sionally reworking a passage if there was an obvious problem, and generally making sure that I would be happy with the paper. As 1976 dragged on, more and more frequently a second manuscript or even a third or fourth would come in before I had sent the first to the Managing Editor, Garth Hopkins, for final copy editing and marking for the typesetter. I still have the yellow sheets on which I used to keep track of the stages each paper went through during its final days with me. They now tell me that it took, for my four November issues, means of 4.7, 5.3, 4.2, and then, in 1976, 10.7 days for them to get through final processing. The editorial term was extended to four years in 1979. If the manuscript flow ever increases again, I would recommend it be cut back to three.

Like many of those who have edited JEAB, I've had the dismaying experience of having a paper received with less than enthusiasm by my favorite journal. The first paper that Bernie Weiss and I submitted to Charlie Ferster described a study of a drug on Sidman avoidance. Still under the strong influence of a traditional psychology graduate program, we used a 2 × 2 × 3 factorial design embedded in a Latin square, with R–S, S–S, and dose as the three variables, all this done with two rats that varied slightly in sensitivity to the drug. The results were muddled, to say the least, and we had some difficulty describing them clearly. But we thought we had a publishable contribution and sent it to JEAB. One reviewer agreed with us and recommended publication without change. The other wrote a two-page, single-spaced commentary that praised some aspects of the work but carefully pointed out all the many ways we had gone wrong. The review was a beautiful example of how an incisive critique could be couched in gentle language, with no trace of the superciliousness that occasionally mars a review; it was like a letter from a friend assessing the strengths and weaknesses of the work. Charlie Ferster didn't accept or reject the paper. Instead, he wrote the enormously flattering words: "You fellows are in the same league; make your own decision."

The long review had started on a page provided by the journal and Charlie had cut out the reviewer's name. But he had done a sloppy job, leaving the tops of some letters. It was easy to check the Board of Editors and find that only one member had a five-letter name starting with *B*: Boren. We knew and admired John and, after a second reading of his review, concluded that there was no way short of starting over that we could rescue the experiment. We tossed the paper into a drawer and never published it. The critique itself taught me something about reviewing and helped me develop my own voice when, a few years later, both Bernie and I were appointed to the board by the next Editor, John Boren.

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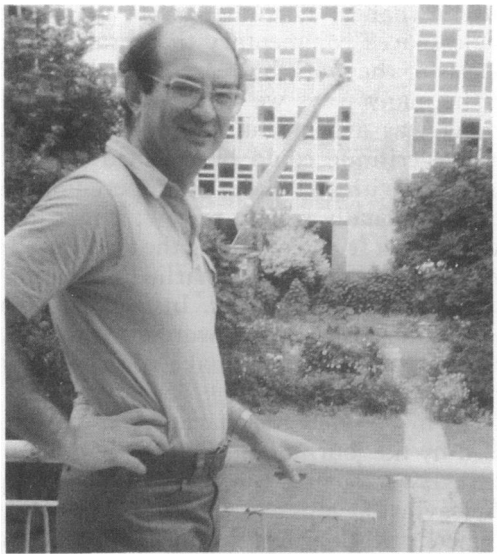
Michael D. Zeiler (Editor, 1977–1979)

TWO SIDES OF BEHAVIOR

My inheritance of the editorial mantle followed years as an Associate Editor for Stan Pliskoff and Vic Laties, a history that gave me first-hand exposure to editorial professionalism. It also followed a sabbatical year spent in London. As Editor-elect, I took part in discussions which developed my awareness of some readers' uneasiness about contemporary developments. As far as I could fathom, their objections stemmed from confusion over the nature and purpose of the Journal.

Readers of JEAB know its debt to the seminal work of B. F. Skinner. By the time I became Editor, this tradition had led to either ambiguity or crisis, depending on individual perception. Skinner's monumental influence



Michael D. Zeiler, 1977.

was the development of a science based on the lawful behavior of individual subjects. The Journal's statement of purpose testifies to this commitment: "The Journal of the Experimental Analysis of Behavior is primarily for the original publication of experiments relevant to the behavior of individual organisms." A publication so dedicated obviously accepts the reality of a science of individual behavior, and this is JEAB's only statement of mission. The phrase "experimental analysis of behavior" in the title supplemented by the statement summarizes what it is all about.

But Skinner stands for more than the experimental analysis of behavior; he also says how behavior is to be conceptualized. The two aspects of his thinking resemble the distinction between methodological behaviorism and Watsonian Behaviorism. Skinner combines the experimental analysis of behavior with the theory of Radical Behaviorism. The theory states the important processes, describes the relation between mental events and public behavior, and emphasizes the concepts of reinforcement and contingencies; but it also postulates rule-governed activity, decides on the role (or nonrole) of intervening variables, and the like. As far as I could determine, JEAB was committed to the experimental analysis of behavior, but it had no essential tie to Radical Behaviorism or any other theory or philosophy of behavior.

So, I was taken aback to learn first in England and later at a meeting in the U.S. that the Journal was being subverted by "creeping cognitivism" and other deviations. Cognitive analyses might be departures from Radical Behaviorism, but why are such criticisms relevant to a journal dedicated to the experimental analysis of individual behavior? No one criticized the way the offending experiments were done or how the data were analyzed; they were shocked only by the way experiments were interpreted or the reasons why the experiments were conducted. My longstanding convictions and editorial training did not coincide with such views. Emphasis in reviewing papers should be on the procedures and the data; interpretation is up to the author. An editor may criticize and suggest, but as long as authors are intelligible and relate their ideas to the experiment, they have the right to be foolish, wise, offbeat, or stolidly orthodox in introductory and discussion sections. An experimental journal is not the place to legislate theory or ideas.

My attempt to maintain the Journal free of theoretical rigidity yielded one pronouncement during my tenure as Editor. In contrast to earlier days when results sections routinely contained cumulative records showing moment-to-moment behavior, some authors now reported only data summed over numerous sessions. Relative frequencies of choices among alternatives or even response rates are meaningless with respect to momentary behavior, so some degree of molar analysis may be all that interests the author or the current reader or is relevant to contemporary theory. But the journal is archival and theoretical fashions change, so it seemed essential for every paper to at least indicate something about variability with respect to smaller points of fracture, even if the author chose to deal only with extremely molar features. As far as I could tell, this legislation was accepted with equanimity by all.

The impact of theoretical ecumenicism is not clear. Did it change the Journal? Probably not, because JEAB always was liberal. Did it contribute to the decline of behavioristic psychology? Maybe so, but probably not importantly given the nature of JEAB's natural constituency. Even that possible influence refers only to theory, not to the experimental science of behavior. History may or may not justify Radical Behaviorism or other behavioristic theory, but the success of the experimental

analysis of individual behavior as scientific method cannot be denied. That, I think, was, is, and will continue to be the importance of the Journal.

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John A. (Tony) Nevin (Editor, 1980–1983)

VARIATION AND PROGRESS

Evolution, it is said, progresses by variation and selection. The same can be said of science. Scientific journals are essential components of the process, both for the individual investigator, whose experiments are not really complete until written up and submitted to the scrutiny of scientific peers, and for the research and theoretical community, which relies on the steady accumulation of archival data for its progress. The principal role of a journal is selection: to consider the variations submitted by the scientific community, and allow some, but not all, to survive as archival contributions.

When I became Editor of JEAB in 1979, I had served two terms on the Board of Editors, and had served as Associate Editor with Stan Pliskoff and Vic Laties. Thus, I had done my share of selecting. As Editor, I wanted to do something more, and made efforts to encourage variation, both in my editorials and in many conversations at ABA, APA, and EPA meetings. The results, I think, were pretty good: Of the 250 articles published during my time in office, only about half dealt primarily with fundamental topics such as stimulus control and reinforcement contingencies. The remainder explored increasingly complex stimulus-control problems, and relatively biological topics such as conditioned aversions, induced behavior, and foraging. Studies of human verbal and social behavior appeared in the Journal's mail, and a few of them made it into its pages. Several substantial review articles were submitted and published (one of them, by Buzsáki, invited at Vic Laties' suggestion). Quantitative analyses of schedule performance became increasingly frequent, and inspired a number of theoretical papers that summarized the data well and showed how their major features could be derived from mathematical statements of behavioral processes. The varied expertise of the Associate Editors who served with me—Ed Fantino, Lew Gollub, Phil Hineline, Andy Lattal, Stephen Lea, Eve Segal, and John Staddon—ensured fair and informed consideration of articles in virtually any area of behavior analysis. All in all, I believe that the diversity of authorship and the number and range of topics addressed by articles published in the early 1980s increased over the previous two decades.

And this, I think, is a sign of good health. A recent article by Gould, Gilinksy, and German (1987) argued that evolutionary groups tend initially to diversify, becoming narrower only as they near extinction. If the same holds for our science, then growing diversity is a sign of health and relative youthfulness, contradicting the oft-asserted demise of behavior analysis.

J. A. Nevin, 1986.