John J. Boren (Editor, 1961–1963)

HOW IT WAS BEING SECOND

Charlie Ferster, as the first editor of JEAB, had set a lot of the tone for the journal. One of his major ideas was that authors, rather than editors, were accountable for their papers (including their errors, inadequacies, and wrong-headed interpretations). He maintained that publishing a few mediocre papers would cause little harm; after all, paper was cheap and only the author would be embarrassed by publishing unreplicable findings. However, excessive editorial criticism could seriously damage an author's "delicate writing behavior" (his phrase). The opposing point of view (from more than one member of my editorial board) was that we should not be publishing "second-rate" research and that we needed to keep the standards up. Exactly what I said at the time I am not sure (because I don't trust my memory). I think my editorial behavior probably vacillated somewhere in the middle. However, if I had it to do all over again, I would lean in the direction (Charlie's direction) of being more accepting and letting the author take most

of the responsibility. These days, when someone tells me that we don't have enough "critical thinking" and that we really need to "uphold the standards," I focus my eyes on the ceiling.

I regret that my generally accepting editorial stance was probably not carried out sufficiently in practice. I can recall a SEAB board meeting some years after my editorship when I rose to complain about the excessive rejection rate of JEAB. At this point someone (I think it was Bill Morse) pointed out that the rejection rate during my third year as Editor was not very far from the one I was complaining about. I was put down. Nevertheless, I still believe that a major function of an editor is to protect authors from the unreasonable criticism of reviewers, and I still oppose high rejection rates. I do recognize the contemporary corrupting influence of grant maintenance and of institutional "values" that provide promotion, tenure, and "merit" salary increases on the basis of number of publications in the past 12 months. People are virtually coerced to publish whether they are ready or not. In response to the system and the flood of paper, editors of refereed journals have often become gate keepers.

What I think I did, as Editor, was to write careful reviews (probably too careful) with the intent of helping the author make a more readable and more scientifically accurate paper. I tried to make specific suggestions rather than to say, "Rework the introduction and cut the discussion in half." I did *not* threaten a punishment contingency such that if the author did not make the suggested revision his article would be rejected. In general I printed whatever the author sent back. As a principle, I leaned toward larger experiments and more extensive work (the extreme case was Jack Findley's monograph, published in 1962), and I tended to send back "little" papers with the suggestion that more work be done. However, given a sufficiently large experiment, a sound method, and reasonably reliable data, then the paper was accepted. I paid little attention to the introduction. In particular I had only a

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little interest in the author's intentions and almost no interest in his/her guesses—better known as hypotheses. I knew that many hypotheses were concocted as the paper was being written. Even today somebody “educates” graduate students that they *have* to test hypotheses, so they dream up a set of guesses for the introduction to their dissertations. When I tell them that it is o.k. just to state what they are trying to find out, they look confused and wonder whether I understand “science.” As an editor, I found the “little” experiment whose main reason for being was to test, for example, the third corollary of Hull's second postulate or to resolve Smith's controversy with Jones, very dull. If the experiment could not stand on its own, I was likely to return the manuscript to the author.

After my election, while I was contemplating the problems of taking over the journal's affairs, Charlie Ferster told me that I could probably do the job in one afternoon a week. Three years later I was still trying to figure out what he meant by that. Besides the up-front job of dealing with manuscripts, I found myself the head of a small publishing business. Charlie and Marilyn had started JEAB on a shoestring, to their credit. However, by 1961 there were all sorts of unattended problems that kept popping up. For example, no one had applied for tax-exempt status or ever filed a tax return, so I had to work with a Washington lawyer to make us legal. I found I needed to review the typesetting (which fonts, hot or cold type?), the printing (letterpress or offset?), and other aspects of putting out a magazine. Then it turned out that we simply did not have enough personnel to put out a publication the size of JEAB, so I had to find some skilled people without spending much money. Undoubtedly my best long-term contribution to JEAB was convincing Kay Dinsmoor that she should be the full-time Business Manager of JEAB. Editors have come and gone, but Kay has been the backbone of JEAB since 1961. I also engaged Garth Hopkins (the former editor of a small-town newspaper and my next-door neighbor) to be the first Managing Editor, and we spent several bottles of Scotch together, going over the redaction of the first batches of manuscripts (that is when I learned

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what “redaction” meant). Finally, I discovered that we were printing JEAB on paper that would eventually turn brown and crumble, so I selected a permanent paper. I have never liked its appearance, but librarians tell me the pages will be preserved for posterity. Before my term was over, I had learned about the printing business—and actually it was fun, though time-consuming.

At the time JEAB was started, only a small group of researchers was involved. Indeed, the members of the first editorial board were authors of most of the first papers. However, by the time I became Editor, the field was developing rapidly, and the number of people interested in operant conditioning was growing larger. At a convention when a paper session concerned operant research, the audience would overflow the room into the hallway. Many more manuscripts were being submitted to JEAB, and the number of pages printed was increasing every volume. There was a sense of excitement, and a number of us believed that we were part of the beginning of a major development in behavioral science. I still have that belief. Those were indeed exciting times.

*National Institute on Drug Abuse
Rockville, Maryland 20852*