The Fuzzy Outline of an Operant

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My history is in the *experimental-control camp* as Chritchfield and Reed (2017) describe it. And yes, I worked on the basis that establishing experimental control was of central importance to developing explanations for behavior. That makes perfect sense when you look into an operant chamber at the behavior you are studying. When problem-solving skills are guided by strategies that are grounded in the experimental analysis of behavior (EAB), then solutions emerge that both inform us about controlling variables and at the same time strengthen the value of the EAB in identifying these variables. The problems presented by rats and pigeons, however, are in fact problems generated by the contingencies to which they are exposed. Because of this, the behaviors were often dismissed as being as arbitrary as the arbitrary contrivances (i.e., schedules of reinforcement) that produced them and therefore of little relevance to our understanding of the natural world outside of the lab (cf. Ferster, 1978).

While these arbitrary behaviors may have resisted analysis on occasion, their study stimulated discussion of how best to develop a taxonomy that might make sense of the unlimited variations in contingencies that could be invented (Zeiler, 1984). As a student, I wondered if the solution might be the construction of a periodic table of basic principles. An applied perspective, I thought, might be necessary to achieve this. Imagine for a moment that schedules had not been invented and that for some whacky reason you wanted to generate a behavior pattern to be called a scallop, where a period of non-responding is systematically followed by a period of accelerating responding. In the operant chamber, this is doable because of the degree of control that is the privilege of any lab-based research (Keenan & Toal, 1991).

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Outside of the lab, though, the mere mention of control in the study of human behavior instills ire in those not familiar with EAB (Keenan, 2016). This of course misses the point of being escorted by what EAB has taught us in preparation for going outside the lab. The problem is whether it is possible to guide the behavioral stream observed in various applied contexts with the same degree of control as in the lab. The variety of publications discussed by Chritchfield and Reed (2017) shows what is possible and what is not possible thereby exposing what appears to be some limitations of the early “rules of engagement” for studying human behavior in applied settings. The difficulties encountered, though, are not to be confused with limitations of EAB per se. Instead, they can be regarded as examples of how investigative behavior adapts to the constraints that define different contexts. The outcomes produced by the investigators, then, stand in relation to outcomes possible in other contexts. This is essentially the main thrust of the target article. What they conclude is that the original definition of ABA as proposed by Baer et al., (1968) (BWR) effectively rules out a variety of investigative techniques from inclusion in a broader definition of ABA, hence their reference to *fuzzy ABA*.

If we remember that the BWR definition of ABA is but an outcome of the behavior of scientists controlled by the rules designed in a particular historical context, a point made by the authors, then there is room to maneuver in developing a taxonomy that embraces both the BWR definition and the concerns identified by the authors. I argue here that we can gain some traction from conceptualizing the issue as a question of how to deal with variability in the operant behavior we call *investigating behavior*.

One particular concern to the authors is the way some investigative procedures are treated because of how context forces them to relegate the importance of control in the analysis of behavior. This point, though, is not peculiar to the applied field. For example, I too ran up against the gatekeepers of “control” in a lab-based study that was rejected by *Journal of the Experimental Analysis of Behavior*. I designed one of the first studies to examine social categorization using procedures normally reserved for equivalence research (Watt et al., 1991). The procedures used had all the controls necessary for generating equivalence classes if I had been using arbitrary stimuli. However, in the context of pre-ceasefire Northern Ireland, I wondered what would happen if socially loaded stimuli were used instead. Performance during training went as expected (i.e., behavior was under control) but testing revealed something interesting. Catholics and Protestants from Northern Ireland produced different performances compared to participants who were not reared in Northern Ireland. This finding was dismissed as evidence of losing experimental control over the emergence of equivalence relations. No, it was not evidence of losing control! Rather, the production of variability was evidence of a different kind of control that lay in the histories brought to bear on a standard laboratory procedure! Similar results have since been replicated and have spawned other variations of the basic idea (e.g., Cartwright et al., 2016). The reason for drawing attention to this story is that it shows how lab-based studies can also cause problems for the gatekeepers of control in EAB, with the result that findings can be dismissed as not providing valuable data about behavioral processes:

If an empirical finding is totally unexpected from the perspective of the reigning theories, the initial scientific response will often be one of skepticism that the finding is genuine. Hence, the response to an early report on taste aversion

learning: “Those findings are no more likely than birdshit in a cuckoo clock” (reported by Seligman & Hager, 1972, p. 15). Even when an effect has been well documented, its status as a problem for a particular domain can remain unclear. Until a solution is at hand, it is often unclear to which scientific domain a problem belongs (Batts & Crawford 1991, p. 342).

The flip side of the BWR rules of engagement is the BWR rules for inclusion in the definition of what constitutes applied behavioral research. I believe Chritchfield and Reed (2017) have done a tremendous job of setting the stage for the field to view various manifestations of behavioral research in applied settings simply as examples of how an operant class adapts to a variety of contexts. Within the various strands of this operant class, the rules of engagement that have evolved can be viewed as examples of using an EAB perspective *as best as one can under the circumstances*. Lab-based individuals with no interest in the social world never engage in the issues addressed by those who do engage, and therefore, they never mature in the problem-solving skills demanded by different contexts in the social world, like the prevailing ethical restrictions that determine what can and cannot be done.

Because a central theme in each social context is the application of a natural science perspective, might it not be better to reserve the term *fuzzy* to characterize the rough outline of an operant? Figure 1 is an attempt to augment the figures provided by the authors by including the element of time. The investigating behavior of scientists across time and contexts is simply evidence of how an operant has evolved. The figure is not perfect, but it is offered as a way to stimulate discussion. For example, perhaps the locus labeled as BWR in the figure should be replaced simply with BA (behavior analysis). An updated figure, then, would still have to find a way to incorporate and characterize the role of BWR in the evolution of the other practices; does it function as a behavioral lens that entrains a range of practices, each of which then adapts to different contexts. Either way, the purpose of the figure is to stimulate ways to reconsider a positive perspective on the problems identified by the authors’ analysis of BWR.

Returning to the term “fuzzy ABA,” I would urge caution in promoting it at a time where there are already many branded versions of ABA that cause problems in the field of autism research. There is no need to rebrand the name ABA, but I believe there is a serious need for many of the strands in the operant class shown in Fig. 1 to rebrand. For example, *Positive Behavior Support* (PBS) could be rebranded as *ABA: PBS*. The same goes for *Early Intensive Behavioral Intervention* (i.e., *ABA: EIBI*), the *Picture*

Fig. 1 When time is inserted into the figures provided by Chritchfield and Reed (2017), then the range of procedures that evolve in the field of behavior analysis can be construed as an operant class, each stand adapting to the constraints of the applied setting in which it operates

Exchange Communication System (i.e., ABA: PECS), *Pivotal Response Training* (i.e., ABA: PRT), etc. By doing this, colleagues would be prevented from fragmenting a discipline. There is also an ethical reason for doing so. It would prevent confusion about the nature of their particular branded version of ABA in countries where ABA of any kind is struggling to emerge. Here, for example, is an argument against investing in ABA that appears from Research Autism (2017), a major charitable organization that informs the National Health Service on autism interventions in the UK:

Because there are many different interventions, programmes and techniques used to help individuals with autism which incorporate the principles of applied behaviour analysis it is not possible to provide a ranking for applied behaviour[al] (sic) analysis as a whole.

Across Europe, there is an explosion of interest in ABA for autism, but along with that, there is a market for those who want to sell their services. In my experience, there is little effort made to connect with local behavior analyst to help each country become self-sufficient in their ABA provision. Rather, a captive market is milked with the sole purpose of promoting a brand (Keenan et al., 2010). The result is that governments and others get confused with all the ABA-type interventions appearing on the scene. They have no way of making the discrimination a trained behavior analyst would make when recognizing these services as merely branded versions of procedures developed from the same discipline. Which version of ABA should they invest in? Do they need to provide training at university level if the commercial packages can do the job for them? Marketing of branded versions of procedures within ABA without aligning them to the science from which they are derived does a terrible disservice to those in other countries who struggle against the impression that ABA is simply one of a number of treatments for autism (Keenan et al., 2014). Parents often feel they have to choose between PBS and ABA, between VB and ABA, between PECS and ABA, between PRT and ABA, between EIBI and ABA, and between *each new commercially available package developed by behavior analysts* and ABA. A voluntary re-branding of procedures developed by colleagues would evidence their understanding of the problems they create by not doing so, and it would be a constructive step in helping clients/countries achieve independence in their service provision of training in the basic science, surely an important ethical issue. And that is just within autism! As I said, though, all of this is only possible if the term ABA is used in the way it was intended to be used, in bringing a science of behavior to applied settings. It is up to colleagues to ensure that rounding up brands does not descend into something akin to herding cats.

In conclusion, the paper by Critchfield and Reed, in time, may be regarded itself as iconic, as defined by the authors own depiction of this term. It provides valuable reasons for reflecting on issues arising from how broad the field has become. In many ways, the authors extend an invitation to what is essentially a healthy problem for the community to address, how to categorize the variety of methods and issues in such a way that they retain legitimacy in the use of the term ABA.

Compliance with Ethical Standards

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

References

- Research Autism (2017). Applied behavior analysis and autism. Retrieved March 1, 2017, <http://researchautism.net/autism-interventions/our-evaluations-interventions/120/applied-behaviour-analysis-and-autism>.
- Baer, D. M., Wolf, M. M., & Risley, T. R. (1968). Some current dimensions of applied behavior analysis. *Journal of Applied Behavior Analysis*, 1, 91–97. doi:10.1901/jaba.1968.1-91.
- Batts, B., & Crawford, L. L. (1991). Problematic progress: a review of Laudan's *Progress and its problems and science and values*. *Journal of the Experimental Analysis of Behavior*, 55, 337–349.
- Cartwright, A., Roche, B., Gogarty, M., O'Reilly, A., & Stewart, I. (2016). Using a modified Function Acquisition Speed Test (FAST) for assessing implicit gender stereotypes. *Psychological Record*, 66, 223–233.
- Chritchfield, T.S. & Reed, D. D. (2017). The fuzzy concept of applied behavior analysis research. *The Behavior Analyst*, in press.
- Ferster, C. B. (1978). Is operant conditioning getting bored with behavior? *Journal of the Experimental Analysis of Behavior*, 29, 347–349.
- Keenan, M. (2016). ABA and the nail that sticks out. ABA – klinec, ktorý vytŕča.... ŠPECIÁLNY PEDAGÓG, Časopis pre špeciálnopedagogickú teóriu a prax, 5. ročník, 2016, č. 1. p. 3–14 (Slovakian Journal of Special Education). Retrieved March 12, 2017, <http://docplayer.net/36812919-Specialny-pedagog-casopis-pre-specialnopedagogicku-teoriu-a-prax-rocnik-5.html>.
- Keenan, M., Dillenburger, K., Moderato, P., & Röttgers, H.-R. (2010). Science for sale in a free market economy: but at what price? ABA and the treatment of autism in Europe. *Behavior and Social Issues*, 19, 124–141.
- Keenan, M., Dillenburger, K., Röttgers, H.-R., Dounavi, K., Jónsdóttir, S. L., Moderato, P., Schenk, J. A. M., Virués-Ortega, J., & Roll-Pettersson, L. (2014). Autism and ABA: the gulf between North America and Europe. *Review Journal of Autism and Developmental Disorders*, 2, 167–183.
- Keenan, M., & Toal, L. (1991). Periodic reinforcement and second-order schedules. *Psychological Record*, 41, 87–115.
- Seligman, M. E. P., & Hager, J. L. (1972). *Biological boundaries of learning*. New York: Appleton-Century-Crofts.
- Watt, A. A., Keenan, M., Barnes, D., & Cairns, E. (1991). Social categorisation and stimulus equivalence. *Psychological Record*, 41, 33–50.
- Zeiler, M. D. (1984). The sleeping giant. *Journal of the Experimental Analysis of Behavior*, 42, 485–493.