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 differences 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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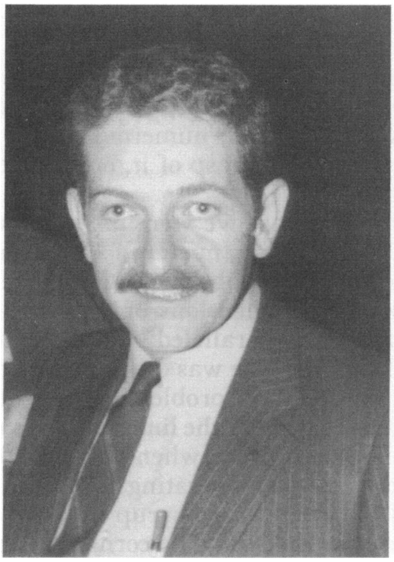
REMINISCENCES ALREADY?

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



R. J. Herrnstein, 1968.

to the productivity and ambience of Harvard's operant laboratory.

Ferster set and enforced exacting technical standards for research, which remained Harvard's standard even after Ferster left in 1955 for the Yerkes Laboratories. In addition to the usual demands for experimental regularity and discipline, Ferster insisted on uncommonly precise physical specifications of response and of the reinforcement contingency (but, curiously, not of the stimuli). The design of the pigeon key and of the relay circuits that ran our experiments received special attention. The goals were to define response topography sharply, to minimize the delay between response and reinforcement, and to open up the possibilities for varying the contingency of reinforcement, as exemplified by new schedules of reinforcement.

It was the impression among the Harvards, as I recall it, that we were at the technological forefront in operant research, for which we had mainly Ferster to thank. I still believe that the increment in precision paid off in data so orderly that, before long, we found functional relationships on a par in reproducibility and generality with those of psychophysics, then psychology's most advanced field. Like psychophysicists, we could get away with two or three or, at most, four subjects in mapping out a function.

This essay is a reminiscence, not a history, a distinction I interpret as allowing me to take few pains to substantiate my recollections. Let the reader beware that most of what he or she finds here is raw memory, unleavened by real data. What follows are further reminiscences of Ferster, plus of those other members of the first JEAB editorial board whom I got to know as a graduate student at Harvard from 1952 through 1955.

At some point, I remember Ferster storming into the office I shared with Morse, Blough, Anliker, and Azrin, waving a rejection letter from the *Journal of Comparative and Physiological Psychology,* probably from Harry Harlow, its editor. JCPP wanted statistical tests, but, said Charlie, the behavior under the various experimental conditions did not even overlap, or words to that effect. This indignation over, first, the demand for pointless inferential statistics and, second, the heavy-handedness of a journal editor was, from my vantage point, the seed that grew into JEAB.

At the time Ferster waved his letter, I may or may not have heard about the mimeographed proceedings of Conferences on the Experimental Analysis of Behavior, mentioned by other reminiscencers. I certainly heard of CEAB at some point in the founding of JEAB, but it seemed to me that Ferster meant business in a way that the earlier organizers did not. Ferster was going to start a

1955 Harvard group. T. Lohr and N. H. Azrin (front row); R. J. Herrnstein, W. H. Morse, O. R. Lindsley, B. F. Skinner, C. B. Ferster, and A. Lagmay (back row).

real *journal,* on the model of the APA experimental journals, minus their flaws; perhaps indignation is a better motive for starting a journal than camaraderie.

I may be making Ferster too much the hero of the story. From his point of view, as well as from everybody else's at the time, Skinner was, of course, the central figure. We were mostly just plowing the field he laid out. Ferster, in particular, was keenly dedicated to Skinnerianism. The involvement transcended the merely academic. With good humor masking serious intent, Ferster emulated Skinner in personal ways as well. He copied Skinner's disciplined work habits; he adopted Skinner's techniques for writing; he aped Skinner's gestures; he used the same barber in Harvard Square; he even wore the same odd brand of shoes. None of the other operant conditioners at Harvard were as dedicated as Ferster, but it was all done with a hint of tongue in cheek.

B. F. Skinner was an amiable but remote figure. I do not remember his demanding attention, agreement, or obeisance, the way Freud or others founders of "schools" are depicted. Quite the contrary. Skinner budgeted his time carefully, leaving little room in the budget for graduate students. We all have our Skinner stories, and this one may not be representative, but I recall an appointment with him. As I entered his office, he set a minute-minder for five or ten minutes. Whichever it was, he talked about a paper he was writing until the timer chimed, at which point the interview ended. Similarly, it took what seemed to Morse and me an inordinately long time for him to get our two names sorted out—it was some time before "Dick Morse" and "Bill Herrnstein" extinguished. During 1954–1955, when the six of us were coming up for our PhDs, Skinner took academic leave, much of it spent at an inaccessible hideaway in, I believe, Vermont.

But for me, as for most of the others in the laboratory, these were small matters compared to the inspiration we found in his approach. The best course I ever took, in nineteen years of going to school, was the course he gave from an early draft manuscript of *Verbal Behavior.* In three densely packed lectures a week for a semester (permitting no questions or comments from the twenty or so attendees), he described a system of concepts that spanned the range from simple conditioning to the limits of epistemology. Thirty-three years later, I still admire the analysis deeply, though I now consider it flawed. But I have yet to meet or read a critic of this system from outside the behaviorist tradition, numerous as they are, who has any true grasp of it, or its defects as I see them.

The feeling that we were on to something special was further nurtured by encounters with Skinner in the laboratory. His insight into the behavioral dimensions of a psychological question was unparalleled, and often still is. In the laboratory, he was quick and ingenious in tackling technical problems. The ingenuity sometimes edged over the line from the sublime to the ridiculous. Once, when a resetting device in a recorder was operating too rapidly, he poured a bottle of corn syrup into the apparatus to slow it down. The corn syrup spoiled a few days later (the recorder, no doubt, ruined). He did not keep pace with the increasingly electronic automation of operant research—nor have I, for that matter, a generation later.

During one of our weekly, data-reading, "pigeon meetings," I remember suddenly thinking that, in his experimental method, Skinner had invented the psychological equivalent of the microscope, exposing behavior in a detail invisible to the "naked eye." The behavioral microscope would do for psychology what the optical microscope had done for biology, I thought with excitement. I have now seen this analogy in print someplace, but at the moment I am describing, it was novel. That may have been the moment of my conversion. Skinner, in my view, is the premier behaviorist, not for his theory of behavior, but for having defined a productive and challenging behavioral level of analysis. I was fortunate to be around when Ferster put the behavioral level into high gear at Harvard and when he founded JEAB.

Among fellow graduate students, I was closest to William Morse. He arrived at Harvard when I did, but far more knowledgeable about behaviorism than I, having been well schooled in the subject by Starling Reid and Frank Finger at the University of Virginia. It was my good luck to be assigned the desk adjacent to his, and also my good luck that he was so patient with me while my knowledge of our subject caught up with my enthusiasm for arguing about it. Laconically, in his tidewater

Virginia style, he won most of the arguments, although it took me a while to realize it. We started collaborating on research in the pigeon lab in our first semester, under Ferster's watchful and sharp eye.

Morse and I were about equally helpless with power supplies, relays, stepping switches, rectifiers, solenoids, capacitors, etc. We learned by lots of trial and error, usually together in the laboratory until the small hours of the morning. I missed the night the large capacitor in our home-made, direct-current power supply exploded, having gone out to dinner with my visiting parents. Morse's face and eyeglasses were still smudged with soot when I saw him the next morning.

At first I, and perhaps also Morse, was having more fun learning to use the apparatus than by answering questions with our experiments. In time, behavior itself became far more fascinating. By our third and last year as graduate students, we were collaborating on several dozen experiments concurrently, keeping the laboratory going continuously. We had taken an oath with each other never to be deterred from doing an experiment by technical difficulties, and we tried hard to live up to it. We got to be fairly good with the design of switching circuits. The resulting freedom in molding new reinforcement environments spawned countless ideas for experiments.

Only a small fraction of the collaborative work with Morse was published, and perhaps only a small fraction deserved to be, but it whetted my appetite for behavioral data to this day. One of the strengths of the Skinnerian method is how easily it yields data, once the procedures are automated. Morse and I were fortunate to be among the first to mine this rich ore. We were clearly going to need a new journal for our kind of work, but that is the clarity of hindsight.

Once, I remember Morse telling me to observe our office-mate, Donald Blough, who, Morse said incredulously, can be seen, sometimes for minutes on end, "deep in thought." Surprising as this was, I soon confirmed the observation. Blough evidently planned his research in careful, elegant detail. Perhaps the rest of us would have spent more time thinking if it had paid off as well as it did for Blough. The rest of us were more likely to do an experiment wrong a few times, hoping to get it right later on.

That was not the only way Blough was atypical among the operant conditioners. His Swarthmore undergraduate training in psychology evidently imprinted him with an interest in the stimulus, particularly in visual stimuli. He was not much interested in operant conditioning per se, nor did he display the feverish excitement of the true believer, as the rest of us often did. Although Blough was not excited about schedules of reinforcement, his use of them in extracting psychophysical data from animals was and remains one of the triumphs of the Skinnerian method.

At the other extreme of true belief was another office-mate, Nathan Azrin. He arrived at Harvard well indoctrinated by, I recall, Ogden Lindsley, whom he had met while Lindsley, although a Harvard graduate student, was running an operant research project at Boston University, where Azrin was an undergraduate. Azrin had no inhibitions letting people know where he stood. He quickly established a reputation for himself in the Department by, it was said, asking the intimidating S. S. Stevens, during a meeting of the psychological measurement course Stevens taught, whether he really believed all that nonsense about body build and personality. I do not recall hearing about Stevens's response, only my surprise that Azrin survived.

Azrin took a position on the far side of behaviorism, compared to Morse and me. In one discussion, Azrin was insisting that he knew when to eat or go to the john by looking at a clock, rather than have to confess to internal states or stimuli. As I recall, it was later in this conversation that Morse punched Azrin, not too hard and not in anger, while saying that he was glad that Azrin, lacking internal states, would not be feeling anything like pain. My apologies to Bill and Nate if, despite my vivid recollection, I have invented this tale. I am sure, at least, that it catches the spirit of our arguments.

The doctrinaire side of Azrin vanished in the laboratory, where he was a relentlessly open-minded empiricist. Theory yielded when it confronted contrary evidence. For example, his doctoral research rewrote the Skinnerian canon on punishment, by refuting the suggestion that it was fundamentally less effective than reinforcement in controlling behavior. His interest in punishment was more practical than theoretical. Azrin was pursuing a practical agenda from the start. He had, I believe, specialized in clinical psychology as an undergraduate. Coming to an experimental department like Harvard's at that time did not mean that he had changed his mind (*pace* Nate) about applying psychology, only that he believed that operant methods worked better.

Ogden Lindsley, too, was interested in practical applications of operant conditioning. After his stint running animals at Boston University, he, with the help of Skinner, got the chance to set up and direct an operant laboratory for human subjects at a local state hospital for the insane. His subjects were seriously ill, long-term schizophrenics and other psychotics. Lindsley's strong, almost domineering personality did not come through in his research, which was conservative, as was appropriate to the situation. It was a rare opportunity to work with institutionalized human subjects, and Lindsley was careful to do nothing to jeopardize it. Involved as he was outside the Department, he became something of an outsider to the group working in Skinner's laboratory, and his research, also, was usually not ideally suited for JEAB. JABA, in due course, was the natural habitat for Lindsley's work, and all that followed it.

James Anliker was something of an outsider as well, but not because of any lack of proximity. This office-mate of ours was hardly an operant conditioner at all, except in the narrow sense of using some sort of lever-pressing apparatus. A former student of Klüver's at the University of Chicago, and an ex-instructor of anatomy at a small medical college, Anliker had brought two rhesus monkeys with him to Harvard, to use as subjects in behavioral experiments. I learned about dominance orders the hard way from the monkeys, Ike and Alfred. I agreed to feed them when Anliker went home for Christmas one year. I found myself number two, just ahead of Alfred, but well behind Ike, who would alight on my head when I entered the cage, and sit there grumbling at me and boxing my ears if he disapproved of what I was doing.

Despite an interest in primate behavior, a taste for psycho-philosophical argument (which I shared, to the dismay of our office-mates), and a flair for creative scientific instrumentation (which he drew on in a brief collaboration with Jean Mayer on eating behavior in rodents), Anliker's true love was not psychology at all, but music. A fine organist and accomplished choir director, he soon found work at local churches. He is the one person on my list who, as far as I know, drifted out of psychology altogether.

Douglas Anger had his own laboratory down the hall from Skinner's, supported by Harvard's Society of Fellows. Anger was the third psychologist to be a "Junior Fellow" since this distinguished program of university-wide fellowships began in 1931, preceded only by James Greer Miller and Skinner himself. That seemed about right to me. Anger was, like Miller and Skinner, truly exceptional. His research in the early 1950s was much ahead of its time. Anger was looking for quantitative laws of reinforcement when the rest of us were still happier than we should have been with qualitative, phenomenological observations of behavior. There was little real quantification in Skinner's laboratory at that time, and little genuine effort to discover functional relations, despite the rhetoric to the contrary. In time, of course, the functional relations came, but only in the wake of Anger's research. Anger also pushed the frontiers of automatic instrumentation in psychology at Harvard the furthest. At our best as circuit designers, Morse and I never doubted that Anger was ahead of us.

Peter Dews was at Harvard, too, but far across town, a junior member of the faculty of the Medical School. Distance did not daunt him, nor did much of anything else. I recall exciting visits, when Dews arrived at the laboratory literally with a bagful of drugs, which were injected into almost any plausible pigeon running in an experiment, to see how behavior changed. It sometimes changed a lot, as when we tried LSD-25 on a pigeon, whose behavior on a simple variable-interval schedule remained disrupted for weeks. We seriously underestimated that drug's potency, and its importance. For several months, I had, without a glimmering, probably enough LSD-25 in my desk drawer to send much of the city of Cambridge on a bad trip.

After some wild early sessions, the drug research rapidly settled down and started paying off. The first articles Morse and I published were mostly drug studies, with Dews behind the scene, providing the compounds and telling us what to look for in their behavioral effects. Dews soon set up his own behavior laboratory in the Pharmacology Department at the Medical School, just when many large pharmaceutical companies were discovering the importance of stable behavioral baselines in their search for useful drugs. That is why drug companies were the major early financial backers of JEAB.

Like Ferster, I left Harvard in mid-1955, when JEAB was still embryonic and unnamed, but in utero. I was drafted into the army, among the last to go under the old selective service act. I would not have been subject to the draft a month or two later, when the law was changed. It turned out well, for I landed in Joseph Brady's laboratory at Walter Reed, as Murray Sidman's research assistant for a time. Across the hall from the operant conditioners were Walle Nauta, Robert Galambos, David Hubel, John Mason, and others, an all-star line-up of physiologists and anatomists. From Harvard's pigeon laboratory to the laboratories that neurologist David Rioch had created at Walter Reed was a large and broadening step. But that is another story.

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COLUMBIA GEMS[1]

There were five one-time Columbia students on the Board of Editors of JEAB for 1958: Douglas Anger, John Boren, Charles Ferster, Murray Sidman, and Thom Verhave. I suppose that I could add Nat Schoenfeld to the group. He was a graduate student at Columbia and received his PhD there, but under what we might now call suspicious circumstances: He was given his doctorate because of a study of "stereotypes," a concept from another field than ours, before I had a chance to steer him to *The Behavior of Organisms.* He was never a pupil of mine and I'm not responsible for his later actions. He had a major part to play in the instruction of the other Columbians in my list, except for Douglas Anger, but I don't believe he should be blamed for all their faults.

Doug Anger, the Apparatus Editor on the Board, came out of Colgate University and was a pupil of mine in one course only: an introduction to behavior analysis in Columbia's School of General Studies. He was a rather frightening student, intense, and with a powerful voice. He used to walk me home from Pupin Hall to our Morningside Drive apartment after class was over, in order to pick my brains (I mean, explore my repertoire). Find-

Frances and Fred Keller, 1987.

[1] This is an abbreviated version of a talk given at the May 1987 meeting of the Association for Behavior Analysis.