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THE HARVARD PIGEON LAB UNDER HERRNSTEIN

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The history of the Harvard Pigeon Lab is a history of two periods of remarkable productivity, the first under Skinner's leadership and the second under Herrnstein's. In each period, graduate students flocked to the leader and then began stimulating one another. Chance favored Herrnstein's leadership, too, because an unusually large number of graduate students were admitted in the fall of 1962. In each period, productivity declined as the leader lost interest in the laboratory and withdrew. Directly and indirectly, the laboratory finally died as a result of the cognitive "revolution." Skinner and his students saw the possibility of a natural science of behavior and set about establishing that science based on concepts such as response rate, stimulus control, and schedules of reinforcement. Herrnstein and his students saw that the science could be quantitative and set about making it so, with relative response rate, the matching law, and the psychophysics of choice (analogous to S. S. Stevens' psychophysics). The history might provide a golden research opportunity for someone interested in the impact of such self-organizing research groups on the progress of science.

Key words: R. J. Herrnstein, B. F. Skinner, Harvard Pigeon Lab, Society for Quantitative Analyses of Behavior

The history of the Harvard Pigeon Lab may be divided into two periods: the lab under Skinner and the lab under Herrnstein. Two waves or generations of students were produced. The first period began with Skinner's move to Harvard in 1948, when he started the lab and began building it up. Several people besides Skinner were doing experiments, but few were actually Skinner's graduate students until after Charlie Ferster arrived as a postdoc from Columbia in February 1950 (Skinner, 1981). According to Herrnstein (1987), Ferster was largely responsible for organizing and enlarging the lab, thereby making it attractive to incoming graduate students. Herrnstein himself arrived as a graduate student in the fall of 1952.

Table 1 shows a list of graduate students, divided according to whether they studied primarily under Skinner or under Herrnstein. It may be incomplete, but most experimental behavior analysts today will recognize many names on the list, both from the generation under Skinner and the one under Herrnstein. The order is roughly chronological. Skinner's students are divided into those who arrived before Ferster and those who arrived after. Strictly speaking, Peter Dews should have been left off, because he was never Skinner's student officially, but he participated in the weekly meetings—the pigeon staff meetings—regularly and behaved as one of the group. That the number of students post-Ferster exceeds the number pre-Ferster illustrates that the numbers built up over the period. The peak under Skinner occurred in the mid-1950s.

Herrnstein's Beginnings

Skinner stopped doing research in the lab after he and Ferster completed the studies that went into *Schedules of Reinforcement* in 1953 or 1954 (Ferster, 1970; Ferster & Skinner, 1957; Skinner, 1984). He continued to take an active interest, interacting with the students about their research, for a few more years. Herrnstein left Harvard in 1955 for a stint in the Army, which was well spent at the Walter Reed laboratories in Washington, D.C. He returned in 1958 as an assistant professor.

The university regarded assistant professorships as temporary positions. One would typically be appointed for a period of 3 to 5 years, possibly followed by a lectureship for another year or two, and then one was expected to move on. Tenured faculty were almost always hired from other institutions. Beyond expectation, Herrnstein was promoted to associate professor with tenure after 3 years. Probably the reasons were that he had been productive and that he had taken over responsibility for the Pigeon Lab. I am unsure whether his discovery of the matching law figured in, because its publication coincided with his promotion (Herrnstein, 1961). Iron-

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Table 1

Harvard Pigeon Lab, 1950–1980

Skinner	Transitional	Herrnstein
Pre-Ferster	Herb Terrace	Lois Hammer
Bill McGill	Neil Peterson	Ed Fantino
Ruth Edwards	Peter van Sommers	John Staddon
Edward Green	Sandy Autor	Bill Krossner
George Heise		Laurel Furumoto
Herb Jenkins		Billy Baum
Doug Anger		Howie Rachlin
Post-Ferster		Shin-Ho Chung
Og Lindsley		Richie Schuster
Thomas Lohr		Al Neuringer
Merle Moscowitz		Phil Hineline
Fred Lagmay		Bob Boakes
Nate Azrin		Arturo Bouzas
Bill Morse		Bruce Schneider
Peter Dews (medical school)		Rhoda Kessler
Don Blough		Gerry Zuriff
Dick Herrnstein		Peter Killeen
Lew Gollub		Fran McSweeney
Harlan Lane		Phil Sagan
Charlie Catania		Hal Miller
George Reynolds		Ben Williams
		John Schneider
		Paul Bogrow
		John Cerella
		Gene Heyman
		Will Vaughan
		Peter de Villiers
		Jim Mazur
		Lexa Logue
		Mark Snyderman

ically, S. S. (''Smitty'') Stevens, Dick's first mentor, probably appreciated the matching law more than Skinner did, judging from Skinner's later comments (Skinner, 1976, 1986). Smitty and Dick continued to have a warm relationship, even though Smitty was hurt when Dick had moved to the Pigeon Lab in his 1st year of graduate study (Baum, 1994). Through Dick, Smitty's quantitative psychophysics, particularly his psychophysical power law (Stevens, 1975), was to have a profound effect on the course of research in the Pigeon Lab. Dick once remarked to me that he thought the matching law was like the psychophysics of choice. When I proposed what came to be called the generalized matching law, which was a power law just like the psychophysical power law, I too was showing the influence of Stevens' psychophysics (Baum, 1974).

Herrnstein as Mentor

In Table 1, I have listed some students as ''transitional,'' because they worked with Skinner but were probably influenced by Herrnstein too. Remarkably, there were only about four such students, probably because Skinner's loss of interest in the lab left a vacuum that Herrnstein soon filled. I left off the list Paul Rozin and Jerry Hogan, because they declined to be associated with either Skinner or Herrnstein, they did their research outside the Pigeon Lab, and, aligning themselves with ethology, considered themselves a sort of opposition group to the operant conditioners. One could argue about Neil Peterson, but both Skinner and Herrnstein claimed him. Herb Terrace might regard himself as Skinner's student, but his stay coincided mostly with Dick's reign. If he were counted as Skinner's, that would reduce the transitional category to just three students.

My first exposure to Herrnstein occurred when I was an undergraduate. I had taken Skinner's course, Natural Sciences 114, as a freshman in the spring of 1958. In the fall, I took Psychology of Learning from Herrnstein, and in one of our meetings during his office hours Dick persuaded me to switch my concentration from biology to psychology. The following year, I started on my honors thesis. Dick suggested that I build a three-lever rat chamber (two choice levers, each with its own feeder, and a trial-initiating lever on the back wall) that would serve as an automated analogue to a T-maze. I designed and built the chamber in the machine shop, first making the acquaintance of the machinists, Ralph Gerbrands and George Silkwood. Both men were friendly, but Ralph seemed more aloof, an impression that later, when I was a graduate student, I reassessed as shyness. George taught me how to use the drill press and sheet-metal bender. I realized only much later how remarkably well equipped the shop was. I also worked in a smaller shop adjacent to the Pigeon Lab, which was stocked with parts, soldering equipment, and a drill press; Charlie Ferster had organized this shop on his arrival (Ferster, 1970).

The Pigeon Lab was a beehive of activity. I could see people going in and out, and I could hear the relays clicking away, but as an undergraduate my experiments were carried out in an auxiliary room down the hall, next to the animal rooms, because my oversized chamber wouldn't fit in the main lab. The main lab was fitted up with vertical relay racks, but Dick gave me one of the older, horizontal setups (Ferster, 1970, Figure 1) to work with.

Herrnstein's treatment of students differed considerably from Skinner's. By all accounts, Skinner's style with students was extremely "hands off." Herrnstein (1987) described him as "amiable but remote," remarked that Skinner took a long time to learn his name and Bill Morse's, calling them "Dick Morse" and "Bill Herrnstein," and that in the year 1954–1955, "when 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" (p. 450). Of the same incident, Skinner (1984) wrote, commenting on the freedom he allowed his students,

> Unfortunately, freedom could look like neglect, and when I returned from Putney I found that I was seriously in default. Five of my graduate students had finished their work, written their theses, and come up for their oral examinations without my help. It was the largest number of graduate students ever to take their degrees under me in one year. Even so, the protest came mainly from the staff, who were pressed into reading theses and conducting examinations in my absence. (p. 88)

Ferster (1970), taking a more positive view, commented,

> I don't remember any experiment being called "great" or "bad" or anyone being given credit for doing something especially useful or valuable. Some experiments led to further planning, new apparatus, exciting conversations, new theoretical arrangements of data and procedures or a rush to tell everyone about them, while others enabled less behavior of this kind. I don't know whether Skinner was conscious of the lack of personal praise in interpersonal relations in the laboratory. I certainly was not. My behavior was generated by the natural reinforcement of the laboratory activity. But some of the graduate students found the absence of personal support difficult. (p. 43)

In contrast, I found Herrnstein to be supportive, ready to praise and ready to criticize. As the years of our contact continued, I came to think of him as my mentor. Yet, his experience with Skinner's "freedom" had some effects, even if they were subtle. When the day came for me to begin work on my undergraduate honors thesis, he took me into the auxiliary room, showed me the apparatus I was to use, showed me how relays operated, showed me how one could arrange for a relay to operate itself (a lock-up), and then told me to learn how to program and left. I think he gave me, or I obtained somehow, some mimeographed sheets explaining some basics, but I was left to struggle. Eventually I learned enough to run my experiment.

I too would describe my experience of Skinner as "amiable but remote." As an undergraduate I had little contact with him. The readers of my honors thesis were Dick Herrnstein and Smitty Stevens. At the end of the oral exam, Stevens told me I needed to learn to write and scribbled the title of *The Elements of Style* (Strunk & White, 1979) on a scrap of paper. Handing it to me, he said, "If you follow this little book, you will write better." Crusty though he was, Stevens was more help than Skinner. During the summer after my junior year, I had spent several weeks working in the psychophysics lab as a research assistant, running subjects (mostly graduate student ''volunteers'') in an experiment on ratio estimation and production and completing an experiment on power-function learning curves with J. C. Stevens and a graduate student, Harris Savin.

Like Skinner, Herrnstein still allowed the graduate students extraordinary freedom in the lab. We were permitted to do whatever experiments we wanted to do. I remember very little competition over apparatus or pigeons; there always seemed to be plenty. Dick had several experiments going, but beyond those, the lab belonged to the graduate students. Like Skinner, too, Dick served as an excellent model. He was enthusiastic about his research and would often grab whatever student was handy to tell him about his latest result or theoretical idea. He would also get into long debates with students about politics, particularly during the Vietnam War.

Later, when I had the assistant professorship, Dick commented to me that he thought my cohort had had too much freedom and proposed that we now require students to justify experiments before allowing them to run them. That seemed like common sense to me, particularly because we had progressed so far beyond the phenomenological level of doing experiments ''just to see.'' It meant, however, a further departure from Skinner's ''freedom.'' In retrospect, I think it could be tied also to the decline in importance of cumulative records. The cumulative recorder was the key to the phenomenological approach. One saw orderly changes minute by minute. Patterns such as ''scallops'' and ''knees'' could be reproduced by reproducing the local circumstances. By the time I was a graduate student, although every experiment had a cumulative recorder attached, the records were used primarily to detect apparatus failures. Our interest was in reading counters and timers.

Herrnstein's Graduate Students

As an undergraduate, I had no idea how the Pigeon Lab was supported or who was ''in charge.'' I was aware of some of the graduate students. Lew Gollub and Herb Terrace were there. Dale Brethower, who finished his degree elsewhere, was there. I think George Reynolds helped to run the self-instruction lab that was part of Natural Sciences 114. Sandy Autor was the teaching assistant. I met Jim Holland. My girlfriend was doing her honors thesis with Dews and Morse at the medical school, so I met them when I visited there. Charlie Catania taught the course I took in comparative psychology. The room where I was doing my research was next to another suite where Peter Van Sommers, Neil Peterson, Paul Rozin, and Jerry Hogan were doing experiments. Next down the same hallway was von Békésy's laboratory, but I only got to know him later when I was a graduate student and we both had the habit of pacing this same hallway while thinking.

The first graduate students who I would definitely call Herrnstein's were John Staddon and Ed Fantino. I say this because I'm unsure whether Bill Krossner or Lois Hammer, both of whom were present, started with Skinner. Fantino and Staddon were admitted in the fall of 1961, while I was taking a year off.

The department's policy, I learned later when I myself was on the graduate admissions committee, was to admit all applicants who looked smart and inclined to do research. Because most accepted, the policy resulted in fluctuation in numbers from year to year, depending on how many qualified people applied. In 1962, an unusually large number of qualified people applied, with the result that an unusually large number of graduate students arrived in the fall: at least 12, and possibly 13. Two or three dropped out, but most (10 by my count) survived the rigorous selection imposed by the department. In the first semester, we were subjected to a strenuous proseminar dominated by E. G. Boring and S. S. Stevens. Apart from the preliminary exams, which we had to take at the end of the 1st or 2nd year, the exams given at midsemester and at the end of that first semester were the largest hurdles we faced; it was a time of high anxiety. The spring semester was easy sailing by comparison. Skinner took 4 weeks. I remember finding him interesting, but feeling that I learned little. He spent a lot of time talking about ''seeing that you see,'' an idea about which I was skeptical.

The students who survived had three choices for research. At the east end of Memorial Hall was the laboratory of psychophysics, ruled by Smitty Stevens. A few blocks away was the Center for Cognitive Studies, presided over by George Miller. At the west end of Memorial Hall was the Pigeon Lab. Along with the proseminar, the 1st-year students were obliged to take a practicum, which consisted of a series of apprenticeships in these different laboratories, with the aim of insuring that each student had exposure to at least two of the three. Having done one experiment with George Miller and a second with another cognitive psychologist, I knew where my interests lay.

The action was in the Pigeon Lab. No one could go near it without noticing the amount of activity. Those students who started research there were outspoken about their enthusiasm. Like Herrnstein before him, Howie Rachlin started out in psychophysics, but was persuaded to try an experiment with pigeons by other students. He switched, just as Herrnstein had switched the generation before. He continued Nate Azrin's work on punishment by incorporating it into studies of choice, helping to undo the myth of the inefficacy of punishment that Skinner clung to (e.g., Rachlin, 1967; Schuster & Rachlin, 1968). In the end, 7 of the 10 students joined the Pigeon Lab.

Graduate students were housed in three offices, each with several desks. The one close to the psychophysics lab contained Ed Fantino, Phil Hineline, Howie Rachlin, Lois Hammer, and me. Two other desks were occupied by a student in cognition and a student in psychophysics (Larry Marks). Lois Hammer was quiet and kept to herself, but the rest of us were vocal and argumentative. We all practically lived there, so we had plenty of time and occasion for lively debates. Howie Rachlin and I would sometimes shout at each other so loudly that people from the psychophysics lab would complain. One of the offices close to the Pigeon Lab housed Laurel Furumoto, Al Neuringer, Richie Schuster, and Shin-Ho Chung. They were more ruly than the east-end guys, but still lively enough. Furumoto was really interested in history, but was doing an experimental dissertation in the lab. Neuringer and Schuster were friends before they arrived, and Chung seemed to be quiet by disposition.

Herrnstein's Pigeon Meetings

We took seminars together, we studied for prelims together, we had parties together, but once a week, without fail, we all converged on the seminar room for the pigeon staff meeting, presided over by Dick. Including Dick, we took turns presenting, one person each week, which meant that everyone presented at least once a semester. Usually it was a progress report, presenting some data, usually graphs with one point plotted for each condition run up to then. Sometimes it was description of a new experiment that had yet to produce any stable data. Sometimes it was an idea for an experiment yet to begin. Rarely were cumulative records unrolled; that was for the generation past. Diagrams sketched on the blackboard were usually speculations about quantitative molar relations. And many an equation was proposed.

Dick set the tone. He required that each experiment have a rationale. As part of the presentation, one had to be able to say what problem the results would illuminate. He often asked, and got us to ask, "What are the possible outcomes?" and "What would each outcome mean?" He would try to get us to do experiments that might be revealing regardless of how they turned out. Many of the experiments were aimed at extending and applying the matching law (e.g., Fantino, 1967; Fantino & Herrnstein, 1968; Killeen, 1968). Others aimed to reexamine old phenomena, such as delay of reinforcement (e.g., Chung, 1965; Chung & Herrnstein, 1967), conditioned reinforcement (e.g., Schuster, 1969), avoidance (Herrnstein & Hineline, 1966), and punishment (e.g., Rachlin, 1967). The most common theoretical suggestions were molar accounts, but any order at any level was relished. Dick would interrupt, ask challenging questions, and the rest of us followed suit. Experiments were redesigned, new conditions were added, and entirely new experiments were conceived. The New York element (Dick himself, Fantino, Neuringer, Schuster, Rachlin, and I) dominated. Sometimes the person presenting would stand mutely at the head of the table while the rest of us debated loudly. Naturally these debates spilled over into other occasions, in offices, at parties, even disrupting seminars.

How to recapture the excitement of the time? Every experiment seemed to break new ground and raise new possibilities. We knew we were turning the science that Skinner had founded into a quantitative field, with both reliable phenomena and a broad conceptual framework within which to understand them.

Running the Lab

The Pigeon Lab's capacity was soon challenged, but new experiments were set up in auxiliary rooms, and relay racks were wired so that two or more different experiments could be run in tandem. In an 8-hr day, one could run two experiments with 4 subjects each, and then, if you could find a way, sessions could be run overnight. The students ran some experiments themselves, necessarily on weekends, but during the week we had the luxury of two full-time workers to run experiments for us: Mrs. Papp and Wally Brown. Mrs. Papp, whose name was Antoinette and who Wally called ''Kitty,'' was a formidable, stout, middle-aged person, who took her mission to be the facilitation of the research. Her cooperation was essential, and usually was easily obtained, as long as one was respectful and well organized. Woe unto the student who crossed Mrs. Papp! Between them, Wally and Mrs. Papp changed a couple of hundred pigeons and rats each day. They hung out in the anteroom to the lab, guarding the entrance.

At the end of my 1st year as a graduate student, in 1963, the university moved us to a new building, William James Hall. The Pigeon Lab had about the same amount of space, taking up almost all of the seventh floor. Now we had windows with views, but the department was housed on four different floors, an arrangement that tended to isolate the different laboratories. The Pigeon Lab now had its own seminar room, two main rooms with about 50 relay racks in one and 50 enclosed chambers in the other, and several auxiliary rooms. Moving was a cooperative effort; the students ran cables under the raised floor and arranged equipment. A few of us moved important experiments across the street during the night. I remember wheeling a relay rack down the middle of the street. The rats in my dissertation research never missed a day.

Later Developments

After the peak in 1962, a few students came and went each year, maintaining the numbers about steady. The next year, Peter Killeen, Bob Boakes, and Bruce Schneider arrived. I went away for a postdoctoral year during 1965–1966, and returned as a postdoc in the fall of 1966. Howie Rachlin had the assistant professorship, so Howie, Dick, and I were again present at the pigeon staff meetings. Dick obliged whoever was assistant professor to apply for funding, which in those days was usually forthcoming, so Howie had a grant in addition to Dick's. Gradually, Howie and I began working together and took over much of the responsibility for running the lab, which was going at full capacity.

This period was an exciting and productive one for the three of us and for the students. I think Dick first sketched on the blackboard what we later came to call feedback functions—curves showing the dependence of rate of reinforcement on response rate—during this period (Baum, 1973, 1989). The first time may have occurred earlier, but we started using them a lot in the late 1960s. John Staddon had suggested taking the logarithm of ratios of responses as a way of making comparisons of rates, and I began applying this method to our experiments on choice (Baum, 1974, 1979; Baum & Rachlin, 1969). One day I met Dick as he came out of the bathroom, and he told me, ''Even if there's only one key, there's still choice. It's between pecking and everything else.'' He raced back to his office, and that day the Herrnstein hyperbola was born (Herrnstein, 1970). After we had talked about the hyperbola and the effects of varying the reinforcement for other behavior (r_o) in the pigeon meetings, I realized one day that higher response rates on ratio schedules might mean smaller r_o on ratio schedules. I found Dick in his office and told him. About a month later, he presented the same idea to me, apparently having forgotten our conversation. The group discussed so many ideas that no one was entirely sure where they all came from. When I wrote the paper that was published in 1973 under the title ''The Correlation-Based Law of Effect,'' I thought of it as an attempt to set down in writing the thinking of the group as a whole. I believe that Howie began talking about optimality before he left, but it wasn't until the early 1970s that I first proved that matching on two concurrent variable-interval schedules was optimal and presented it at a pigeon meeting. (I was ''scooped'' by Staddon & Motheral, 1978, but published my own proof in 1981.) Dick had little patience with optimality theory for some reason (possibly unfamiliarity with the calculus). I nevertheless incorporated the idea into the paper I submitted to *JEAB* (originally called ''Instrumental Behavior and the Theory of Feedback Systems: A Reinterpretation of the Law of Effect''), but the editor, Charlie Catania, had me cut the paper in half, and the ideas about optimality in the other half were published only in 1981.

In April of 1967, Dick offered me a position as research associate. I resigned from my postdoctoral grant and took him up on the offer. My job was to introduce experimental control by computer. Dick had gone in with a few other members of the department to apply for a large grant to found the computer-based lab, which was located on the 12th floor of William James Hall. This had started with a PDP-4® computer, but was augmented by a PDP-9T, a PDP-9 modified to facilitate time sharing. The general idea was to run various sorts of experiments all at once, some with human subjects, some with animals. I took over a small room on the 12th floor, installed a few pigeon chambers, and wrote programs in assembler code for the PDP-4 at first. A programmer on the project, Alan Razdow (now president of a software company), and I began work on a compiler-based system for programming operant experiments. Our first product was called OCSYS. It allowed one to specify and name inputs and outputs and to code modules that ran on interrupts generated by inputs and timers. I wrote several programs and successfully ran the experiments. Razdow and another programmer, Rob Strom, had a compiler planned, but that never materialized, in part I think because the SKED® program was coming along. I and students who arrived after 1967, such as Gene Heyman and Will Vaughan, continued to use OCSYS for about 10 years.

Because carrying pigeons up to the 12th floor was inconvenient, and part of the rationale behind the computer-based lab was to distribute control throughout the building, Scott Bradner, the technician for the lab, ran cables down the elevator shaft to the seventh floor. The computer-run experiments could then be integrated into the rest of the Pigeon Lab. Computer control never completely replaced the relay racks, probably because Dick was never completely comfortable with the computers.

When Howie Rachlin left for Stony Brook in 1970, I was given the assistant professorship. I applied for and got some funding for the lab. The only change was that now I had to attend department meetings.

At around this time, Dick hired Don Loveland to manage the lab. Don continued to organize and expand the opportunities for research. He installed four chambers for computer-controlled experiments in one auxiliary room. In another room across the hall, we had oversized chambers with their relay racks. In a room nearby, I had a few chambers in which pigeons lived continuously and enough space to run human beings. Don took care that all went smoothly, including running of animals and building and fixing apparatus. He stayed for several years, but eventually left to start his own business.

Society for Quantitative Analyses of Behavior

Also around this time, Dick suggested that we should organize a symposium on response strength and other quantitative topics. Neither he nor I was willing to put in the effort, however, so the idea languished. Several years later, in 1977, Dick was able to interest the indefatigable Michael Commons in it, with the result that Michael and Jim Mazur brought off the first Symposium on Quantitative Analyses of Behavior in April 1978. The same year the Society for Quantitative Analyses of Behavior (SQAB) was founded, for the purpose of sponsoring the symposium, which was henceforth an annual event. When SQAB incorporated in 1983, Dick was the first president. When he stopped being president in 1991, the SQAB symposium was moved to the time and place of the annual meeting of the Association for Behavior Analysis in 1992 (Commons, personal communication; more information may be found on the SQAB Web site, which may be reached by link at www.behavior.org).

The Final Years

Undergraduates also came to do research, about two or three each semester. Some just wanted the experience of doing research. Some did honors theses. Usually they went somewhere else for graduate school, but every now and then one would stay for graduate work in the department. In the early 1970s, an undergraduate, Lexa Logue, turned up in my office, wanting to do research. She performed excellently in Psychology of Learning, which Hal Miller and I taught by self-paced instruction, allowing the students to challenge themselves to the fullest. She came up with a good idea for an experiment, modified a rat chamber, and was off and running.

Lexa was one of the last graduate students in the Pigeon Lab. After her came Mark Snyderman, and I think he may have been the last. The lab was still going strong when I left in June of 1975. Except for the year I took off after college and my first postdoctoral year at the University of Cambridge, I had been there steadily since 1957, for a total of 16 years. After Don Loveland left, Will Vaughan took over his role, and Will and Dick worked together for years after. Jim Mazur and then Gene Heyman filled the assistant professorship. Gene was the last, however, and the number of graduate students declined steadily.

What killed the Pigeon Lab? Ultimately, I think one could blame the so-called cognitive revolution. The beginning of the end came in the early 1970s, when the university insisted on combining the Social Relations Department and the Psychology Department into one department, which occurred in 1972. The two departments had originally split over irreconcilable differences, and deep antipathies were apparent to a junior faculty member in either department. In part, the split could be traced to the ''soft'' versus ''hard'' dichotomy within psychology as a discipline; in part, it could be traced to the particular individuals, some of whom hated one another. Members of Psychology made openly deprecatory statements in my presence, and I assume similar talk went on in Social Relations. Because Social Relations was by far the larger department, merging the two could only spell disaster for Psychology.

The administrators had their way, of course. The administrator for the new entity, E. L. (''Pat'') Pattullo, calculated the number of square feet per faculty member in the various groups, and discovered that this ratio was hugely greater in the Psychology Department than in the Social Relations Department. The reason, of course, was the laboratories. Herrnstein argued in vain that one ought to count in all the pigeons when making this calculation. Playing squash with me one day, Pat wondered out loud if experimental psychology wasn't just ''too expensive.'' I pointed out to him that by such reasoning the university ought to eliminate the physics department, but I doubt I convinced him.

The merger with Social Relations affected both hiring decisions and the admission of graduate students. When Skinner had lost interest in maintaining the Pigeon Lab, only the members of Psychology were involved in the decision to preserve it by hiring a younger person to take charge. When Herrnstein lost interest, the members in Social Relations were in on the decision. I suspect the old antipathies came to the fore and, combined with the lab's occupation of desirable space, made the refusal to hire a new tenured member easy. It was probably facilitated too by the lack of students, which resulted at least in part from the shift of responsibility for graduate admissions to the new department as a whole. The members of the graduate admissions committee were primarily from Social Relations, and they favored applicants who shared their own interests. Incoming psychology students no longer were restricted to the earlier three choices and gravitated toward the ''softer'' areas.

With Herrnstein losing interest, fewer students, and no tenured person taking Herrnstein's place, federal funding agencies were bound to withdraw support. Probably the cognitive revolution operated there too, as cognitive psychologists became more numerous in the ranks of review panels.

Conclusion

Behavior analysis may be dead at Harvard, but it spread throughout the country as the students in the Pigeon Lab went elsewhere and established laboratories of their own. Other institutions, such as Columbia University and Indiana University, also produced prominent researchers. Instead of a few large laboratories, today we have many smaller laboratories spread from coast to coast in the United States and to New Zealand and even to some European countries and Japan.

The Harvard Pigeon Lab was a historical phenomenon with a clear beginning and end. It went through two generations, each inspired by the vision of its leader. Skinner discovered response rate, stimulus control, and schedules. He and his students saw the possibility of a real (natural) science of behavior and set about establishing that science based on those concepts. Herrnstein discovered relative response rate, the matching law, and the psychophysics of choice. He and his students saw that the science could be quantitative and set about making it so. In each generation, a pattern played out. An inspired leader attracted students until their numbers were large enough that they stimulated one another. When the group exceeded this critical mass, it took on a life of its own. Each group eventually ceased as the students left and the leader withdrew.

To me, the history of the Harvard Pigeon Lab seems like a golden opportunity for some historian of science. I say this for the following reasons. First, the phenomenon that occurred there must be typical of much of scientific progress. Although isolated individuals may make progress, groups in which members stimulate one another tend to be extraordinarily productive. Understanding in some detail how it happened at Harvard might result in insight into the social factors involved in science. Second, it happened twice. Someone studying the lab would have two replicates, not just one. Third, most of the students in both generations are still alive today. Someone could interview them for details and build a substantial database. Perhaps someone interested in the history of science may read these words and be inspired to do so.

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