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The Openness is There

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I agree with the main thrust of Vyse's (2013) argument that basic researchers in behavior analysis can and should ensure their relevance by pursuing questions of interest to broad audiences. I think nothing bad can come from encouraging behavioral scientists to broaden the scope of their questions and develop the additional skills necessary to answer and communicate their findings. Therefore, I am glad Vyse found the recent exchanges on translational research engaging (e.g., Critchfield, 2011; DeLeon, 2011) and that this ongoing discussion, at least in part, motivated him to write his essay. I am fully convinced that it is important for basic behavioral researchers to engage in research that is both translational and relevant to mainstream psychology.

Translating basic behavioral research and achieving broader relevance, however, are not entirely the same, and Vyse does not treat them separately in his essay. Vyse criticizes several commentators for defending the usefulness of traditional behavioral methods (e.g., Branch, 2011; DeLeon, 2011; Pilgrim, 2011), or "classic operant methodologies (rats and lever presses or pigeons and key presses)" (p. 125). Vyse is critical of basic researchers for, as he sees it, being "bound to a limited group of methodologies and, as a result, a relatively restricted set of experimental questions and publishing outlets" (p. 126). Instead, Vyse recommends that basic researchers use more group designs with humans and statistical analyses (see also Critchfield, 2011).

I can appreciate the intent of Vyse's arguments in the context of connecting and publishing with colleagues in mainstream psychology but, as he contends, "science is simply a set of tools" (p. 129). I believe the points of DeLeon (2011) and Pilgrim (2011), in particular, were that these traditional methods are ideal tools for translating between basic and applied behavioral research. Some examples are modeling treatments for problem behavior (e.g., Mace et al., 2010) and preclinical tests of drug effects on behavior (see Sanchis-Segura & Spanagel, 2006, for a review). There probably are refinements suitable for making basic behavioral research even more relevant to applied questions (see Critchfield, 2011). Nevertheless, it is difficult to conceive of a better model system than the operant chamber for addressing basic questions relevant to applied behavioral researchers and practitioners. These methods can reveal how basic learning processes operate in natural settings and provide a platform to evaluate potential behavioral treatments.[1] I might be disappointing Vyse, as he predicted a defense of these traditional methods. But if methods are simply tools, it makes sense to use the right tools for the job.

I would like to revisit Vyse's statement that basic behavioral researchers are "bound to a limited group of methodologies and, as a

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[1] On a related issue, moving the Society for the Quantitative Analysis of Behavior (SQAB) away from the Association for Behavior Analysis International annual convention would only hurt efforts to establish translational research.

result, a relatively restricted set of experimental questions and publishing outlets'' (p. 126). Some basic behavioral researchers certainly are guilty of asking similar, perhaps tired, questions while making little effort to connect their findings with a bigger purpose and broader audience. But it is hard to see how he would come up with any other conclusion when he defines the experimental analysis of behavior (EAB) from a single year of the *Journal of the Experimental Analysis of Behavior* (*JEAB*) and then, most importantly, uniformly lumps together all studies that used traditional methods. And to look only in *JEAB* and then to say that few are publishing elsewhere is unfair. Even if it were the case that basic behavioral researchers publish too narrowly, a much more extensive analysis that includes other journals is necessary to support this statement. A primary obstacle to pinning this down, in general, is that there is a lot of gray area. It is difficult to categorize many basic behavioral researchers explicitly within or outside the boundaries of EAB. Whose research fits neatly within EAB and whose does not? Where is the fence line?

My impression is that this sort of insular behavior reflects a minority of basic behavioral researchers. In making his point so forcefully, Vyse underrepresents the breadth of topics explored by many basic behavioral researchers and treats them as monolithic. Most basic behavioral researchers who publish in *JEAB* have their irons in multiple fires and publish in a range of other journals. Some of these lines of research include those already discussed by Vyse, and include a range of questions concerned with the discounting of consequences or, more broadly, decision making (e.g., Estle, Green, Myerson, & Holt, 2007; Jones & Rachlin, 2006). As I have already mentioned, some researchers rely on traditional animal models to ask questions relevant to drug development or neuroscience. Many others apply the empirical and theoretical backgrounds developed within the pages of *JEAB* to answer questions of interest to broad audiences. For example, White and colleagues developed methods based on their understanding of stimulus control and signal detection to distinguish between wine experts and novices in wine recognition, categorization, and memory (e.g., Parr, Heatherbell, & White, 2002). The groundwork for this research can be traced back to traditional methods. Therefore, the reason these methods have been with us for 80 years now might be that they continue to be useful for a wide range of pursuits.

Even *JEAB*, the source for the most traditional studies in EAB, proves to be open to alternative methodologies. For better or worse (see Gallistel, Fairhurst, & Balsam, 2004), the editors of *JEAB* repeatedly have signaled openness to the types of methods Vyse encourages basic behavioral researchers to use more frequently (e.g., Madden, 2012; Mazur, 2008). Furthermore, many basic behavioral researchers conduct research on the discounting of consequences in the pages of *JEAB* using the nontraditional methods Vyse describes. Such pursuits suggest that most basic researchers likely agree with Vyse's argument that they should be open to using designs other than within-subject designs. Futhermore, SQAB, which Vyse referred to as ''a small band'' (p. 131) of basic researchers, featured a special section on delay discounting at the annual conference in 2010. SQAB also regularly invites researchers from a broad range of areas outside EAB (see SQAB.org for links to past programs and tutorials). From my perspective of the field, the openness is there.

I feel the need to point all this out after reading that basic behavioral researchers are ''plugging away at the kind of EAB esoterica that will never be of much interest to those outside

our little group'' (p. 128); that the primary goal of basic behavioral researchers is ''the promotion of a particular brand of science based in a unique set of methods'' (p. 128); and that ''many behavior analysts have become distracted with the goal of defending that grand theory and the methodological strategies that built it'' (p. 129). Perhaps I am naive or out of touch, but I thought the old battles pitting ''our field against theirs'' (e.g., Skinner, 1977) largely were over. I really do not identify with this approach to science at all and I know many others do not either. After all, most basic behavioral researchers need to stay relevant to obtain funding from granting agencies and to keep their labs open. For these reasons, I completely agree with Vyse that ''spitting contests about your grand theory versus mine are a distraction from the simple yet essential task of gathering new knowledge'' (p. 129).

In conclusion, efforts to broaden the range of questions asked by basic behavioral researchers can only increase further the variety of topics addressed by them. I wholeheartedly endorse Vyse's suggestion that we read widely and increase the breadth of graduate training to include more mathematics and to understand better the methods used in fields related to psychology. Although luck certainly is involved in the development of great research questions, such skills allow one to take better advantage of a great research question compared to those without these skills. For those of us who have completed our graduate training, the key to increasing one's relevance likely lies in collaboration with others, whether it is engaging in translational research with applied behavior analysts or with those in related fields such as economics, pharmacology, neuroscience, and so on. Collaboration saves us the formidable task of mastering more than one discipline when it already is a challenge to master one (see also DeLeon, 2011).

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