

JEAB AND THE SKINNERIAN INTERPRETATION OF BEHAVIOR

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At one time, *JEAB* editorial policy was perceived by some to consist mainly of dogmatically enforcing a Skinnerian interpretation of all findings reported in the journal. Partly in response to that undesired reputation, the journal explicitly defined itself as the place to publish research on the behavior of individual organisms, and not as a place that encourages any particular theoretical orientation. My own view is that this should not be the journal's identity; instead, *JEAB* should be the one journal that seriously considers Skinnerian (and related environment based) interpretations of empirical results, whether or not the study involved an analysis of individual subject behavior and whether or not the Skinnerian analysis is ultimately endorsed by the author.

Key words: Skinner, *JEAB*

My perspective on *JEAB* is shaped not only by my background in operant psychology but also by my extensive experience working on topics traditionally associated with cognitive psychology. From 1993 to 1996, I served as an associate editor of *JEAB*, and from 2002 to 2006, I served as an associate editor of *Cognitive Psychology*. That diverse background provides the basis for the main point I make in this essay.

I have watched editors strive over the years to define *JEAB*'s mission, and I have a vivid memory of reading an editorial on that topic written by Michael Zeiler (1977). In that editorial, he stated that the journal's primary mission was to publish work on the behavior of individual organisms, not to publish work that is interpreted in Skinnerian terms. In fact, he specifically said, "Actually, the journal never legislated a particular theoretical or metatheoretical stance..." (p. 1). This caught me by surprise because I had thought of *JEAB* as the place to publish findings that are interpreted from a Skinnerian perspective. I still do. Against that way of thinking, but in agreement with Zeiler's position, the masthead of the journal states: "The *Journal of the Experimental Analysis of Behavior* is primarily for the original publication of experiments relevant to the behavior of individual organisms." Indeed, it says nothing at all about theoretical orientation.

Focusing on the behavior of individual organisms, not on a particular theoretical orientation, is a reasonable approach to take, and it seems that most of those who identify

themselves as behavior analysts would agree with it. But for reasons I describe next, I have come to believe that *JEAB* should define itself in exactly the opposite way. I am not suggesting that *JEAB* should be run by Skinnerian "thought police" who rigidly enforce a single ideology (a point that Leonard Green, 2004, made in his more recent editorial as well). Instead, I am suggesting that there should be a place where Skinnerian and related environment-based interpretations of data are considered and that *JEAB* should be that place. Thus, for example, if an author submits a manuscript that presents a purely cognitive interpretation of the experimental findings it reports, that author would be encouraged to consider a Skinnerian account as well (even if, in the end, the author wishes to argue in favor of the cognitive interpretation). As I see it, *JEAB* would be the place where Skinner's brilliant ideas about everything are considered and taken seriously, not where they are necessarily endorsed. That would be the journal's identity, and it would welcome both single-subject methodologies (which it already welcomes) and group methodologies (which it generally eschews). Note that I am not advocating a new era of abstract debate about the general value of a Skinnerian approach versus a cognitive approach. Instead, I am suggesting that, in *JEAB*, Skinner's ideas would be routinely considered with respect to empirical data that were collected using methods that are widely regarded as being scientifically valid (including group designs).

I realize that Skinner was vitally concerned with the practical control of human behavior, and for those whose interests fall along those

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lines, an emphasis on the analysis of individual subject behavior is understandable. But many researchers who specialize in other areas of psychology are not as interested in the practical control of human behavior. Instead, they are seeking to conceptualize human behavior in ways that make it less mysterious, and they use group designs and standard statistical tests to do so. What they do not do—ever—is analyze their results in Skinnerian terms even though they could. Just because Skinner was concerned with the practical control of behavior does not mean his ideas about behavior only apply when behavior is studied with that in mind. His ideas also apply to many of the issues that are studied by cognitive psychologists despite the fact that they favor group designs. His ideas apply to that domain, but they are rarely (if ever) considered.

As matters stand, authors who publish in *JEAB* tend to work on certain issues (e.g., the quantitative law of effect, choice, self-control, conditioned reinforcement, equivalence classes, etc.), whereas authors who publish in journals like *Memory & Cognition* work on seemingly unrelated issues (e.g., recall and recognition, attention, psycholinguistics, etc.). At first glance, this might seem like a perfectly acceptable state of affairs because everyone knows that the world is becoming increasingly specialized. Authors who publish their work in *Perception & Psychophysics*, for example, are similarly insulated from those who publish in *Memory & Cognition* and in *JEAB*. A major difference, though, is that Skinnerian thought is overarching and applies to many domains, including cognitive psychology (and, perhaps to a lesser extent, perception). Skinner's ideas apply to those domains, but the influence of his thinking in those areas is hard to detect, and that is partly because operant psychologists find the approaches used by cognitive psychologists (namely, large group designs) to be methodologically abhorrent. Those approaches *are* methodologically abhorrent if the goal is to apply the results to, say, the treatment of autism or mental retardation (i.e., to the practical control of human behavior). To do that, one needs to demonstrate the ability to predict and control behavior at the level of the individual subject. But the goal of much research in other domains of psychology is, essentially, to be able to conceptualize why we behave as we do (even if the applied implications are not

immediately obvious). In these other domains, Skinner's thoughts are very relevant, but they are not influential, and that is a shame.

Skinner was a giant in the field of psychology, and I believe that even cognitive psychologists would concede the point. Even among giants, he was exceptional in that he offered ideas that are broadly applicable, like William James did before him, but unlike what most other notable figures do, such as Noam Chomsky (whose ideas are mainly limited to psycholinguistics) and James Gibson (a luminary in the field of perception). Even so, people in other fields do not seem to know much about Skinner's penetrating insights into human behavior. To many of them, Skinnerian research is concerned with schedules of reinforcement and pigeons, and that's about it. The truth is that Skinner's ideas are often applicable to what they do and how they think. As such, having a journal that is characterized by the consideration of Skinner's ideas (always with respect to actual data) makes sense to me.

As a concrete example, one that happens to hold my current interest, consider how we learn about certain private events. I remember being surprised by what Skinner had to say about this in *Science and Human Behavior*. His essential point, if I have it right, is that we learn about private events in much the same way that we learn about everything else (i.e., by consequences, many of which are delivered by the verbal community). This may not apply to direct sensation (e.g., I can tell the difference between a hot stove and a cold one by touch without having to learn that discrimination), but it seems likely to apply to much of what we refer to as "mental life." About this Skinner (1953) said: "The environment, whether public or private, appears to remain undistinguished until the organism is forced to make a distinction. Anyone who has suddenly been forced to make fine color discriminations will usually agree that he now 'sees' colors which he had not previously 'seen'" (p. 260). The idea that we learn to form internal discriminations based on external consequences still strikes me as a fascinating insight, one that has considerable relevance to issues of interest to cognitive psychologists and neuroscientists. Even so, Skinner's brilliant idea about this has not had much impact on how people think about these issues.

To make this more concrete still, consider the confidence you sometimes express in the accuracy of your memory for a prior event. You might say, for example, that you remember reading a novel during your last vacation in Hawaii and that you are almost sure that the novel was *Shogun*. Almost sure? Is that expression at all meaningful? If so, how did you acquire the ability to appropriately characterize your degree of confidence in this slightly fuzzy memory for a past event?

As it turns out, people are amazingly adept at characterizing the accuracy of their own memories. This fact is sometimes obscured by research that focuses on the additional fact that this ability is less than perfect. For example, across a set of decisions in which subjects are, on average, 55% accurate, they might be, on average, 65% confident. And across a set of decisions in which subjects are 90% accurate, they might be 95% confident. The consistent degree of overconfidence is interesting, but, to me, the fact that confidence and accuracy are so strongly related (despite the consistent bias) is far more interesting. How did people acquire that ability?

The question remains interesting even though other research shows that people can be fooled into confidently believing that they remember something when they clearly do not. In the Deese-Roediger-McDermott procedure (Roediger & McDermott, 1995), for example, subjects are presented with a list of words that are all related to another word that was not presented (e.g., they are all related to the word *sleep*, which did not itself appear on the list). On a subsequent recognition test in which subjects are asked to discriminate between targets that appeared on the list and lures that did not, the word *sleep* is very likely to be endorsed (often with high confidence) as having appeared on the list. Results like these show that memory researchers can create memory illusions in the same way that vision researchers can create visual illusions. These interesting exceptions notwithstanding, the question remains: How is it that, usually, people are very good at expressing an appropriate level of confidence in their memories? How did they learn to do that? That question is not often asked, and I am not sure that it has ever been considered in light of Skinner's ideas.

This question about confidence is a question about private events. That is, presumably, a subject is basing a confidence judgment on events that only he or she is privy to. Those private events presumably do not come with instructions about how to use them. Instead, it seems much more likely that individuals discover that, sometimes, their memories are accurate and that, at other times, their memories are inaccurate. That is, they form the discrimination between memories that are accurate and memories that are not in much the same way that they learn to form a discrimination between, say, fine shades of green.

The point of this extended example is simply that any discussion about the relationship between confidence and accuracy that might appear in the literature rarely, if ever, takes into consideration what B.F. Skinner might have had to say about it. The place where this might naturally be addressed—in the pages of *JEAB*—is not particularly welcoming because the methods used to study confidence and accuracy almost always involve group designs. Researchers could, of course, use single-subject designs to investigate this issue, and they could interpret their results in purely cognitive terms. If they did, they could publish in *JEAB*. To me, it would be better for the journal to encourage Skinnerian thinking (or, more generally, to encourage environment-based explanations) to a greater extent than it encourages single-subject methodology.

In summary, I am suggesting that *JEAB* should someday consider opening its door—wider than it is now—for analyses of any behavioral phenomenon using any scientifically acceptable experimental design that seeks to apply Skinner's ideas about learning to issues that are traditionally of interest to people who specialize in other fields. In fact, that is what I thought *JEAB* did until I read Zeiler's (1977) editorial. I am still stuck on that thought.

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