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

opportunity had come to me only after refusals to stand for the position by at least half a dozen others.

An election was held at the next meeting of SEAB. Vic Laties was the other candidate. Not knowing that Vic had expressed particular interest in the publication aspects of JEAB rather than in the editorial side of the journal, I didn't think I had a chance. Up to that time, both jobs were handled by a single Executive Editor and both were becoming bigger and more time-consuming. After I learned I had been elected as the next JEAB Editor, Peter Dews described Vic's interests and suggested that the tasks could be divided. I then made what was undoubtedly the single most important decision of my editorial tenure and appointed Vic Laties as Executive Editor. I retained responsibility for editorial decisions, and Vic took on the multitudinous problems of journal publication. I have never had cause to regret the decision: Vic's continued dedication to JEAB and later also to JABA has been essential to the continued vitality of both journals, and we are all in his debt.

Soon I was receiving packages of past editorial materials from Nate Azrin, and the editorial work began: first the selection of Associate Editors and members of the Board of Editors; then the actual handling of new submissions. I assigned some manuscripts to Associate Editors and kept others to handle myself. The editorial practices that had evolved during the early volumes of JEAB had been and continued to be passed on informally, mainly through the examples presented by the treatment of particular manuscripts. Editorial correspondence was circulated among the Associate Editors in a round-robin mailing, and comments were added to editorial letters and to reviews. To the extent that editorial decisions were governed by rules, they were rules that were generated incidentally through such interactions and during telephone consultations or discussions at professional meetings. We did not regard particular decisions as precedent setters; any decision might later turn out to be a mistake that should not be repeated.

Some aspects of the editorial process were usually easy to deal with. For example, the contingencies involved in asking an author to fix a figure that would not reproduce well in the journal were unambiguous. But the most important dimensions of a manuscript, such as whether the findings it presented were in-

teresting or significant, resisted quantification or explicit definition and quite rightly remained a matter of judgment.

The power of the Editor is based upon one simple contingency: Only what the Editor accepts for publication appears in the journal. The power can be attenuated in various ways (e.g., as when the Editor allows Associate Editors to make binding decisions on manuscripts, the usual custom with JEAB), but the most important constraint is that the Editor's task is mainly one of selection: The Editor cannot accept a manuscript for publication unless it has been submitted. The Editor's selection manifests itself in future journal issues, and these may in turn affect later submissions, but such contingencies involve time spans that are large relative to editorial terms.

For example, if a JEAB Editor decided to publish some papers on psychoanalysis, the psychoanalytic community would probably not recognize JEAB as a possible publication outlet for some time. I have often wondered about one manuscript purportedly submitted to JEAB by a psychoanalyst during my editorial term. According to the correspondence, the author thought he was submitting his paper to the *Journal of Experimental Anal Behavior*. I decided not to send the manuscript out for review and wrote a gentle letter of rejection in which I told the author that in JEAB his paper would probably not find the audience it deserved. But now I wonder. Was the psychoanalytic community closed off from JEAB prematurely? Or is there a colleague still snickering out there somewhere, having submitted the article as a practical joke without conceiving of the possibility that the submission would be treated seriously?

Occasionally an Editor can take an initiating role. Book reviews provide an example, but there is no need to recapitulate here the features of JEAB Book Reviews; they have already been the topic of two JEAB editorials (Catania, 1976, 1984). Another case arose because an editor can also make decisions about journal style. Only long after I had been an undergraduate at Columbia did I recognize that Keller and Schoenfeld had consistently spoken of "reinforcing responses" rather than "reinforcing organisms"; the two usages were mixed in Skinner's writings. As a graduate student at Harvard, I had participated in discussions with Dick Herrnstein, Lew Gollub, George Reynolds, and others about the im-

A. Charles Catania, 1968.

plications of the alternative usages. One argument was that a grammar of reinforcing responses rather than organisms made it unlikely that the response could be omitted in descriptions of contingencies. As JEAB Editor, I saw the opportunity to do something about this usage, and the grammar of reinforcing responses rather than organisms became standard (Catania, 1969); the standard was also extended to other procedural verbs, such as *punish* and *shape*.

Some initial resistance was encountered, but it diminished rapidly, especially as it became clear that the organism need not be omitted if the behavior is described in the possessive (e.g., reinforcing the pigeon's response). But some years after my editorial term had ended, Vic Laties wrote to me enclosing a manuscript submitted to JEAB by B. F. Skinner. There in two or three sentences was the now-prohibited usage: The rat was reinforced. What were we to do now?

In a letter to Vic that also dealt with other matters, I embedded a suggested letter to Skinner that outlined the logic of our editorial usage and argued that it would be difficult to maintain our usage once it was seen that he was violating it in the pages of JEAB. I also included reworked versions of the offending sentences in which the rat's response rather than the rat was reinforced. Vic simply circled my suggestions and sent my entire letter on to Skinner. But I need not have worried. Skinner accepted the changes and a proper version of his manuscript subsequently appeared in

JEAB (Skinner, 1975). When Vic and I asked him about the usage at a meeting some time afterward, Skinner's response was simply that one has to keep up with the times. We are pleased to note that he has since come to find the usage useful, and has adhered to it with reasonable consistency in his own recent writings.

There are other stories to be told. Some of them may one day be recovered from the JEAB editorial correspondence, much of which has been stored with the Archives of the History of American Psychology. For example, given the widely circulated myth that behavior analysis was involved in a conspiracy to suppress the bait-shyness research of John Garcia, it should be of interest that the JEAB editorial files will show that the initiative was instead Garcia's: He withdrew one of his bait-shyness papers from JEAB even though it had been accepted with enthusiastic reviews and conditional on only minor revision. Other aspects of the JEAB history are more difficult to document. For example, I owe substantial debts for innumerable sessions of advice and encouragement, especially to Kay Dinsmoor, Vic Laties, and Nate Azrin, but also to many others I consulted about journal matters at various times. The list is long, but most of the names have appeared on the JEAB masthead at one time or another. I therefore welcome the opportunity to record those debts here. But the most important history is still the one most readily accessible: our progress in behavior analysis as documented in the published volumes of JEAB.

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