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Exploring the Controlling Conditions of Importance

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Some verities are eternal—as eternal as the conditions under which we use those terms, anyway. Murray Sidman proposed a fairly distinctive set of tactics for scientific research 30 years ago, and they are still studied, much as certain military tactics of centuries past are still studied by soldiers: These tactics still increase the probability of winning, in part because they define winning better than usual. True, the generic campaign for which Sidman designed his tactics is much newer than the one that interests military students; there has been only a little time in which to judge these tactics. On the other hand, given the precedent set by military tactics, we may claim a great deal more time before we decide if Sidman's tactics are no better than their alternatives—unless, of course, some of the soldiers' future tactics preclude any of us finishing any of our campaigns.

I have taught a graduate course in experimental design every academic term since 1961; *Tactics* had been my primary text. Indeed, I began teaching the course only because *Tactics* had just been published. It was the first text on research design I had seen that evaluated research according to the importance, reliability, and generality of what it might discover. Every other design text that I knew (and I knew many) did so with only the single criterion of reliability. Sidman's text reminded me that knowing the generality of every reliability was much more important than having any number of reliabilities of unknown generality, and that there were a number of ways in which knowing the generality of reliabilities could prove important. A text with three criteria for good design, a rational, strong ranking of those three, and five more criteria for evaluating the best of them, was irresistibly better than the one-criterion operant-level text of the research-design field. My students had to study it.

In the ensuing 30 years, applied research examples have become numerous and interesting. Yet *Tactics* was written, seemingly, for basic research. Despite the applied specialization of so much contemporary behavior analysis, I have found that *Tactics* serves perfectly in teaching research design for either adventure. That suggests that neither convincing research nor behavior analysis changes fundamentally when turned to applied questions. In recent years, however, I have been tempted to add two additional criteria, some of them unique to application, to the original five that define importance.

Tactics nominates five parallel criteria for what makes research important. Important research is that which:

- tests important theory (important theory is general theory); or
- satisfies important personal curiosities (important curiosities are systematic, programmatic curiosities, and thereby are about generalities); or
- asks whether each newly studied behavior requires a different analysis than the already studied ones (because if it does, then our analysis of the already studied ones suddenly has less generality than before, and if it doesn't, then the analysis of the already studied ones now has that much more generality than it had before); or
- asks whether each newly developed technique of behavior control is fundamentally different from the already studied ones (because if it is, our analysis of why the already studied techniques control behavior suddenly has less generality than before, but if it isn't, our analysis of why the already studied techniques control behavior now has that much more generality than it had before); or
- explores the controlling conditions of any behavioral relation, which certainly will maximize our understanding of when

it changes and when it does not–that is, of its generality.

I might argue from this somewhat creative restatement of Sidman's criteria that the five ways of seeing importance are just five ways of evaluating the various kinds of generality that exist, in which case perhaps there are only two criteria for good research, reliability and generality. Perhaps importance lies in acknowledging the superiority of knowing the generality of any reliability over having only reliabilities of unknown generality, and in appreciating how many kinds of generality there are to know. However, that argument hinges on my having restated Sidman's five criteria in the ways that I did, which was to emphasize their relevance to the kinds of generality that *Tactics* specifies. Because I mean to suggest some additional criteria for importance in applied research, and because not every one of them will be just another way to evaluate generality, then for today, let the criteria for good research continue to list importance as more than examining the various generalities of every reliability, and re-affirm the five criteria for importance listed above as just that: the criteria for importance, which is not always generality. To those five, which, however they are categorized, I find as eternal as I find anything, I presume to add the following two, only in recognition of some tangents that applied research had better pursue in the interests of its own viability.

The Response Analysis of Trouble

The fundamental research tactic of behavior analysis is to gain experimental control over the interaction of behavior and environment, and to codify the ways in which that can be done in as general terms as possible. That is also a research tactic of applied behavior analysis, but not its fundamental research tactic. The fundamental research tactic of applied behavior analysis, its defining tactic, I submit, is to discover what behaviors need to be changed in order to solve a problem.

Behaviors are not problems. Behaviors are natural events; they occur or fail to occur because that is what must happen, given the history of the organism and the current environment. The problem is invariably that someone complains about that necessity powerfully enough to generate behavior-change interventions by themselves, by their agents, by the organism complained about, or by agents of those segments of society that take upon themselves interventions into complaints. The point of these interventions is to reduce the complaint (cf. Baer, 1975, 1976, 1978, 1982, 1988; Baer, Wolf, & Risley, 1988). Then the fundamental problem of *applied* behavior analysis is not knowing how to manage behavior, but knowing what behavior, if changed, will reduce the complaint which is the stimulus control for the intervention. Given knowledge of the correct target behavior, *then* disciplinary applied behavior analysis can ask what is known about changing that kind of behavior in that kind of organism and those kinds of settings, and use it.

Remarkably, the discipline of applied behavior analysis offers principles of its secondary research tactic, how to change behavior, but not of its primary research tactic, how to identify what behaviors to change. Perhaps that is because the problem is far from easy; or perhaps that is because there are no such principles, and the discipline will have to proceed in the future as it has so far, which is primarily by experience, precedent, and what may as well be termed shrewdness, intuition, and apparently arbitrary guesses. In the second case, there is little more to be said about the importance of research relevant to this primary research tactic: We should simply begin amassing experience in changing behavior, *while establishing reliably and generally the extent to which doing so reduces the originating complaints,* and base our future shrewd intuitive guesses as much on that as possible. In the first case—if behaviors become intervention-target behaviors only because of some initiating complaints, and there are principles of identifying what target behaviors, if changed, will best reduce those complaints—then any research that establishes or clarifies those principles will be as important as any research could be to the discipline of applied behavior analysis. (It will also remind us that interveners sometimes decide not to reduce complaints, even when they know or suppose what behaviors, if changed, would do so. The analysis of *that* behavior remains, as ever, a supremely interesting one to the discipline of applied behavior analysis and to its societal audience, and thus to at least a pair of superordinate disciplines—call one *ethics* and the other *public policy*.)

No doubt, those principles, if they exist, will have only a certain amount of generality; like all other principles, their statements will have to vary with certain contexts. Thus, once any of them has been established by reliable research, the next and much more important tactic will be to evaluate the generality of that reliability—as argued above.

Establishing Cost-Benefit Ratios

One consequence of being under the stimulus control of complaints to reduce is that they typically exist in a context of sharply interdependent other complaints. When someone wants a behavior changed, they also want it changed cheaply. Thus there are thresholds describing when cost will prove too high and complaints about cost will supersede the original complaint. Often enough, those who complain about an unchanged behavior are not the ones who complain about the cost of changing it, which creates an interesting problem in analyzing the conditions under which one complaint can have more stimulus control over intervention-and-analysis behaviors than another.

Thus, any research is important to the discipline of applied behavior analysis if it teaches us how to appreciate the range of events that function as benefits and as costs: money, effort, time, materials, and human senses of well being, fairness, joy, and anguish. Those events far exceed money (but often can be equated to money, and arguably can always be equated to money—another discipline in itself). The understanding that any benefit has costs, and that any cost has benefits, and the understanding of their range, is probably just an exercise within systems theory, field theory, or contextualism, but it is a remarkably important exercise of applied behavior analysis.

In that systems theory, field theory, and contextualism are all at least theories about generality—its extent, its modification, and its failure—this consideration once again returns us to the familiar *Tactics* theme recited above: how much more valuable is an understanding of the generality of our reliabilities than are the reliabilities themselves.

Summary

Tactics is a research-design text; it also is a philosophy of science and describes some of the scientific analysis of behavior. Its genius is that research design *is* an exercise in the philosophy of science: Any use of design exemplifies a philosophical stance, whether functionally or not. Similarly, any adoption of a philosophical stance dictates certain tactics of research design, whether acknowledged or not. Best of all, though, in this text the behaviors of choosing, developing, or merely exemplifying philosophical stances, like the behaviors of choosing, developing, or merely copying research designs, are behaviors of the class that the analysis of behavior is about. Those behaviors are verbal behaviors, and they generate further verbal behaviors about themselves, sometimes on the assumption that the further verbal behavior can at times affect the prior verbal behavior. The verbal behavior of this commentary is offered on the assumption that this is one of those times.

And if this is not one of those times, so what?

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