Nathan H. Azrin (Editor, 1964–1966)

BEHAVIOR IN THE BEGINNING

JEAB, where were you when I needed you? In 1955, I had finished my dissertation research and was deliberating with Dr. Skinner on where to publish it. (Others may call him “Fred”—folks who scarcely know him, or he, them—but to me even “Mister” seemed presumptuous.) He suggested the *Journal of Psychology*. One alternative was *Science*, which was receptive to operant behavior studies but usually published one-page reports. The *Journal of Experimental Psychology* was not receptive and was too enamored of statistics rather than the behavioral referent of the numerical transformations. So off my manuscript went to *Journal of Psychology*, where it appeared in a 1956 issue that is the only one of that journal I have ever looked at. We have come a long way; I now subscribe to ten journals that are strongly behavioral in orientation and wish I had the time to read even the abstracts from several others.

I can still recall a meeting in a hotel room at the EPA convention in 1957. Nat Schoen-

feld was surveying the small assemblage to determine the number of manuscripts each of us anticipated submitting per year for the “newsletter” I thought was being considered. Joe Brady, Murray Sidman, Charlie Ferster, Og Lindsley, Dr. Skinner—everyone there responded with estimates of “Five,” “Four,” “Six,” “Three,” and so on, with no number less than three. When my turn came, I hesitantly whispered “One” and wondered how I would find an excuse if I couldn’t deliver. The estimates were so impressive that Nat and the others concluded that a newsletter would be inadequate and a journal, which had also been discussed by some of the participants, was needed. And the Journal was born. In due time, most of us delivered our articles. More important, so did many others whose identities were unknown at the time to the founders. The initial burst of manuscripts slackened and JEAB was, for a time, coming out very late because it did not have enough to publish at the promised quarterly intervals. But soon the flow of manuscripts was sufficient to sustain us and, indeed, seven years later, my first year as Editor (1964) was the first to feature bi-monthly publication.

Once JEAB began, I looked forward to receiving it in the mail just as I always looked forward to attending the annual EPA meetings; each issue was rather like a family reunion, where I could see what my friends were doing. Authors, editors, readers—we were a small group alternating in taking different roles with each issue, even with each article. JEAB helped satisfy our obsession with rigorous research, our need for a sympathetic publication outlet of high quality, and our hope of creating a more descriptive science of behavior. And it also meant that we didn’t have to rely so much on carrying sample cumulative records to conventions to share with like-minded enthusiasts.

As the third Editor of JEAB, after Charlie Ferster and John Boren, I was given no guidance as to how to run the Journal. I had served as Associate Editor under John, along with J. M. (Mike) Harrison and Roger Kelleher, and they continued as my Associate Editors, joined

N. H. Azrin, 1968.

by Bill Morse after about two years. The Journal was too young to have strongly established policies and the field was too new and full of surprises for one to feel that a title conferred special wisdom. From the founding, we all had felt so strongly about precision and objectivity that the title of Apparatus Editor had been created and Douglas Anger took on the position for several years. I did it for a couple of years, feeling a strong personal commitment to defining both response and stimulus conditions in as standardized a fashion as possible. I wanted to have physical definitions that were obtained through apparatus rather than the much more nebulous social definitions that sometimes were used. In fact, much of my own research at the time was devoted to developing ways of measuring human behavior with precision. During my term, Thom Verhave and then Bill Holz took over as what had been renamed Technical Notes Editor.

Lower animals were used by most investigators in large part to control for unknown histories, yet we cherished human studies such as those by Ted Ayllon, Jack Michael, and Hal Weiner, because we felt that the general application of our approach in applied settings was imminent. JEAB continued in my years

to be receptive to studies with any subject matter—retardation, drugs, education, psychosis, imprinting, aggression, sex—so long as high standards of evidence and experimentation were met. But, in considering whether a paper in a new field was publishable, we always tried to remain true to Charlie Ferster's admonition that one of the Editor's principal roles in making editorial judgments is to "protect the author from the reviewers," balancing definitiveness against importance. I am forever grateful to him.

In 1967, when we first investigated whether we should start a second journal, one devoted to applications, I was put in charge of investigating its feasibility. And now I found myself doing what Nat Schoenfeld had done in that hotel room at EPA ten years earlier: determining whether there were enough active researchers who would promise us their best work to warrant the founding of a new journal. There were, and we started the *Journal of Applied Behavior Analysis*.

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A. Charles Catania (Editor, 1967–1969)

EDITORIAL SELECTION

I first heard about JEAB while I was an undergraduate at Columbia, and I saw its first issue during my first year as a graduate student. But my involvement with it really began later, with the publication of a few papers and then the occasional review of manuscripts. I assume most of my reviews were reasonably competent, but the one I remember most vividly was not. I had published only one paper on human operant behavior, and during his editorial term Nate Azrin sent me a human operant manuscript for review. I recommended its acceptance, but Nate rejected it on the grounds that no evidence was provided that

the putative reinforcing consequences were indeed effective as reinforcers. I think I learned the lesson, but Nate never again sent me a manuscript on human operant behavior for review.

Nevertheless, I must have done some things right. In early 1966, while I was in the midst of wiring an experiment on the electromechanical equipment of the time, John Boren telephoned me on behalf of SEAB to ask whether I would be a candidate for the editorship of JEAB. I was surprised; after all, I had received my PhD only a little more than five years earlier. I learned much later that the