



Charles B. Ferster
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CHARLES B. FERSTER—A PERSONAL MEMOIR

B. F. SKINNER

For five-and-a-half years Charlie Ferster and I worked together on the research we reported in *Schedules of Reinforcement*. It was a near-perfect collaboration, undoubtedly the high point in my life as a behavioral scientist. Charlie published his account of it more than 10 years ago (Ferster, 1970). Mine, part of the concluding volume of my autobiography, is still, alas, in progress and hence will not be seen by him, as much as I have looked forward to his seeing it.

In the fall of 1949 (curiously, Charlie puts it 1950) it was clear that I could take on a research associate, and I turned to Fred Keller

for nominations. He suggested Charlie, who came up for an interview, and we immediately came to terms. He would start on February 1, 1950, although he would not get his Ph.D. from Columbia until the following June.

A year later I wrote to Fred:

Our research goes on apace. Charlie Ferster has worked out wonderfully. In his quiet way he has vastly improved the basic design of most of our equipment. He has also worked out our manpower problems so that the actual running of experiments is now done by unskilled help, etc. I have found it very profitable to give him a full share in the design of experi-

ments, too. We're starting to work up material for a long monograph on intermittent reinforcement—a subject I'd like to have done with.

One piece of that equipment was a better cumulative recorder. I was still using a simple scheme from Project Pigeon in which a pen was drawn across a strip of paper on a taut string. Charlie and I designed a more substantial model, using standard parts so far as possible—a typewriter platen, for example, to drive the paper. Several of these were made, but improvements were obviously possible, and together we helped Ralph Gerbrands design the first cumulative recorder in the modern style.

Our laboratory worked around the clock, pigeons in pie-shaped divisions of a large drum being moved into place by a clock and motor. Each morning we went through the laboratory on what we called our Grand Rounds, looking at cumulative records and making changes in the schedules. Almost every day there were surprises. Multiple schedules were one of them; and eventually some of our pigeons gave appropriate performances under stimulus control on as many as nine different schedules. Mixed schedules were another. We guessed wrong on the outcome of a limited hold, but the result was nevertheless exciting. We added a clock or timer by projecting a line on the key which changed in length with the time or count since the last reinforcement—and again with surprising, dramatic results.

I wrote again to Fred Keller:

If we have not always agreed about what should be done next or in anticipating the outcome of an experiment, we have always managed to work very efficiently while respecting differences of opinion. . . . He has given very generously of his time to our graduate students and is undoubtedly responsible for much of the technical competence of current research in this laboratory. His influence in this respect has also been felt in several neighboring institutions.

Part of the time we worked as Baconians. On large sheets of paper we drew up tables showing schedules programmed by clocks or counters, with the ratios or numbers fixed or variable, with two or more schedules in effect simultaneously or in succession, correlated or uncorrelated with colors on the keys, and so

on. We strove for a fairly systematic coverage. If we added *drh* to a variable-interval schedule with good results, it was time to add it to a fixed-interval, and so on.

At other times we were Galileans; we had a theory. Unless our pigeons had extrasensory perception (a possibility we dismissed), their only contact with the programming equipment came at the moment of reinforcement. But a number of stimuli could be acting at that moment, corresponding to readings on a speedometer (the bird was responding at a given rate), a clock (a certain amount of time had passed since the last reinforcement), and a counter (a given number of responses had been made since the last reinforcement). We designed our experiments to give these stimuli a chance to make their presence known.

It is always difficult for the younger member of a collaboration to get a proper share of the credit. When I reported a good deal of our joint research at the International Congress in Stockholm in 1951, I said that "Dr. Charles B. Ferster had served as principal investigator." Nevertheless, I gave the paper and was sole author when it was published and, of course, I got all the credit. When, on the other hand, Charlie submitted a paper in 1952 called "The Use of the Free Operant in the Analysis of Behavior," the editor asked for additional information: "In several instances you refer to procedures developed in the Harvard laboratory. Were these procedures developed solely by you or were they shared?"

Hoping to correct for this, I planned to surprise Charlie by making a last-minute switch in the authorship of *Schedules of Reinforcement*. Only when he received his first copy would he know that the book was "by Ferster and Skinner." But Charlie was having trouble finding a job, and was worried, and I spoiled the surprise by telling him in advance that I wanted him to be senior author. Only then would his share in our work be properly recognized.

Although we gave a few papers at meetings (and set up a demonstration at the meeting of the National Academy of Science in Washington in which a pigeon displayed a three-ply multiple performance), too many things were happening in the laboratory to give us time to write reports, and filing cases grew heavy with unanalyzed cumulative records. That

monograph on intermittent reinforcement remained unwritten. In 1954, however, we changed from controlling the behavior of pigeons to controlling our own behavior. We set up a room containing all the equipment we thought we needed—an old wax cylinder dictating machine, a moist-paper duplicator (soon discarded), and a drawing board equipped for making figures from our records. Charlie's term at Harvard was growing short, and there would be no time for a theoretical analysis. The best we could do would be a kind of atlas. We made more than a thousand figures and wrote the text for each of them. Marilyn Ferster reviewed the manuscript, and then, in one last spurt during the very hot summer of 1955, Charlie and I set ourselves a schedule: Each day we put the finishing touches on the text for 50 figures, finishing in mid-afternoon if possible but going on until midnight if necessary. In less than a month we had a manuscript. It would be expensive to publish, but two of the drug companies with operant laboratories gave the publisher subsidies which kept the price within reason (Ferster & Skinner, 1957).

There was a possibility that Charlie would go on to work with Walter Rosenblith at M.I.T., making reinforcement contingent in various ways upon efferent nerve impulses. Instead he went to the Yerkes Laboratories in Florida, where, unfortunately, he found the atmosphere uncongenial. (Tender-hearted colleagues frustrated his efforts to reduce chimpanzees to a satisfactory state of deprivation.) At Indiana University Medical School he did pioneering work with autistic children, and became the first Editor of the *Journal of the Experimental Analysis of Behavior*. He moved to Washington as the second Director of the Institute for Behavioral Research. He then went to Georgetown University, and finally to the American University. Few people have contributed as much to the experimental analysis of behavior.

REFERENCES

- Ferster, C. B. "Schedules of reinforcement with Skinner" in *Festschrift for B. F. Skinner*. P. B. Dews (Ed.). New York: Appleton-Century-Crofts, 1970.
- Ferster, C. B., & Skinner, B. F. *Schedules of Reinforcement*. New York: Appleton-Century-Crofts, 1957.