

Robert Rescorla: Pavlov 2.0 and a Model Academic Psychologist

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INFORMACIÓN ART.

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ABSTRACT

Pavlov is one of the greatest psychologists. Being Pavlov 2.0 is very special. Although there are a number of distinguished students of Pavlovian conditioning, at least in my opinion, Bob Rescorla is the heir to Pavlov. But although Pavlov 2.0 is more than enough, there is a lot more. As discussed later, I see Bob as a linking person between the psychology of learning, perhaps the centerpiece of American academic psychology in the mid twentieth century, to cognitive psychology, its successor in the late 20th century.

Robert Rescorla: Pavlov 2.0 y Modelo de Psicólogo Académico

RESUMEN

Pavlov es uno de los más grandes psicólogos. Ser un Pavlov 2.0 es algo muy especial. Aunque ha habido varios estudiosos destacados del condicionamiento pavloviano, Bob Rescorla, al menos en mi opinión, es el heredero de Pavlov. Pero aunque ser el Pavlov 2.0 sería más que suficiente, hay todavía mucho más. Como comentaré, considero a Bob Rescorla como el nexo entre la psicología del aprendizaje, tal vez el núcleo de la psicología académica norteamericana de mediados del siglo XX, y la psicología cognitiva, su sucesora a finales de ese siglo.

Bob Rescorla is one of very few psychologists who is an academic hero of mine. He is a brilliant experimentalist. His beautiful analytic mind is on display in his teaching, in the same way as it is on display in his scholarship. He is one of the best teachers I have known: I gave a course at Penn on undergraduate teaching, for our graduate students, and one significant part of the course was a visit to a Rescorla lecture.

Whatever strict discipline Bob had with respect to research quality he was surprisingly open to the lower levels of control possible in areas of psychology quite different from his own. I found him sympathetic to the problems faced by other areas of psychology,

like social psychology. Bob was a dedicated citizen of the Penn department, and served as undergraduate chair, and full chair. He also served as undergraduate dean for the arts and sciences at Penn, with a special dedication to getting students involved in research fairly early in their careers. He served with dedication, efficiency, spirit, and without complaint. He was one of few people I know who excelled at research, teaching and administration. His superb and elegant scholarship, down to the smallest detail, is indicated by the fact that, so far as I know, he never had a paper rejected or a grant turned down.

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Bob came to Penn from Swarthmore College, one of the best if not the best undergraduate institutions in the United States. He was exposed to an excellent faculty, seriously dedicated to both teaching and research. At the time, Swarthmore, even though it had no graduate students, was probably the world center for Gestalt psychology, with Wolfgang Kohler, Hans Wallach and Solomon Asch on the faculty. Bob had a chance to study with Wallach and Asch, and also with Henry Gleitman. Henry was a superb teacher, and I'm sure an inspiration for Bob. Like Bob, Henry was outstanding as well in experimental design. Henry Gleitman became chairperson at Penn in Bob's third graduate year, so they had two chances to share ideas. Ironically, Solomon Asch came to Penn about a decade later but retired just before Bob arrived.

I met Bob in 1963, at Penn. It was my first year as an Assistant Professor and his second year as a graduate student. He came to Penn to work with Richard Solomon on animal learning, along with Vincent LoLordo, in the same year. I got to know him as a graduate student, and served on his dissertation committee. After a brilliant four-year-long graduate career, in which he did "classical" work on inhibition (some with LoLordo) and on proper controls, Bob took an Assistant Professorship at Yale, in animal learning. Carrying forward his passion for undergraduate education, Bob chaired the Undergraduate Seminar Program at Yale.

Fifteen years after joining the Yale faculty, Bob returned to Penn in 1981, a Professor of Psychology. I had just stepped down as chair at Penn, and was the chair who hired him. The decision of the Penn department to hire Bob was very special, perhaps unique. We had a senior slot, and actually decided to hire the best person we could find, *independent of area in psychology*. It was a very challenging and interesting search, and our first choice was Bob Rescorla. And he came. We were very fortunate.

I followed up on being on his dissertation committee with being on the dissertation committees of a few of Bob's students. Dick Solomon had a tradition of never being an author on the publications resulting from a student's dissertation. He felt that those properly belonged to the graduate student. Bob followed Dick's tradition, and unselfishly kept his name off his students' dissertation publications.

When Bob came to Penn as a graduate student, animal learning was right at the center of psychology. I had come from a PhD in biology and psychology at Harvard, where B. F. Skinner was a Professor. I had been exposed to learning and behaviorism, with the general framework that Pavlovian conditioning was a small part of the field, described as conditioned reflexes.

After a few years around Bob and Dick Solomon, I realized that Pavlovian conditioning was so much more than what I thought it was! I found it much more interesting than instrumental learning, and of course, it was a model system for learning about foods. John Garcia's two classic studies, in 1966, were just what I needed in my work on specific hungers to make sense of the learning I was studying. My own work in this area, which stopped a few years after Bob graduated, led me to challenge "general process" learning theory, and think of learning as an adaptive specialization, shaped to specific real world contingencies presented by different functional systems. This led me, along with John Garcia, Sara Shettleworth and Martin Seligman, to challenge the reigning general process view of animal learning, and

to participate in a movement that would play a part in downplaying the importance of learning as a domain general system at the center of psychology. The general process feature of learning was not central to Bob's work, and I never felt any tension with him on this subject. In fact, Bob incorporated taste-aversion learning in his beautiful paradigmatic work.

I was not a personal friend of Bob's, but we had many stimulating collegial relations. I liked the idea of having Bob on my graduate students' committees, but they were afraid of him, and imagined spending years running control groups. Of course, Bob was the master of controls, and was very economical. He would find the one control that was the right one. I once had a graduate student who allowed him on her committee. At one meeting, she indicated the controls she thought she had to run for a study, and Bob said she could do with fewer controls. She foolishly took him to task for running insufficient controls. It is my experience that when I run into someone who is a master of something, they are much more relaxed about the "niceties" than people a little less expert. Thus, in my experience, a real master of statistics is quite relaxed about the use of parametric vs non parametric statistics, and the master of controls was likely to decrease the number of controls you would need.

When Bob came to Penn as a graduate student in 1962, behaviorism and learning were still in their prime in academic psychology. By the time he returned to Penn in 1981, there was some decline in the enthusiasm of psychology for his field. There was the modularity approach from within learning, ethology, and language, and the rise of cognitive psychology/science toward center stage in psychology. Ironically, a major piece of the move to cognitive psychology was Ulric Neisser's magnificent *Cognitive Psychology* book, in 1967. I think it was the first synthesis of the new field. Neisser had been a young faculty member at Swarthmore, and he actually wrote the book when he was on sabbatical from Cornell, at the Penn department, I think in 1966-67. He gave a great seminar on the subject of his book, and almost converted me. Bob had already left Penn for Yale, and just missed the seminar. Some of the elegant refinements of the experimental approach in the psychology of learning were adapted by cognitive psychology.

Bob's basic lecture course was the psychology of learning. Although the field became less popular, his lectures were so good that he had substantial sized classes for decades. Bob elected to continue on his path of traditional but very innovative studies of Pavlovian conditioning, and never veered from that path. He realized that graduate students would have more and more trouble getting academic positions in animal learning, and insisted that his graduate students, later in his time at Penn, develop competence and some research interest in either clinical psychology or neuroscience. Those were two areas in which Pavlovian conditioning had a major influence.

On a number of occasions, I tried to get Bob interested in evaluative conditioning, which is Pavlovian conditioning of likes and dislikes in humans. Taste aversion learning is an example. I did some work in that area, and I would have loved to collaborate with him, but he had no interest in this distraction. His attitude was similar to many other learning psychologists, and unlike others, who saw the decline in the area and branched out. One of his co-Solomon graduate students, Martin Seligman, at the same time, moved off toward clinical

psychology (learned helplessness and depression), and moved along from that area after a while.

I think the framework for Pavlovian conditioning that Bob, and some others adopted, actually served as a bridge to cognitive psychology. In Bob's hands, what was central to Pavlovian conditioning was the association of mental representations. From the very beginning of his career, he was interested in inhibition and the determinants of fear. The response, sometimes bar pressing on a variable interval schedule, was an indicator of fear, that is, of what was going on in the head. This was also true of his early work on contingency; learning about the relationships of stimuli. His later work on higher order conditioning and sensory preconditioning, was about the relationships of stimuli, with the response, again, a way of getting a readout of relationships of mental events, or their equivalents.

Bob's later work on context had this same property, and of course, was a rebirth of his early exposure to Gestalt psychology at Swarthmore. From my perspective, his work on stimulus compounds addresses one of the fundamental questions of cognitive science: how do we learn about the coherence of different features of an object? This is often a form of simultaneous conditioning, another problem studied by Bob. His work does a good job making a connection between Pavlovian conditioning and its predecessor, the association of ideas as presented by the British empiricist philosophers.

Bob was making a link between two great traditions of central relevance to psychology, and giving a gift to modern cognitive science. Robert Rescorla really studied a higher order of conditioning and association. Pavlov would have really admired him.