzer-Azaroff was also bothered by the book's focus on the general rather than the specific. To her, learning proceeds from the specific to the general, at which time the labels for generalizations contain many specific examples. So, Sulzer-Azaroff notes that she and her teaching colleagues developed detailed study guides to take student repertoires from where they presumably were when they started on *Strategies and Tactics* and gave them plenty of examples to fill in what presumably were empty labels for the concepts about which Johnston and Pennpacker wrote. Is this not a polite way of saying, that as a text to teach from the first edition required help?

My impression is that these authors would say much the same thing about the second edition, for between-groups research is still given a short shrift. The second edition still has a polemical feel. Like alternative cinema, it's not for everyone. Who, then, is it for? It is for us, behavior analysts. I can't imagine enjoying, let alone understanding, this book without being a behavior analyst. To me that's who the book is for, polemical feel and all, and that's just fine.

Dietz (1981), with the first behavioral review of the first edition, appeared to disagree with Sulzer-Azaroff; he considered the first edition to be a great text, but his reasons were quite different. He considered it a great book because of its neglect of market (sales) factors, evidenced by the strong rationalized point of view it presented (a polemical feel?) in "unwatered-down" fashion. He called it "a book that can be used as a text rather than one intended only to be a text" (p. 67). He seemed not to question whether the book would actually work as a text. The second edition, too, appears to neglect market factors and presents a strong rationalized point of view. And it's still our point of view.

Hurst and Buskist (1983) in their completely positive review of the first edition proposed that it "should be read and studied *ad infinitum* by students just entering the field of behavior analysis as well as by well-seasoned researchers" (p. 576). I wonder if they have used the book as a text since their review and whether they still believe what they said. I can see "just-entering" students reading the material in this book, and without help, never (i.e., ad infinitum) appreciating what they read or why they should have read it. The best use of this book is not as an entry-level text.

Why Write a Book on Research Methods in Behavior Analysis?

There appear to be several general reasons for writing a book, let alone one on research methods in behavior analysis. The most crass, but not the least likely, is to make money—a great reason given the state of faculty salaries. However, this could not be a reason for the complete reworking of the first edition that produced the second edition, nor the reason for writing the first edition. The first author tells us in the preface to the second edition that this edition was totally rewritten on the basis of feedback from the readers of the first edition and the authors' own experiences. Thus, for example, in this revision the authors place much of the background, supportive, and extended material that was included in the first edition in a separate book of readings (Johnston & Pennypacker, 1993; hereafter, *Readings*). Given the 13 years between the first and second editions, money would certainly not be the object in revising the original, insofar as in textbook writing frequent editions are essential if a book is to make money. Finally, the second edition is what is called in the trade a "niche" book, a book focused on a small segment of the overall market. Typically niche books don't make the big money, although they may do moderately well. Usually they are unappreciated much beyond the audience for whom they are written (e.g., Lissitz, 1982, on the first edition). So we can forget about money as a motive.

A second reason to write a book is to produce fame for oneself. Knowing the authors of *Strategies and Tactics,* this is a highly unlikely motive. Objectively, their market is too small

to produce much fame. So both fortune and fame are ruled out.

A third reason to write such a book is to entertain others. When you read *Strategies and Tactics,* you will understand that it was not written for entertainment. There is no levity here, but, it might be argued, there isn't in *Tactics* either, and perhaps there shouldn't be in any serious work. Yet, many textbook authors and their publishers entertain with color, anecdotes, and photos. There are no photos in this one. There are plenty of boxes and a few charts, each illustrative of text material and each informative.

I did not have as much of the same reaction to *Strategies and Tactics* that I had to *Tactics* those many years ago, and I think I know why. I certainly did not have the same reaction to it that I had to any of the semipopular personal accounts of doing science (e.g., Watson, 1968). Perhaps I was less critical then, and more susceptible to gee whiz. Yet a major difference in these other books is that they contained a myriad of personal examples of both correct and incorrect behavior with respect to the business of science, or they contained actual examples from the relevant literature (e.g., Barlow & Hersen, 1984; Iversen & Lattal, 1991; Sulzer-Azaroff & Mayer, 1991). These two events made the books and the problems real rather than abstract. *Strategies and Tactics* contains neither actual examples nor personal experiences in any quantity, and that is too bad because it would have been more readable with these inclusions. There is little that reaches out to touch the reader beyond the text itself. Because of this, there is little that personalizes this book. For me, *Readings* was more entertaining than the text itself. Having not read the first edition with the essence of *Readings* right in it, I don't know whether it would have been a more entertaining experience than the second edition was. This is unfortunate because the popular accounts of scientific behavior, cited above, cross niches and are popular because they are personalized. *Strategies and Tactics* could have profited from some personalization.

The fourth and final reason to write a book on research methods in behavior analysis is to inform and to teach. Obviously this is the most ideal reason to write, and it is clearly the reason this book was written originally and is presented again in a second edition. And this begs the question.

Does Strategies and Tactics *Inform?*

Very much so, but not without difficulties. First, as mentioned above, there are few anecdotes for the hungry student. The authors state their awareness of the need for more examples and appear to attempt to deflect arguments about the number of examples in the second edition ("although there can never be enough in all the right places"; p. xii). There may be more examples in this edition than there were in the first, but, for me, there weren't enough.

The authors choose not to use data from the literature as examples for several reasons, one of which is not to offend anyone (laudatory, but misguided). In many cases such data may not exist, but in others they do. This is a major difference between this book and *Tactics* and this book and Barlow and Hersen's (1984) sourcebook, *Single Case Experimental Designs* (hereafter, *Single Case*), which I also examined for this review. *Single Case* is the definitive sourcebook of behavior-analytic research methods. As a sourcebook it is exemplary with respect to literature examples and provides the greatest contrast with *Strategies and Tactics.* Its focus is clinical and applied behavior analysis, and its examples reflect this focus. It should be noted that the missions of the two books, and therefore their subject matters, only partially overlap. Sidman's *Tactics* and Barlow and Hersen's *Single Case* are considerably assisted by using real exemplars. *Strategies and Tactics* could be improved by the addition of multiple exemplars. That the subject matter of the book is comprised of high-level abstractions is not the fault of the authors; however, the salience of these abstractions would improve with reference examples. I provided the context for my experience with *Tactics* to show how that book fit in with my repertoire at the time. The fit was greatly assisted by the use of examples from the literature.

So, does *Strategies and Tactics* inform? Yes, but not without careful study, and not, I think without a substantial preliminary repertoire in behavior analysis. This is not a book for the general psychology undergraduate (the publisher's advertising notwithstanding) or for beginning graduates. For maximum ap-

preciation, it is a book for those who already have behavior-analytic referents. With a behavior-analytic repertoire, the concepts in the book are instantly meaningful—and appreciated—and it is in this way that my reaction to *Tactics* and *Strategies and Tactics* was comparable.

To teach successfully from *Strategies and Tactics* may be to demand that the student acquire the necessary behavior-analytic repertoire to appreciate the book's contents concurrently via the extra study guide approach (e.g., Sulzer-Azaroff, 1983). On that score, an anecdote: The students in my undergraduate behavior modification class used to be able to persuade me to let them take the class without having had the introductory experimental analysis class. They claimed that they could get the principles from the behavior modification text while simultaneously learning about the application of the principles in the field setting. It was never so. Those with the principles already firmly in their repertoires always did better, which leads me to infer that they had a better experience in the class. In that respect, nothing appears to have changed between the two editions of *Strategies and Tactics.* Supplemental behavior-analytic experience, particularly prior (not concurrent) experience, will heighten the utility of this book considerably.

You Can't Get There From Here

The careful reader will note that I have been selectively—and, I hope, constructively—critical of *Strategies and Tactics.* No book is all things to all people. The question is begged: Are there other books out there that do the same job as this one? Or that do some semblance of the job? To my knowledge, the answer to the former is a flat no, and to the latter is a qualified maybe. *Strategies and Tactics* is largely unique. It overlaps with *Tactics* in a few necessary areas (e.g., design, variability, replication) and with *Single Case* as well on similar issues. But Johnston and Pennypacker make the point several times that their book is not a sourcebook (which one infers from them is a bad thing to be); however, Barlow and Hersen plainly state that *Single Case* is. What are we to make of this? *Single Case* is in some appealing ways the antithesis of *Strategies and Tactics. Single Case* is replete with examples of the research designs that Barlow and Hersen define and then discuss. One wants to take *Single Case* and say to the student, after said student has read *Strategies and Tactics,* "This is the outcome of what Johnston and Pennypacker said." As such, *Single Case* does not replace *Strategies and Tactics*; rather, it follows from it. Continuing to the student, one would say, "Read *Single Case* to acquire labels and applied exemplars with respect to research designs in behavior analysis. But don't expect to know why these are the research designs we use—you have already discovered that from *Strategies and Tactics.*" To buttress this point, note that Barlow and Hersen cite the first edition of *Strategies and Tactics* about twice as often as Johnston and Pennypacker cite *Single Case. Single Case* relies on *Strategies and Tactics.* These two books are both valuable resources, but one (*Strategies and Tactics*) is the tree and the other (*Single Case*) is fruit on the tree. As sets they intersect only a little, and perhaps this is appropriate.

Or is it? A different perspective is taken in the two volumes (Parts 1 and 2) of the *Experimental Analysis of Behavior* (hereafter, *EAB*) edited by Iversen and Lattal (1991). This is a multiauthored treatise that attempts "to describe representative, effective research techniques in the Experimental Analysis of Behavior" (p. vii). In so doing, it succeeds very well. (Notably, Iversen and Lattal also consider their volumes to be a sourcebook.) How do these volumes compare with *Strategies and Tactics*? To begin with, note that techniques are not strategies and tactics. There is much in *EAB* on technique in experimental analysis. Also there is much that is topical—that is, that describes what has been accomplished in specific areas of experimental analysis. In both senses (technical and topical) *EAB* is about research. But the topical chapters, although nodding toward typical experimental preparations and values of independent and dependent variables, are really just lovely reviews of the topics of the experimental analysis of behavior. After reading them one can say, "This is how I should run an experiment in, say, generalization or avoidance"; "This is how such data are usually analyzed"; and also, "This appears to be an important question to ask."

The technical chapters in *EAB* are very di-

dactic with respect to experimental preparation and analysis. These chapters are pointed toward application (in the sense of conducting experimental analysis), including things like the care and feeding of laboratory animals (Ator), the use of computers (Gollub), methods of analyzing behavior patterns (Iversen), and methods of appropriate mathematical description (Shull). (Shull's chapter is without peer with respect to illustrating mathematical treatments of operant behavior.) In exemplary fashion the technical chapters in *EAB* describe literal experimental analysis. As such, these chapters are definitely about research, sometimes about methods, and once about research design (the chapter by Perone). With the exception of the Perone chapter, there is only incidental overlap with *Strategies and Tactics.* As sets, *Strategies and Tactics* and *EAB* also intersect only a little.

The topical chapters in *EAB* seem to be other fruit on the *Strategies and Tactics* tree—experimental-analytic fruit rather than applied behavior-analytic fruit (as in *Single Case*)—but fruit nonetheless. Because neither the technical nor the topical aspects of experimental analysis are to be seen in *Strategies and Tactics, Strategies and Tactics* and *EAB* compare less so than they contrast. I would prompt the student to read relevant topical chapters in *EAB* before reading *Strategies and Tactics.* To do so would provide the student with plenty of repertorial referents with which to more fully appreciate *Strategy and Tactics.*

What of other presentations of strategies and tactics? If the book in question has anything to do with the teaching of behavior analysis, it is likely to contain something on single-subject research design. A brief review of a nonrandom sample of other presentations sitting in my library courtesy of their publishers (i.e., Malott, Whaley, & Malott, 1993; O'Leary & Wilson, 1987; Pierce & Epling, 1995; Sulzer-Azaroff & Mayer, 1991; Thorpe & Olson, 1990) shows that most of them contain a chapter on single-subject design; only Sulzer-Azaroff and Mayer's book (about which more below) contains two. In all of these chapters the basic designs are covered (i.e., ABAB, multiple baseline). In a second chapter, Sulzer-Azaroff and Mayer extend their treatment to changing criterion and alternating treatment designs and variations thereof; Malott et al. discuss these in one chapter. However, these books do not qualify as state of the science, as does *EAB.* Nor are they for the most part in any way comparable to *Strategies and Tactics.* More in keeping with *Single Case,* they provide one or two examples (usually one) of each of the designs they describe. They all are undergraduate texts, and as such are more area surveys than anything else. The text by Sulzer-Azaroff and Mayer is the possible exception. Although clearly an undergraduate text, these authors also attempt a rational and empirical "how-to" for applied analysis. The rational parts of their text come close to strategies and tactics per se, although the focus is on applied analysis, and the application examples are what one wishes for *Strategies and Tactics.* Is Sulzer-Azaroff and Mayer's text an outcome of writing one's own study guides for the first edition of *Strategies and Tactics*? In doing so can one end up with a book of one's own? In sum, there really is only one *Strategies and Tactics.* Accept no substitutes.

Should You Buy Strategies and Tactics?

Definitely. The book is full of gems. The vaganotic/idemnotic distinction is worth the price alone. Should you read it from cover to cover? Yes, if you have to teach from it, or perhaps if your history differs substantially from mine. No, if it is similar to mine. In the latter case, I'd consider the book an excellent source on specific matters valuable to the strategies and tactics of behavior-analytic research and use it as a reference source—an excellent one at that.

Postscript: Life's Like That

Since I began this review (too long ago to think about), the person in our department who has been adroitly teaching our graduate single-subject design and assessment course has decided to do so no longer. Who do you think is going to take on the job? And what text do you think he is going to use?

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