REMINISCENCES OF JEAB

The following pieces, with the exception of the first, have been written especially for this anniversary issue by some of those involved with this journal over its first 30 years. The contribution by B. F. Skinner is reprinted from *The Shaping of a Behaviorist* (New York: Knopf, 1979, pp. 330-331) and *A Matter of Consequences* (New York: Knopf, 1983, p. 138).

B. F. Skinner

ANTECEDENTS

With the war at an end, research along the lines of *The Behavior of Organisms* was being done again. Some of Hull's students were active, but the centers were still at Columbia, where the rat was the principal subject, and Indiana, where the pigeon was king. We had trouble getting our reports published in the regular journals. We used very small numbers of subjects, we did not "design our experiments" with matched groups, our cumulative records did not look like learning curves, and we were asking questions (for example, about schedules) that were not found in the "literature." At meetings our papers were mixed in with others we seldom wanted to hear.

As a temporary solution, Fred [Keller], Nat Schoenfeld, and I organized a conference. Indiana University made some dormitory space available and paid Fred's and Nat's expenses. The meetings were informal. We simply took up different topics and spoke from the floor when we had something to say. We called it a conference on the "experimental analysis of behavior," taking the "experimental analysis" from the subtitle of *The Behavior of Organisms*.

It was not a wholly satisfactory name for a field. What should we call ourselves? "Students of behavior"? "Behavior analysts"? And what adjectives could we use to identify our research, our theories, or our organization? "Behaviorism," "behaviorists," and "behavioristic" were not quite right. They were too closely tied to John B. Watson. To most people "behaviorism" mean a denial of genetic dif-

B. F. Skinner, c. 1979 (courtesy of R. F. Gerbrands).

ferences. Watson's famous challenge ("Give me a dozen healthy infants, well-formed, and my own specified world to bring them up in, and I'll guarantee to take any one at random and train him to become any type of specialist I might select") had attracted far more attention than his immediate disclaimer ("I am going beyond my facts and I admit it, but so have the advocates of the contrary and they have been doing it for many thousands of years"). It was a connotation to be avoided, and in an interview I said that although as a psychologist I was concerned with behavior, "that did not of necessity make me a behaviorist."

Operant conditioners were finding it hard to publish their papers. The editors of the standard journals, accustomed to a different kind of research, wanted detailed descriptions of apparatus and procedures which were not needed by informed readers. They were uneasy about the small number of subjects and about cumulative records. (The cumulative record was, in fact, attacked as a "subtle curve-smoothing technique" which concealed differ-

ences between cases and should be abandoned in favor of "objective statistics.") At our Conference in 1948 we considered several solutions. Nothing was done until the spring of 1957, when the *Journal of the Experimental Analysis of Behavior* was planned and a Society for the Experimental Analysis of Behavior incorporated as publisher. Five of the first board members were students of Keller and Schoenfeld; seven were mine. Charlie Ferster, who had left the Yerkes Laboratories and was studying the behavior of autistic children at the Indiana University Medical Center, was appointed editor, and the first issue went to its 333 subscribers in 1958. Fred had been elected President of the Eastern Psychological Association, and his Presidential Address, "The Phantom Plateau," a delightful assessment of the "learning curves" which had appeared in textbooks of psychology for nearly half a century, was the first paper.

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REMINISCENCES ALREADY?

In 1955, I was one of the six operant conditioners to earn a PhD at Harvard, along with Douglas Anger, James Anliker, Donald Blough, Alfredo Lagmay, and William Morse, out of the seven psychology PhDs given at Harvard that year. The six of us thought of ourselves, more or less, as B. F. Skinner's students, but Skinner, who had been a professor at Harvard since 1948, could, prior to 1955, lay claim only to Ruth Page Edwards, Edward Green, George Heise, and Herbert Jenkins as "his" Harvard doctor's degrees. In 1956 and 1957, Harvard's list of operant conditioners continued to grow rapidly, adding Nathan Azrin, Ogden Lindsley, Thomas Lohr, and Merle Moskowitz, and continued to do so until, in the mid-1970s, the cognitive school took first place among experimentalists at Harvard, as elsewhere.

The Zeitgeist evidently had its eye on operant conditioning for a decade or two, but the

surge at Harvard and the founding of JEAB in those few years in the mid-1950s seem to me to have had a more obvious element in common. Johnny-on-the-spot for both events was Charles Ferster. He had migrated from Columbia University to Harvard as Skinner's "research associate" in 1950, Harvard's designation for a soft-money, postdoctoral research appointment made at the convenience of a member of the regular faculty.

Ferster's job was to run Skinner's operant laboratory, which he more than did. He not only ran it, he refashioned it. He rebuilt and greatly enlarged the "pigeon lab." He was an indefatigable, enthusiastic researcher, an 80-hour-a-week-man, and an unselfish, natural leader for graduate students eager to dig into a subject. Not all the graduate students were as close personally to Ferster, nor as influenced by him scientifically, as, for example, Morse and I were, but he surely contributed greatly