

Joseph V. Brady

BACK TO BASELINE

It will come as no surprise that my JEAB reminiscences, like many another, focus upon that smoke-filled hotel room at an EPA meeting in 1957 where the decision having been taken to "start our own journal," a head-count was required to ensure the availability of papers in sufficient numbers to at least fill the first issue. But while this focal occasion in the launching of the Journal stands out, the antecedents and consequences of that momentous undertaking provide an array of equally noteworthy remembrances.

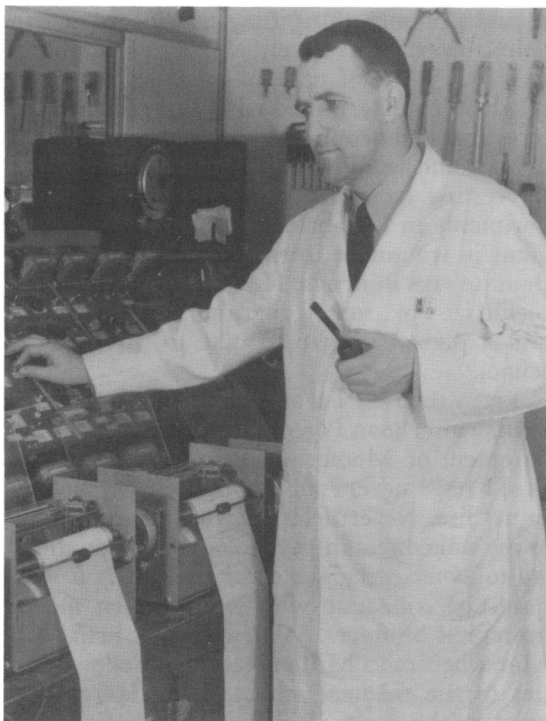
In the early 1950s, Murray Sidman and I had come to Dave Rioch's newly minted neuropsychiatric laboratory at the Walter Reed Research Institute in Washington, and a healthy exchange with the "Harvards" and "Columbias" (Fred Skinner, Nat Schoenfeld, Fred Keller, Charlie Ferster, Jack Findley, Dick Herrnstein, John Boren, Bill Morse, Peter Dews, Nate Azrin, and a host of others) was fostered by the informal CEAB meetings in Boston and New York that followed the earlier Indiana initiative. But our publication outlets in the more formal journal media were both limited and hampered by heated disagreements and unsympathetic exchanges with even those psychological periodicals that had some of our names on their mastheads as editorial consultants. Perhaps the depth of our despair in this regard is best illustrated by the following anecdote to which I can bear personal witness.

I had published several papers during the mid- and early-1950s in what was then perhaps the high-prestige journal of its time—the *Journal of Comparative and Physiological Psychology*—when our laboratory at Walter Reed was graced by a visit from the Editor-in-Chief of that esteemed periodical and the then-current President of the American Psychological Association, one Harry Harlow. The purpose of his visit, as it turned out, was to inquire about my willingness to serve as a consulting editor for JCPP since the number of submissions in "behavioral pharmacology" had been increasing and Harlow thought I might be of

some help in reviewing these papers. In addition, he observed, a goodly number of these manuscripts were "Skinnerian" in orientation and he had noted my methodological, if not my radical, dispositions in this regard. Only a few years beyond my degree at the University of Chicago with Howard Hunt at the time, I was more than a little flattered by this obviously premature invitation, and as a small token of my appreciation offered Harlow a tour through the newly mechanized lab that Murray Sidman and I had not only conceived and designed, but actually put together with our own hands.

Upon viewing this confusing array of relay racks and assorted snap leads, Harlow expressed his obvious dismay at not being able to see the animals under study—confined of course, to experimental chambers in adjacent sound-attenuating cubicles—and inquired as to how we could possibly keep track of their behavior under such conditions. We proudly displayed our several Gerbrands cumulative recorders at this point and explained their functional properties in somewhat casual fashion, preferring to emphasize the experimental questions being addressed at this behavior-analysis frontier. Harlow endured our detailed account of "knees" and "elbows" in the "bloody red" cumulative records rather impatiently, all the time eyeing the strip chart inquisitively before posing the question that came to be for me the turning point in the decision to support the JEAB movement. Tracing the cumulative record to its apex on the 900-response Gerbrands and then moving his finger precipitously downward as the reset brought the red pen line back to baseline, Dr. Harlow—President of APA and Editor of JCPP—astutely inquired, "How do you get the animal to come back down like that?"

I can't imagine that anything I could add in the way of further reminiscences could top this as an "occasioning condition" for our pioneering efforts in "opening our own store"! The consequences—both acute and chronic—of that undertaking over three decades ago have



Joseph V. Brady, 1955.

influenced my personal and professional life, as I'm sure many of my colleagues, old and young, will testify. In the early days, I spent a good deal of time in Indianapolis while Charlie Ferster—doing the first editorial job in ½ day a week, he claimed—turned the task of making readable English out of our “scientific jargon” over to Marilyn Ferster. He and I would then take off for Alamagordo, New

Mexico, to train chimpanzees for the National Aeronautics and Space Administration's Animal Pretest Flights of Project Mercury. After Charlie turned over his editorial post to John Boren in our laboratories at Walter Reed, it became obvious that this was not a “½ day a week” job and the realities of proprietorship and its burdens became painfully apparent, remaining with us for some years.

One final reminiscence about those early years. Shortly after John Boren took over as Editor of JEAB, Charlie Ferster moved to Maryland and joined us in another ambitious initiative involving the Walter Reed, on the one hand, and the laboratories that Dick Herrnstein, Jack Findley, and I had started at the College Park Campus of the University of Maryland, on the other. Once again, we felt called upon to “open our own store” since neither the University nor the Federal establishment seemed to satisfy our “cutting edge” experimental and applied behavior analysis needs. As a result, the Institute for Behavioral Research was born and shortly thereafter Charlie submitted a paper to JEAB on some of the initial experiments conducted at this independent bastion of scientific freedom. When he received a letter of rejection from John Boren some weeks later, Charlie Ferster confided to me that he thought it might be time for us to start a new journal!

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AN OUTSIDER ON THE INSIDE

When I arrived in Boston to join Otto Kray-er's Department of Pharmacology at the Harvard Medical School in January 1953, Fred Skinner had already been in contact with Kray-er suggesting that he had techniques that might be useful to pharmacologists. (The Harvard Medical School is in Boston separated by a river and about 4 miles from most of the rest

of the university, and the Department of Psychology, in Cambridge. Fred had spent time at the medical school when he was a Junior Fellow, in the laboratory of Alex Forbes, and was aware of the potential for good behavioral techniques in medical research. Some years later when he was in Minnesota, he and Heron performed experiments on amphetamine that