

we do, is what Skinner once called methodological behaviorism. Its practitioners still are in the business of inventing intervening variables: Only the names of the intervening variables have changed. "Theories" that sprout like mushrooms, rather than basic research, are currently the popular roads to fame and tenure. The operational details of research on the behavior of nonhuman organisms offer little to practitioners who are practically concerned with knowing which of their customers is most likely to jump off a bridge this week, and in the present climate of "relevance," funds to pursue basic lab research are hard to come by. The Pigeon Lab was initially supported by money given for answers to the question of how most effectively to steer a bomb or nuclear missile to a remote target. Those who funded the Pigeon Lab were almost certainly innocent of the very real advances in the study of behavior that their grants subsidized.

Of course all of this activity went forward with our implicit assumption that something like Newtonian determinism was the appropriate paradigm for all scientific inquiry. Nonetheless, day-to-day observations contin-

ually revealed that behavior, whether that of rats, pigeons, or humans, violated one of the prime implications of ontological determinism; namely, that the behavior of an organism reverts to the steady state that prevailed before an intervention. If there is a fundamental truth about operant behavior, it is that it is a constantly evolving process characterized by an unending series of divergences. No organism can ever be the same as it was before its behavior was selectively reinforced. Skinner certainly did not propose the concept of a class of "emitted" behavior to anticipate quantum mechanics. The concept was simply the honest concession of the fact that we do not know, nor *can* we know, the specific eliciting stimulus that is responsible for the occasion of a particular operant response. The research conducted in the Pigeon Lab would be valid even if the new physical paradigm had been recognized and broadly accepted as the appropriate paradigm for behavioral research. In this respect, its work stands alone as a model for the new century and beyond.

*2023 Marina Cove Drive  
Hixon, Tennessee 37343*

---

### Gene M. Heyman (1970–1976)

#### THE HARVARD PIGEON LAB, 1970–1998: GRADUATE STUDENTS AND MATCHING LAW RESEARCH

In 1970, the year I began graduate school, the Pigeon Lab occupied about half of the seventh floor of William James Hall. The animal colony took up the center room, and the shops and "running" rooms, filled with experimental chambers and relay racks, formed the periphery. The heart of the lab was the collection of relay racks with their electromechanical counters, steppers, clocks, and timers. Linked by relays and wires, these devices counted behavior and doled out rewards. It looked like science but also a little like a Rube Goldberg cartoon. In the spirit of the latter image was the laboratory legend that Skinner once tried to dampen the action on his feeders by coating them with Karo<sup>®</sup> syrup.

#### *Graduate Student Education and Interest in the Matching Law*

In the Pigeon Lab graduate students had free rein. We had easy access to equipment and animals and pursued our interests with little overt direction from the faculty. For a while the lab technicians, paid by Herrnstein's grants, even ran our experiments. We weren't apprentices but new researchers. Under this laissez faire educational system, research projects were varied and sometimes idiosyncratic. In the 1970s they included projects on autoshaping, taste aversion, delay of reward, concept formation, visual discrimination, and foraging. The matching law, however, in its encompassing single- and con-

current-schedule form, was new, and its generality and predictive powers attracted the interest of many of the students who showed up to study behavior in the 1970s.

Herrnstein's first general paper on the matching was published in 1970. I had seen the equations the year before in Dick's undergraduate behavior course (Motivation and Action). In the first lecture, Dick promised an analysis of behavior built entirely on observable regularities, "as if we were visitors from Mars and had no assumptions about the inner life of behavior." The course's guiding theme was that behavior was a function of its consequences. Near the end of the semester, we learned that the rule for how consequences governed behavior was the matching law.

For those of us working on matching, a division of labor naturally emerged. Peter de Villiers and Hal Miller extended the equations to new situations. Peter developed a matching law description of avoidance and punishment (e.g., de Villiers, 1974), and Hal demonstrated that Bill Baum's generalized matching law (1974) predicted preferences between novel combinations of qualitatively different reinforcers (Miller, 1976). Jim Mazur and Lexa Logue found links between the matching law and other choice theories. Jim rewrote Premack's theory of reward from the perspective of the matching law, substituting quantitative for qualitative predictions (Mazur, 1975). Lexa conducted novel studies based on the formal similarities between the matching law and signal-detection theory (e.g., Logue, 1983; and see Davison & Tustin, 1978). Arturo Bouzas and I found that the matching law described the frequency of polydipsic drinking (Heyman & Bouzas, 1980). Will Vaughan focused on the relation between matching and reward maximizing (e.g., Vaughan, 1981). And Drazen Prelec, who started working in the lab as an undergraduate, derived a reinforcement feedback function for multiple reinforcement sources that tested a basic principle of formal choice theory, the constant ratio rule (Prelec & Herrnstein, 1978).

In the 1980s, Drazen and Will collaborated with Herrnstein on the idea that matching was the result of a simple, myopic form of reward maximizing that they called *melioration* (Herrnstein & Prelec, 1991; Herrnstein & Vaughan, 1980). During this period most of

the lab research was on melioration and the relation between the matching law and microeconomic principles. Because of the overlap with economic theory, these studies often used human subjects (Herrnstein, 1991). The experiments applied methods of the earlier animal studies to economic questions. The results serve as one of the early chapters in behavioral economics, a burgeoning new field that applies experimental methods to economic questions (Herrnstein, Rachlin, & Laibson, 1997).

I do not recall that we ever discussed why we found the matching law a compelling topic. My reasons, which are likely similar to those of the other graduate students, were its generality, predictive power, and simplicity. We played with the equations, deriving new, more complex models, or substituting real numbers for parameters that predicted new results. The predictions led to new experiments, and often enough the pigeons and rats agreed with the math (see publications by Mazur, Prelec, Vaughan, and me). Very exciting stuff, especially when the domain is behavior, a subject matter that is not usually the focus of mathematical description.

#### *Enter Computers*

In 1970 most of the experiments were controlled by electromechanical relay racks. However, Bill Baum and an MIT undergraduate, Allen Razdow (who later helped develop mathematical software, e.g., MathSoft and MathCad), created a real-time software program for running operant experiments. The computer, a PDP 9®, resided on the 12th floor of William James Hall and was connected to the Pigeon Lab by cables that ran through five floors of offices and classrooms.

Baum and Razdow called their language OCSYS. It consisted of little more than a clock, timer, and the PDP assembly language commands, such as "move the accumulator left," "deposit in displaced memory location 8," "retrieve from direct memory location 1005," and so on. The program for my first experiment required more than 2,000 lines of instructions (Heyman, 1977<sup>1</sup>). A later version of the same experiment, written in MED

<sup>1</sup> Heyman, G. M. (1977, March). *Reinforcing deviations from matching*. Paper presented at the meetings of the Eastern Psychological Association, Boston.

PC®'s state language (Heyman & Tanz, 1995) required just a few hundred lines.

With OCSYS it was possible to program dynamic reinforcement contingencies that mimic processes such as tolerance and satiation. For example, for my 1st-year project, I arranged a contingency that differentially rewarded behavior as a function of measures that were recalculated with each new response. The logic of this contingency was beyond the capacities of electromechanical equipment, but was well within the scope of even a primitive computer. Now, dynamic real-time contingencies are familiar and have helped identify the relationship between matching and reward maximizing (Heyman, 1977<sup>1</sup>; Heyman & Tanz, 1995; Vaughan, 1981). As for OCSYS, it remained a local language and disappeared not long after simpler languages, such as SKED®, became available.

#### *The Pigeon Staff Meetings*

In weekly lab meetings we presented new results and new equations. It was an intellectual free-for-all, with no special deference afforded the faculty or personal feelings. However, Dick Herrnstein's imaginative and insightful responses to new data and new models often led the discussion. His comments came with humor and anecdotes, and he was as quick to see the positive features of a research project as well as what rested on untested assumption. These meetings often had visitors. In the early 1970s they included George Ainslie, David Premack, Jock Millenson, and Herb Terrace. Skinner never came to the meetings while I was at Harvard, which probably reflected the lab's shift to theoretical and quantitative accounts of schedule behavior. Conversely, most of the graduate students of my generation were not that interested in the philosophical and methodological issues that Skinner championed. This distinction persists. For instance, graduate students of the 1960s went on to publish widely on behaviorist views of psychological phenomena, whereas those of the 1970s have not.

#### *1989 to 1998*

I returned to Harvard and the Pigeon Lab in 1989 to take a position as assistant professor (completing a chain of junior faculty associated with the Pigeon Lab that included

Herrnstein, Rachlin, Baum, de Villiers, and Mazur). At about this time, Dick stopped running animal experiments. The pigeon staff meetings became less frequent, and with Herrnstein's death in 1994, they stopped. However, matching law studies continued along with research in psychopharmacology and drug self-administration. The graduate students included Terry Belke, Nancy Petry, Larry Tanz, Jamie Taylor, and, at times, Bill Reynolds and James Roach. In addition, there was a constant stream of undergraduates in the lab, working on senior honors theses or special research projects.

#### *The Contingencies of Graduate Training in the Pigeon Lab*

There is an irony at the core of this story. In the cauldron of reinforcement contingencies, the students could not have been freer from environmental constraints. Adequate resources were available for the asking, and they came with no strings attached. To be sure, many graduate students worked on faculty-related projects, but this was not built into the program, and those who pursued independent interests had equal access to the lab. Yet, year after year, students undertook ambitious behavioral studies, producing a steady stream of PhD theses and publications. Zuriff, who was a student in the lab in the late 1960s, suggests that we were reinforced by the orderly results and sense of discovery (personal communication, August 2001).

The Pigeon Lab came to a nominal end in June of 1998. Over the years it served as a congenial and supportive home for newly minted behavioral scientists. Lab graduates have continued to be productive, training new generations of behavioral scientists and publishing widely on behavioral phenomena. The dynamics driving this output are, I believe, the inherent orderliness of behavior, the inherent curiosity of the students, and an environment that supported the healthy mix of natural order and curiosity.

#### REFERENCES

- Baum, W. M. (1974). On two types of deviations from the matching law: Bias and undermatching. *Journal of the Experimental Analysis of Behavior*, 22, 231-242.
- Davison, M. C., & Tustin, R. D. (1978). The relation between the generalized matching law and signal-detect-

- tion theory. *Journal of the Experimental Analysis of Behavior*, 29, 331–336.
- de Villiers, P. A. (1974). The law of effect and avoidance: A quantitative relationship between response rate and shock-frequency reduction. *Journal of the Experimental Analysis of Behavior*, 21, 223–235.
- Herrnstein, R. J. (1970). On the law of effect. *Journal of the Experimental Analysis of Behavior*, 13, 243–266.
- Herrnstein, R. J. (1991). Experiments on stable suboptimality in individual behavior. *American Economic Review*, 81, 360–364.
- Herrnstein, R. J., & Prelec, D. (1991). Melioration: A theory of distributed choice. *Journal of Economic Perspectives*, 5, 137–156.
- Herrnstein, R. J., Rachlin, H., & Laibson, D. I. (Eds.). (1997). *The matching law: Papers in psychology and economics*. New York: Russell Sage Foundation.
- Herrnstein, R. J., & Vaughan, W., Jr. (1980). Melioration and behavioral allocation. In J. E. R. Staddon (Ed.), *Limits to action* (pp. 143–176). New York: Academic Press.
- Heyman, G. M., & Bouzas, A. (1980). Context dependent changes in the reinforcement strength of schedule-induced drinking. *Journal of the Experimental Analysis of Behavior*, 33, 327–335.
- Heyman, G. M., & Tanz, L. E. (1995). How to teach a pigeon to maximize overall reinforcement rate. *Journal of the Experimental Analysis of Behavior*, 64, 277–297.
- Logue, A. W. (1983). Signal detection and matching: Analyzing choice on concurrent variable-interval schedules. *Journal of the Experimental Analysis of Behavior*, 39, 107–127.
- Mazur, J. E. (1975). The matching law and quantifications related to Premack's principle. *Journal of Experimental Psychology: Animal Behavior Processes*, 1, 374–386.
- Miller, H. L., Jr. (1976). Matching-based hedonic scaling in the pigeon. *Journal of the Experimental Analysis of Behavior*, 26, 335–347.
- Prelec, D., & Herrnstein, R. J. (1978). Feedback functions for reinforcement: A paradigmatic experiment. *Animal Learning & Behavior*, 6, 181–186.
- Vaughan, W., Jr. (1981). Melioration, matching, and maximization. *Journal of the Experimental Analysis of Behavior*, 36, 141–149.

*Behavioral Psychopharmacology  
Research Laboratory  
McLean Hospital  
115 Mill St.  
Belmont, Massachusetts 02478*

---

## Philip N. Hine (1962–1966)

### THE HARVARD PIGEON LAB IN CONTEXT

For me, to remember the Harvard Pigeon Lab is also to remember its context, the PhD program in psychology. When I settled into the basement of Memorial Hall in the fall of 1962 it soon became clear that a major priority would be mere survival. The results of the notorious annual preliminary exams had just been announced, leaving several students looking smug and comfortable, whereas the demeanor of some others suggested a shrinking presence. Survival was also going to require attending to some dimensions I'd never thought of. I quickly got into trouble by trading desk chairs with someone, only to be informed by Didi Stone, S. S. Stevens' secretary, that my chair had been purchased on a Stevens research grant and I was not at liberty to reallocate it. She did let me know, however, that I was welcome to continue using her grand piano in the nearby hallway (after hours, of course). That was the pattern: Relationships were universally cordial with music or skiing under discussion, but in academic matters we were acutely aware that there were two major fiefdoms in the basement of Memorial Hall, clearly demarcated by invis-

ible boundaries. S. S. Stevens, psychophysics, and the power law reigned at one end; Skinnerians with relay racks generating intricate patterns of behavior on reinforcement schedules were at the other end. Békésy's lab and the department office provided neutral zones on the back and front hallways, respectively.

Smitty Stevens (I found it easier to call Stevens "Smitty" than to call Skinner "Fred") became an even more imposing presence on the first meeting of the proseminar, when surveying the 13 newcomers arrayed around the massive table, he commented, "There are too many people in here." That first semester's heavy dose of sensory psychology was not what I had come to graduate school for, and it was several years before I recognized the value of what I had learned then. Billy Baum recognized the value more quickly, applying power functions to behavior in formulating the generalized matching law. In any case, our number had shrunk to 12 by the 3rd week, and one or two more disappeared by the end of the year. By the first midterm, it had become abundantly clear that I was in extremely formidable intellectual company.