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Three Variations of Translational Research: Comments on Critchfield (2011)

Timothy R. Vollmer
University of Florida

I agree with Critchfield's (2011) thesis (to paraphrase): Behavior analysis must adapt; we cannot simply will ourselves into greater social relevance. Critchfield focused on the survival of the basic research arm of behavior analysis, known as the experimental analysis of behavior (EAB). Herein I will briefly categorize three general variations of translational research to make a case that translational research is an essential part of both our history and our future as a field, whether basic or applied or at any point along that continuum. I will attempt to relate these categories to comments and concepts presented by Critchfield, and my point about his thesis is that we should support and conduct translational research because it is genuinely useful to do so.

Variation 1: Applications of basic principles. Perhaps the purest form of translational research in behavior analysis involves the application of principles first studied in the basic research laboratory. Indeed this type of translational research provided the very basis for and foundation of applied behavior analysis. Principles first discovered through basic research are now commonplace in the application of behavior analysis. Dozens of prominent examples can be named, including positive reinforcement, shaping, extinction, timeout, discrimination training, punishment, differential reinforcement of other behavior, and so on.

In the early days of applied behavior analysis, one focus of the field was to demonstrate that such basic principles were applicable to socially relevant human behavior (e.g., Ayllon & Michael, 1959; Azrin, Holz, Ulrich, & Goldiamond, 1961; Azrin & Lindsley, 1956; Lovaas & Simmons, 1969). The translational focus of applied behavior analysis was famously detailed in the first volume of *Journal of Applied Behavior Analysis* (Baer, Wolf, & Risley, 1968), although *translational* was not yet a buzzword.

More recently, this first category of translational research can be seen in studies that involve, for example, application of the matching law principles (e.g., Neef, Mace, Shea, & Shade, 1992), applications of behavioral momentum (e.g., Mace et al., 1988), behavioral economics (e.g., Roane, Falcomata, & Fisher, 2007), progressive-ratio schedules (e.g., DeLeon, Iwata, Goh, & Worsdell, 1997), and so on. Also, although we already know that, say, positive reinforcement can influence human behavior, it can be considered translational when that phenomenon is demonstrated in some novel way, such as when teaching infants to sign before they can speak (Thompson, McKerchar, & Dancho, 2004) or when training giant African pouched rats to find land mines (Poling et al., 2010).

In his essay, Critchfield reminds us that Skinner (1938) had cautioned against allowing questions of ultimate application to influence the direction of science at an early stage.

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Address correspondence to Timothy R. Vollmer, Department of Psychology, University of Florida, Gainesville, Florida 32611 (e-mail: vollmera@ufl.edu).

Critchfield then pointed out that behavior analysis is no longer at an early stage. In a sense, Critchfield's redirection puts the onus of translation on the basic researcher. In my view, for this first category of translational research, the onus is at least equally on the applied researcher. Applied researchers not only apply basic principles because they can; they also apply basic principles because it is useful to do so. The very existence of applied behavior analysis shows that the indirect benefits of basic behavioral research abound.

I will never forget a time when I was working at the University of Pennsylvania (Children's Seashore House) and my students and I invited Lynn Hammond from Temple University over to visit our clinical laboratory. Hammond (1980) had done some elegant work on contingency manipulations with rats, and we were applying those principles to the treatment of severe behavior disorders displayed by children. He reviewed some of our data and watched sessions from a clinical treatment study we later published (Borrero, Vollmer, & Wright, 2002). At one point Hammond said, in words to this effect, "I never in a million years thought there would become anything useful of my study." Hammond had approached his question as one of "pure" basic research, and was astonished to learn it had implications for application.

Critchfield suggests that the basic researchers should now be aware of the potential or current influence of their research on application; I agree. Anyone writing a grant proposal or a research paper on any aspect of basic research (e.g., choice) should immediately cite the dozens of papers relating principles of reinforcement on concurrent schedules during behavioral treatments (e.g., Fisher & Mazur, 1997). I further suggest that applied researchers should be aware of new findings in basic research and bring those basic researchers into the fold via citation and collaboration. Then, maybe the basic researchers could allocate at least some of their time to the second variation of translational research.

Variation 2: Laboratory research to solve applied problems. In the first variation, there is an intentional application of a basic principle or set of principles. In the second type, there is laboratory research intended to solve an applied problem. Critchfield touched on several historical examples of laboratory research designed to address applied problems without sacrificing the methodological rigor of the time, including the work of Pasteur. In genetics, an example would be searching for the genetic anomaly responsible for a developmental disorder. Critchfield pointed to examples in behavior analysis, perhaps most prominently in pharmacological research aimed at testing the behavioral effects of commonly used medications for disorders such as attention deficit hyperactivity disorder (ADHD) or in testing substances of abuse. As Critchfield mentioned, this approach can be labor intensive because it requires the scientists to familiarize themselves with the basic methods *and* with the social problem at hand. Thus, many researchers adopt a strategy of cross-disciplinary collaborations. An excellent example of conducting basic research to solve problems related to application is summarized by McIlvane and Dube (2003), who have studied principles pertinent to autism intervention.

I strongly agree with Critchfield on the following point: Within our own field, we need coordinated collaborations between basic and applied researchers (e.g., Mace et al., 2010). We need to isolate basic behavior–environment relations to understand nuances of behavioral treatments and behavioral teaching approaches because understanding nuances will improve application. That type of research has much to entice the basic

researcher, such as variations on complex reinforcement schedules, evaluations of contingency and contiguity, effects of reinforcer delay, stimulus control features, and so on. By explicitly conducting research to address a particular problem or set of problems, such as autism, self-injurious behavior (SIB), or ADHD, basic researchers get a foot in the door of programs aimed at solving these behavioral puzzles. Imagine if every, or at least many, of the bourgeoning masters degree programs or medical-school-based clinical programs in behavior analysis had teams of basic researchers who coordinated their research with the applied faculty. How do we get to that point? Money. How do we get money? By demonstrating that it is *useful* to aim some of our basic research at applied problems. How do we do that? By coordinating our basic and applied research (Mace & Critchfield, 2010).

Variation 3: Translating the logic of behavioral methodology. Critchfield addressed methodology toward the end of his paper. My concerns about methodology are related, but can be summarized as follows: At times the methods of behavior analysis constrain our research questions. What I mean is that we have a very long history of measuring behavior that occurs at a high rate (e.g., lever press, SIB), behavior that is easily reversed or turned on and off in a single-subject experimental design, and behavior that is easily observed either through automated devices or through direct human observation. So, should we stop doing research on lever pressing and SIB? Absolutely not. Behavior analysis has the best game in town for treating the previously mysterious disorder of SIB and many other forms of high-rate behavior. Lever pressing and key pecking are useful for many purposes. However, at what point do we translate, beyond theory, the logic of our methods to address various social problems? At what point do we extend our understanding of principles to behavior that does not fit neatly into our standard methods? I am not saying this kind of translation is never done; it is just not done frequently.

I became acutely aware of a need for this type of translation several years back when, along with my colleague Michael Stoutimore and my graduate student Luanne Witherup, I was invited to sit on a committee to advise the Florida governor on the problem of running away by children under the state's ward (foster children). Several runaways had gone missing, become involved in crime, or even died while on the run, so this was a serious issue. We were invited because we were managing a foster-parent training program focused on reducing problematic behavior and increasing alternative behavior along with improving parent–child interactions (see Van Camp et al., 2008). What should we do about running away? Surely my graduate students could not sit in a bedroom and count how many times an adolescent climbed in or out the window. Surely we could not use repeated measures of individual behavior because sometimes the behavior occurred only once, and if we somehow "treated" it we would not wish to see it reappear. What about a functional analysis? We could not manipulate variables to turn the behavior on and off in the sense of Baer et al. (1968) or Iwata, Dorsey, Slifer, Bauman, and Richman (1982/1994). In the interest of space I will not present all of our proposed or actual solutions (but see Witherup et al., 2008, for some of the measurement issues). Rather, my point is to highlight that for behavior analysis to expand its social relevance (in Critchfield's terms), we must be able to design methods to address other types of behavioral problems. We should not let the cart drive the horse (by asking questions suited to our methods). We need not sacrifice

the logic of our methods and we need not construct explanations that stray from our principles, but we must adapt (translate) our methods in order to have a say in resolving the most socially relevant problems of our time, such as war, murder, behavioral reaction to disease, public education, and so on.

CONCLUSION

Critchfield reminds us that translational research is key for the field of behavior analysis. He specifically recommends that basic researchers conduct studies with direct social relevance. In an effort to say I agree, I summarized three general domains of translational research that could bring our field into greater social relevance. I suppose my point is that we should not be doing translational research to keep EAB alive, but we should do it because it is useful.

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