

You Are in the Way! Opening Lines of Transmission for Skinner's View of Behavior

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Published online: 25 April 2017

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Skinner's vision of behavior analysis was that it was a generic science that would one day be seen to be widely relevant for all human behavior. Unfortunately, his vision has not been realized. One of the reasons is its adherents in both Applied Behavior Analysis (ABA) and the Experimental Analysis of Behavior (EAB), its basic version keep getting in each other's way as we all try to make Skinner's vision a reality. This distresses me, to be sure. But it distresses me even more to watch adherents from other fields, fields based on unsubstantial conceptual and empirical foundations compared to behavior analysis, glide gracefully across the dance floor while we keep bumping in to each other. Rigid adherence to the prescriptions supplied in 1968 by Don Baer, Mont Wolf, and Todd Risley (Baer, Wolf, & Risley, 1968; BWR) is but one of the several ways adherents of ABA have gotten in each other's way. Critchfield and Reed (2017) make this case with considerably more conceptual and empirical style and substance than I will be able to muster, but I do want to join them on the case. As an aside, I believe the problem Critchfield and Reed discuss is representative of a larger problem in ABA, and indeed the entire field of behavior analysis, that has multiple components, including canonization of its founder and his writings, an obsessive attachment to an arcane vocabulary whose purpose is merely to describe simple behavior, over-emphasis on behaviors exhibited by persons residing in one tail of the normal distribution, and over reliance on a narrow range of research methodologies. There are other problems but you get the idea. For now, I will maintain (most of) my focus on the Critchfield and Reed article.

Before I begin, however, I need to disclose that BWR were my major professors at the University of Kansas. I took classes from each, consulted on my research and

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writing projects with each, and Mont Wolf was the chair of all my committees. I revered them and their ideas in life and, if anything, do so even more in the aftermath of their deaths. Having said that, ideas can retain their beauty and continue to evoke affection, admiration, and even love long after they have reached their maximum utility. Descartes' cogito ergo sum comes to mind, so do Newton's mechanics. The seven-dimensional framework proposed by BWR was a lovely set of ideas, and it evoked considerable affection from me and hundreds (thousands?) of others after it was published. It continues to do so, although to a lesser extent, to this day. Critchfield and Reed discuss this shifting sentiment. There remain a considerable number of adherents of BWR, however, who relate to their message as if it were timeless and continuously capable of evoking the music and logic of the spheres. In other words, they relate to it like it was a canon in the strictest sense of that word. But it is not a canon. It is merely a set of recommendations for how to create an applied version of behavior analysis. And as Critchfield and Reed argue, it is a paper of its time. This perspective is reflected in the title BWR chose: "Some Current Dimensions of Applied Behavior Analysis." BWR no doubt chose the word "current" carefully. They did not choose the word "timeless" or various synonyms for timeless (e.g., lawful, canonical) because they likely saw their paper as a good start and not as a canonical fait accompli. And Critchfield and Reed argue that at least an update or more accurately, a revision, is past due. This is not to say it needs to be rewritten entirely but rather that it, and the field, would benefit if it were amended to reflect the changing times and behavior analytic practices. I support this position.

My doctoral dissertation provides a good jumping off point. It focused on appointment keeping in a pediatric outpatient clinic (Friman, Finney, Rapoff, & Christophersen, 1985). Mont Wolf was the chair. The names of the other members are a matter of public record but I see no need to list them here. In addition to the committee, several students and a few other faculty sat in. In the middle of the defense, a protracted argument over whether my study actually involved ABA broke out. The concern was that my study did not involve ABA because I did not actually observe behavior, merely the artifacts of behavior (records of appointments kept and broken), and that my study did not involve actual people because it was a multiple baseline across five clinics. The person most strongly making the case that the study involved ABA was Mont. So, although he may have been committed to the idea that ABA must include direct observations of behavior in 1968, it seems his commitment to that idea had softened by 1984.

As reflected in Critchfield and Reed, when BWR nailed their seven propositions to the church door, so to speak, they served the field of ABA in at least two significant ways. Perhaps the most important service involved a sort of birthing process in which ABA was born or at least individuated from EAB. It also provided an immunological service that prevented trivial, remotely empirical, and or poorly controlled research from getting into the flagship journal of the field, the *Journal of Applied Behavior Analysis* (JABA). There was a downside to this particular service, however. As with any strong immunological system, some of the good gets caught up with the bad. That is, BWR also either prevented some very important behavioral research from being accepted by JABA or it discouraged prominent investigators whose work did not fit the

dimensions specified by BWR from even attempting to have their work considered by *JABA*. There are actually many exceptional ABA researchers that no longer submit their best work to *JABA*. These individuals are still very active in their careers but have sought audiences who are much friendlier to their work than those strongly influenced by BWR.

As editor of *JABA* (2004–2007), one of my jobs was to recruit submissions. And for 3 years, I argued with my editorial board over their highly restrictive reviewer practices. If papers did not conform religiously to the BWR specifications, they were all too frequently rejected. I beseeched the board to be a little more flexible with submissions in order to supply reinforcers for junior investigators, researchers new to the field, diverse investigations, and novel investigative methods. I made little progress (acceptance rates were flatlined). At one editorial meeting, I proposed that very harsh toilet training in early childhood was the most likely reason for the board's rigid editorial conservatism.

Here are three of several areas toward which I wish ABA would become more accepting and thus allow its adherents to get out of each other's way. The first is behavior modification. Several of the papers cited in Critchfield and Reed, and indeed most members of the editorial boards who served the Journal during my tenure, were antagonistic towards behavior modification and in favor of BWR analysis. ABA has produced highly successful treatments for a broad range of clinical concerns (e.g., incontinence, habit disorders, addiction, and autism). The research showing the success of these treatments is almost all behavior modification. Furthermore, treatments such as these have done more for disseminating and growing ABA than any of the research that more closely fits the analytical BWR model.

The second involves the resistance to direct replication. A frequent criticism directed at ABA research involves the small number of persons who have been shown to respond to ABA interventions. Refusing to publish direct replications keeps this number small. I contended with this one during my editorial years at *JABA*. An all too common basis for recommending rejection was that submitted research, although well controlled and accompanied by strong results, was not sufficiently novel to publish in *JABA*. My position was and still is this. If a study asks a socially significant question, addresses that question with an acceptable method, produces results suggestive of a functional relation, and tells a plausible story about that relation that has not been told many times before then it is acceptable for publication. Researchers should not need to show they grew hair on a bald head or raised a person from the dead to be published in *JABA*.

The third area in need of more acceptance is large N research. There is some irony about this one. First, Early Intensive Behavioral Intervention (EIBI) for autism is currently one of the most vital research areas in ABA. And the foundational study for EIBI was a large N study (Lovaas, 1987). Second, for many years, the Teaching Family Model (TFM) was the best known, most widely used, and most grant supported approach to treatment for out of home troubled adolescents (Phillips, Phillips, Fixsen, & Wolf, 1971). All of this was undone by a single large N comparative study that yielded results suggesting the TFM was not significantly more effective than community alternatives (Jones, Weinrott, & Howard, 1981). Third, behavior analysts design effective treatments for a broad range of clinical conditions. But for those treatments to be classified as empirically supported (the gold standard), successful randomized

clinical trials are needed (Chambless et al., 1996). This means the task goes undone, is done by people outside the field, or is done by persons inside the field who then publish their findings outside the field. Fourth, Don Baer himself was not opposed to the use of large N methods. He supported their use for answering what he called “brand X” questions (i.e., is brand X greater than brand Y). The study that undid the TFM was a brand X study. There are other areas in need of more acceptance and for a sample see Critchfield and Reed.

Let me address these issues from a different angle. Skinner promoted behavior analysis as a natural science. By taking this stand, he was promoting a larger idea, specifically that behavior was solely a physical phenomenon brought about, maintained, strengthened, or weakened solely by physical (environmental) events. In other words, he was promoting the idea that behavior is a function of environmental circumstances and their context. This is the most powerful idea ever invented by mankind for understanding, knowing, and approaching human behavior especially when it is a problem. The idea is not well known, understood, or widely used. One reason for this state of affairs, as noted by Skinner, is that the idea is in competition with some much older ideas that are well known, understood, and widely used. Specifically, these ideas attribute behavior to hypothetical constructs such as character, personality, morality, and the psyche. Another reason is that ABA seems to take little note of how important the means of transmission are for the dissemination of an idea (Friman, 2010). The easier the means of transmission, the more readily and widely an idea disseminates. But because of the various ways ABA gets in its own way, mentioned above, and in Critchfield and Reed, the means of transmission for the circumstantial approach to behavior are dauntingly difficult. The result is evident throughout the world. Despite the power of the circumstantial idea, and in the presence of the extraordinary results its application has produced (e.g., deinstitutionalization, cure for incontinence, normalized development in autism, effective treatment for addictions, anxiety, and depressive disorders), it loses out to the older ideas virtually everywhere except in behavior analytic clinics, labs, classrooms, journals, and conferences.

Transmission lines are more efficient for other ideas and fields (e.g., cognitive behavior therapy) and their adherents borrow (steal?) ideas central to ABA and repackage, repurpose, describe, study, and publish papers on them using more accessible language and less prohibitive experimental methods and acquire the credit and attendant resources (e.g., grant dollars, faculty lines, scholarly citations, consultantships, and students) that could and would accrue to ABA if only its adherents (i.e., we) would lighten up a little. More importantly, if ABA were more tolerant of methods that do not fit BWR but that do yield support for the circumstantial view of behavior, transmission of that view would improve and advance Skinner’s vision for the world in the bargain.

Compliance with Ethical Standards

Conflict of Interest The author declares that he has no conflict of interest.

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