The Aesthetics of Intervention in Defense of the Esoteric

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So it appears that the experimental analysis of behavior (EAB) could benefit from intervention aimed at increasing its translational footprint, thus promoting continued recognition and support as a valuable social enterprise. By profession, I analyze behavioral problem spaces and find behavioral solutions. I therefore greatly appreciated Critchfield's (2011) efforts to identify the problem, its controlling variables, and its potential solutions. At least two behavioral problems are identified immediately in Critchfield's abstract. The first can be restated as ''Why hasn't basic behavior analysis demonstrated social relevance more often?'' I will say little about this first question other than to restate that the perception that basic research frequently demonstrates social relevance is perhaps misguided. Harvey Brooks, a physicist turned Harvard science and public policy analyst, notes that public debate about science and technology has been dominated by a pipeline model (perhaps better known as a linear model; Kline & Rosenberg, 1986) in which new discoveries in science produce new technological ideas, progressing through applied research and terminating in commercialization (Brooks, 1994). This model was exemplified by highly visible successes of World War II (e.g., the atomic bomb, radar), but such a model ''corresponded only to the rare and exceptional cases cited above, it became embodied in political rhetoric and took considerable hold on the public imagination and seemed to be confirmed by a sufficient number of dramatic episodes so that it was regarded as typical of the entire process'' (Brooks, p. 477). The perception that much basic science leads ultimately to technological innovation is something of a base-rate error. If EAB needs help in this respect, it is not alone among basic sciences.

The second question, ''What are impediments to translational innovation that may need to be addressed for basic behavior science to increase its translational footprint?'' implies a behavioral deficit, and behavior analysts are good at addressing behavioral deficits. Critchfield identifies several important historical antecedents, including carryover (largely through training) of a time when the indirect benefits assumption was strongly supported and the adoption of rules that basic and applied science do not mix well.

The second half of Critchfield's article is prescriptive, aimed at promoting translational research behavior on the part of basic researchers. The solutions imply some fundamental changes in operations. Critchfield is not pointing to inadequacies per se, only to changes that are sensitive to the current environment. Change is inevitable. Critchfield is simply recommending how to channel behavior change in ways that are sensitive to that environment. This is good behavior analysis.

Towards promoting translational research in basic scientists, Critchfield suggests the following solutions: (a) Basic scientists need to read applied research and interact with practical problems; (b) basic scientists must master the communicative skills necessary to establish the social relevance of their research; (c) basic scientists should collaborate with applied behavior analysts; and (d) basic scientists should consider, and be trained in, experimental methods that are more suitable to use-inspired research. The last solution implies diverting some proportion of basic endeavors towards research questions with practical end points, adopting human subjects as the focus of basic research, determining how to obtain orderly data more efficiently, and considering experimental designs that resonate more strongly with the scientific mainstream.

If carried out, I have no doubt that this prescription would succeed in producing more translational research out of our basic laboratories. Although I do not object, I can imagine the resistance or objections of others. They might start with reminders of the very good reasons those practices evolved in the first place. A ''drop everything else'' approach is not necessarily aligned with pursuing matters of practical import, but translational research is based only on the knowledge at hand. Translational research is perhaps less likely to result in discovery of novel phenomena. I do not entirely disagree with Poling's (2010) characterization of EAB as ''esoteric analysis of behavior,'' but we should keep in mind that ''the cumulative development of a science provides the only final answer as to the importance of any particular data'' (Sidman, 1960, p. 41). Group designs and statistical inference are perhaps the norm, but they are only as good as the assumptions on which they are based. Replication trumps p value any day. Nonhuman subjects are far removed from most societal concerns, but many questions of applied relevance are difficult, if not unethical, to answer with humans. History effects and punitive effects readily come to mind. Also, adopting a use-inspired research agenda does not necessarily free the basic researcher from the constraints said to impede more widespread appreciation. The criteria are looser, to be certain, but you find no respite from steady-state requirements in applied and translational research. Many submissions to the *Journal of Applied Behavior Analysis* (*JABA*) are still rejected because the authors cannot say with certainty that the same pattern found in Condition A would not have emerged in Condition B if Condition B continued for as long as Condition A.

''Yeah, We've Got an App for That''

The ''aesthetics'' in my title is borrowed from Hineline (2005), who reminds us that good applied behavior analysis is sensitive not only to what is effective but also to what its receivers find appealing and therefore likely to adopt. This sometimes involves drawing a distinction between the ''crucial'' and the ''optional.'' Is collaboration crucial? Mace and Critchfield (2010) suggested that ''innovation is most likely when individuals with basic and applied expertise collaborate'' (p. 293). However, the same authors also note that collaborative efforts between basic and applied researchers ''remain, at best, occasional, and nearly always unidirectional, with applied researchers recruiting basic-research expertise as a means of promoting better applied technologies'' (p. 304). Is it crucial to overhaul our training programs and our research philosophy? I hope not, but if needed, it seems like the sort of change that evolves rather slowly. Perhaps calls like Critchfield's and others are exactly the way this sort of thing starts.

Behavior analysis already has plenty of people engaged in the sort of behavior prescribed and who are willing, and even enthusiastic, about enhancing the footprint of basic behavioral research. Exploiting basic research requires a practical understanding of the applied context that may be a more critical impediment to translation than the generality of the findings in question. A sizable proportion of applied behavior analysts like to believe they are engaged in translational behavior analysis. They already read basic research and speak the language of social relevance. Their efforts are driven by the socially significant problems of human subjects often addressed in more reasonable time spans. They know how to operate successfully in those environments that are foreign to their basic brethren. Most important, they are eager to further both the field and the development of effective intervention through the adoption of relevant basic research. Perhaps increasing the translational footprint of EAB is a matter of letting "these people" help "those people" by helping themselves.

One thing that is crucial for translation is getting the findings of basic science into the hands of those willing to translate. I do want EAB to keep its eye on the socially relevant *and* I want EAB to explore the esoteric, but I also want to know if the putatively esoteric can be made of greater general interest. Critchfield has laid out a fine treatment plan for the former, but has perhaps ignored the latter. If some of our pure basic research currently receives insufficient recognition to be sustained, then the intervention should also aim to promote that recognition.

There are, of course ways to do this that are more readily under our control and certainly more efficient than the wholesale upheaval of our training curricula. The flip side of promoting use-inspired basic research is to point out the use of the basic science already done. Do we know something about best practices for this? Here's one example. Our journals have made prior pointed efforts at promoting the adoption of basic behavioral research by an applied audience. Between 1993 and 2001, *JABA* published, by my count, 15 basic-to-applied papers in which two (occasionally three) behavioral scientists, typically representing both basic and applied research traditions, collaborated to bring recent basic research to bear on topics of potential significance to the *JABA* reader. Those 15 papers could be divided in an interesting way. Seven of those efforts, generally the earlier ones, related highlights from the most recent issue of *JEAB*. Let's call these "*JEAB* issue" papers. The other eight revolved around a topic rather than an issue of *JEAB*, unrestrained by when and in which journal the basic research appeared. Let's call these "topic" papers.

As a loose measure of the influence of those papers, we can examine the number of times papers in each category have been cited. A Web of Science cited-reference search for each paper reveals that the *JEAB* issue papers have collectively been cited 40 times to date ($M = 5.7$, median $= 7$), and the topic papers have been cited 209 times ($M = 26.1$, median $= 17$). This difference becomes more striking if one takes into account that the *JEAB* issue papers usually appeared earlier than the topic papers. The fact that the mean is larger than the median for the topic papers suggests that the mean may be skewed by outliers, which is in fact the case. By far, the most frequently cited of these papers were Critchfield and Kollins (2001) on temporal discounting (67 times to date) and Fisher and Mazur (1997) on choice (61 times to date). However, even if one excludes those two papers, the topic articles have been cited roughly twice as often as the *JEAB* issue articles. There are a handful of other

highly successful examples that were perhaps not explicitly part of this series. Galbicka (1994; 31 citations) set out to describe how and why percentile schedules could be made relevant in applied settings, a call subsequently met by several recent *JABA* papers (e.g., Athens, Vollmer, & Pipkin, 2007; Hall, Maynes, & Reiss, 2009; Miller & Neuringer, 2000). My own current efforts and grants were, in part, inspired by the "Applications" segment of Green and Freed (1993; 78 citations, 27 of which were articles related to intellectual and developmental disabilities and 24 of which were published in *JABA*).

Contributions in both categories were fine pieces of scholarship that brought basic research to a presumably appreciative audience, "filtered" through the pen of applied scientists who spoke the language of that audience and understood the practical necessities and obstacles of applied research. To the extent that "the analyses and language that serve basic science do little to fuel the imagination of those who might develop and disseminate practical innovations" (Critchfield, p. 7), both types of papers aimed directly at casting the information in a more accessible form. So why the dramatic differences in citation rates?

A synthesis of selected papers from an issue of *JEAB* must make do with what it gets; a synthesis without such restriction can tap into existing enthusiasm for a research area. The applied reader is perhaps less inclined to be motivated by, or even to read, a collection of random investigations, no matter how skillfully presented by the authors. On the other hand, the reader who already has an inherent interest in a carefully selected topic can make more immediate contact with their work and interests. I want to be clear that these were not simply reviews; they typically pointed to where in the applied arena the basic research could be of use. The topic papers were an opportunity for the basic researcher to tell the applied audience what might most profitably be made of *their own* endeavors. The indirect benefits assumption asserts that "someone else, someday, can be expected to harness the resulting principles for practical benefit" (Critchfield, p. 5). The topic papers say that someone else is you and someday is as soon as you can figure out how this benefits the problems you address. Mace and Critchfield (2010) suggested that, "when not forced to consider the everyday relevance of their investigations, basic scientists may not develop the skills needed to explain the practical significance of their research" (p. 295). Authors of those topic articles were asked to consider exactly that sort of relevance, and applied researchers noticed. I am certainly not implying that applied behavior analysts cannot distill this information on their own, but as Critchfield asks, who has the time?

What does any of this have to do with promoting the translational footprint of EAB? It is offered solely as an example of the sort of "intervention" we could promote that would perhaps be more "aesthetically pleasing" to the basic researcher, hence more likely to be embraced without the sort of objection I suggested above. Pointed and directed discussion of the implications of one's research can be rather easily commissioned by our journals. Certainly there are other similarly aesthetically appealing strategies. If any of my basic brethren are still reading, and neglecting for the moment that successful translational efforts often require bidirectional input, I want you to know this: You don't have to do it yourself, but do tell me what you think should be done. You are more intimately familiar with your data than I will ever be. You have been thinking about it for months; once or twice I know that you've even thought about how it would

play out in the real world. I want to adopt it to help those I serve and can perhaps do so more readily than you can. I may not agree entirely with your premise. I may, given the benefit of operating in my environment, think it untenable, impractical, or impracticable. But, if it works, I will give you the fullest of credit. You, of course, must take advantage of this credit. You must read and cite my translational and applied efforts and proclaim to the scientific community how you contributed to this socially relevant problem. Perhaps we can get a grant.

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