WHY I AM NOT A RADICAL BEHAVIORIST

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ABSTRACT: Watson set the initial parameters for a science of behavior. The experimental analysis of behavior as developed by Skinner still has much work to do before claiming to be the science of behavior. The cultural environment has been shaping the behavior of behavior analysts in different directions. Emphasis on methods rather than theory has resulted in variants that make communication among them difficult or nonexistent.

Key words: radical behaviorism, science, variation, evolution

Why am I not a radical behaviorist? Because I am a behavior analyst living and working in the 21st Century. I am as good a behaviorist as all those colleagues who identify themselves as radical behaviorists, but “radical” was an adjective used by Skinner some 70 years ago to separate his behaviorism from other kinds in vogue at the time. “Logical,” “methodological,” and so on, are adjectives we use when teaching history. We don’t explain a thing by qualifying “behaviorism”, we create a problem to be explained to the unlearned. We are proud of the Skinnerian tradition, but we live in the second decade of another century. Behavior analysis today is a set of cultural practices shaped and maintained by a cultural environment. Those practices evolved from a set of rules first systematized by Watson. One hundred years of behaviorism led to a variety of species of this family, of which some underwent extinction while others dissolved into other families through crossbreeding (e.g., cognitive behavior therapy). The crossbreeding of behaviorism was so successful that today behavior is a concept used by all of psychology (e.g., Roediger, 2004). Some candidates to new breeds of a new, or modern, behaviorism have been presented recently (e.g., Baum, 2005, 2013; Moore, 2013; Rachlin, 2013; Staddon, 2001, 2013).

Oscillations in the Earth’s climate in past millenniums are mentioned as factors in speciation, a process linked to the development of new species originating from a common ancestor (e.g., Haffer, 1969; Vanzolini & Williams, 1981). The astounding variability in the Amazon fauna, for instance, has been attributed to long periods of dry weather alternating with rainy periods. Dry climate divided the huge forest into small forested islands linked by non-forest vegetation. A given forest species lived separated in colonies for centuries. Geographic separation led to a divergent evolutionary development. Behavior analysis may be undergoing a similar “evolution”:

For years I had had a sublime faith that the truth would prevail. I was quite content to get to my papers into print somewhere; those who needed them would find them. (It was a useful principle, for it permitted me to continue working in
Skinner may have looked for isolation intentionally, and isolation may be the DNA of behavior analysis, but the isolation of the first behavior analysts came from experimental psychology’s selecting cultural environment. None of the seminal papers on the experimental analysis of individual behavior were published in the prestigious Journal of Experimental Psychology: see, for instance, Dinsmoor (1951), Ferster (1957), and Sidman (1953). For a hybrid of experimental psychology and physiology, the Journal of the Experimental Analysis of Behavior was a safe refuge as far as the development of the new species was concerned.

A surviving species was born out of a mutation: Skinner’s Ph.D thesis. A research program for a new kind of behaviorism was presented in The Behavior of Organisms (Skinner, 1938). Ernest Hilgard published a review of that book in which he asked, among other questions, whether the system as formulated had some probability of competing with other systems in psychology (Hilgard, 1939, p. 121; Todorov, 2008). The first decades were promising, leading to positive expectations regarding Hilgard’s question (Keller & Schoenfeld, 1950; Skinner, 1945, 1950, 1953). The mutation generated a new species due to some peculiar conditions: A small cultural environment helped to protect new siblings. For years, a limited number of students from Harvard and Columbia universities met with Skinner and Fred Keller, hearing feedback from others and giving them feedback in turn, a condition for the production of new species in biology (Vanzolini & Williams, 1981).

Theories in human sciences may undergo a process of refuge formation (Ortner, 1984), and so may behavior analysis. As a cultural practice it was initially shaped by the first behavior analysts, in spite of unfriendly environments at the University of Columbia in the fifties. A favorable cultural selecting environment was provided by that initial group, the Journal of Experimental Analysis of Behavior. One good reason to start the new journal was given by its first Assistant Editor: “So, when Charlie returned from EPA with the news that we would start a journal, I was thrilled. He would be the Editor and I would be the Assistant Editor. Charlie had felt shut out from traditional publishing channels” (Gilbert, 1987, p. 475-477). The above mentioned Charlie was Charles B. Ferster, first JEAB Editor; Marilyn B. (Ferster) Gilbert was his wife. Behavior analysts were not completely shut out of the major journals of the fifties, however. For example, Dinsmoor (1951), Ferster (1957) Greenspoon (1955), Hearst (1960), Keller (1940) and Sidman (1953), among others, were publishing in journals such as Science, Journal of Experimental Psychology, and the Journal of Comparative and Physiological Psychology.

In any event, genes of the experimental method with \( n = 1 \) now had a much better chance of being selected. The refuge (in the sense of evolutionary biology) was enlarged later by the publication of the Journal of Applied Behavior Analysis, again restricted to experiments with \( n = 1 \). This is an especially favorable environment for the process of speciation, the development of a species with
characteristics such as to limit breeding to its own specimens (e.g., Haffer, 1969; Ortner, 1984). The experimental analysis of the behavior of individuals with $n = 1$ was not the only method for the development of behavior analysis, however (Todorov, 2009). For Skinner, the data to be analyzed would come from several sources: Casual observations, controlled field observations, clinical observations, controlled observation of behavior in institutions, laboratory studies of human behavior, and laboratory studies of the behavior of animals (Skinner, 1953; Todorov, 2009).

Judging from editorial boards and citation patterns, the two refuges, the Journal of the Experimental Analysis of Behavior and the Journal of Applied Behavior Analysis, functioned almost like geographical islands during several decades. Birds of a different plumage were afloat until Jack Michael rescued them in a new sanctuary, The Analysis of Verbal Behavior. The isolation of applied behavior analysts led to some questions, probably mutations or the work of recessive genes: Why animal labs for the teaching of behavior analysis? Why say that we have no answer yet for a question when we can put forward educated guesses? Why bother with theories when we have useful methods?

The emphasis on methods led to a new breed, the cognitive-behavioral therapists. The new label has more survival value in the market: “Behaviorist, me? No! I’m a cognitive-behavioral therapist.” Or, when addressing an audience of parents of children with autism: “No, I’m not a behaviorist, I’m a humanist that uses the ABA method.” The market favors also the development of new brands of psychotherapy. Amenable to the effect of consequences, as all operant behavior is, applied behavior analysis may disappear as a label in favor of many descendants, each one developing a language proper to its theoretical/ecological niche. Behavior in organizations uses a “behavior systems theory” approach already. Multiprofessional teams seem to generate multitheoretical word salads. The word emergence seems to work as an attractor, and not only for behavior analysis: There is a huge selecting cultural/theoretical environment for that (e.g., Johnson, 2001; Sandaker, 2010).

Is behavior analysis a science of behavior or the science of behavior? I favor the second definition. I believe that the next generations of behavior analysts should continue their work in the direction of what Hilgard (1939) pointed out as a possible future for Skinner’s (1938) theory in The Behavior of Organisms: The validation of the theory when confronted with other ways of addressing the same facts. That endeavor, however, is impossible when working in isolation. Thus, the question is, how can we maintain contact with the so called behavioral sciences and at the same time protect our theoretical genes? Genetic engineering has been successful in improving seeds so that wheat now is harvested in new regions; new seeds are resistant to weather variation and plagues. Successful engineering is responsible for the productivity of agriculture. We may increase the theoretical productivity of behavior analysis by improving our teaching methods and objectives, especially the objectives: How to teach behavior analysts to grow in new environments.
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References


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