Robert A. Rescorla

1940–2020

BIOGRAPHICAL *Memoirs*

A Biographical Memoir by
Vincent LoLordo

NATIONAL ACADEMY OF SCIENCES

ROBERT ARTHUR **RESCORLA**

May 9, 1940–March 24, 2020

Elected to the NAS, 1985

Photography by School of Arts and Sciences, University of Pennsylvania

By Vincent LoLordo

Robert A. Rescorla was a masterful experimental psychologist in the field of animal conditioning. Throughout his graduate school years and a long career in academia, he designed, conducted, and interpreted a number of subtle experiments that were based on the 19th- and early 20th-century work of the Russian Nobel Laureate Ivan Pavlov but greatly expanded Pavlovian concepts in ways that carried our knowledge of animal behavior into new and fruitful pathways, many of which continue to inform physiological research.

Rescorla gained an undergraduate degree from Swarthmore College in 1962 and a Ph.D. from the University of Pennsylvania in 1966. He began his academic career that same year as an assistant professor of psychology at Yale University, eventually becoming a full professor before he departed Yale in 1981. He then returned to Penn, holding two successive endowed chairs in psychology and serving a term as Dean of the College of Arts and Sciences before retiring in 2009.

Robert Arthur Rescorla was born May 9, 1940, in Pittsburgh, Pennsylvania, to Mildred Jenkins Rescorla and Arthur R. Rescorla. His father was in the oil business, and the family moved from place to place while he was a child. He finished high school in Westfield, New Jersey. Bob, as he was generally known, earned his bachelor's degree at Swarthmore College, where he received highest honors. While there he had studied with Henry Gleitman, a former student of the cognitive psychologist E. C. Tolman, and with gestaltists Solomon Asch and Hans Wallach. So when he came to the University of Pennsylvania in 1962 in quest of a doctorate, he was open to theoretical alternatives to the behavioristic, stimulus-response (S-R) approach that had been dominant in animal learning.

The Ph.D. program at Penn in 1962 can fairly be described as a research apprenticeship. In the first year of the program there were two required classes. The graduate proseminar was taught by luminaries in diverse areas, including Richard L. Solomon in learning, Eliot Stellar and Phil Teitelbaum in physiological psychology, and Leo Hurvich and Dorothea Jamieson in vision. In the class on quantitative psychology, Bob Bush, David Krantz, Saul Sternberg, and Duncan Luce attempted, with mixed success, to teach a dozen or so students with varied backgrounds enough probability theory, statistics, and calculus to render them quantitatively sophisticated. In addition there was the research project, functionally equivalent to an M.Sc. thesis.

Bob and I (the author) also attended Dick Solomon's weekly research seminar along with Dick's research assistant Lucille Turner and students Bruce Overmier, Russell Leaf, Mike Lessac, and Judy Crooks, who had come to work with Dick a year or two earlier. Dick's primary grant funding was to study Pavlovian fear conditioning and instrumental avoidance learning in dogs, and his enthusiasm for that area was contagious, so much so that by the end of our first year Bob and I, even though the topics of our first-year research projects had not been especially close to Dick's interests, found ourselves spending most of our research time trying to come up with new experiments that would drive the research on Pavlovian fear conditioning forward, further extending Pavlov's findings from the domain of salivary reflexes to the much more interesting domain of emotional responses.

Bob with our PhD supervisor, Dick Solomon, in Maine, circa 1993.
(Photo Shirley Steele.)

We asked ourselves what sort of temporal arrangements of, say, an auditory conditioned stimulus (CS) and an aversive-shock unconditioned stimulus (UCS) would turn that CS into an inhibitor of fear. Our answer—in cognitive language—was that perhaps a CS that occurred instead of an expected shock would suffice. We implemented this idea by first training dogs to avoid footshock, and then in a second phase where no avoidance was possible, giving them two kinds of Pavlovian conditioning trials, intermixed: (1) CS1 occurred and was followed a few seconds later by footshock; (2) CS1 occurred and was followed a few seconds later by CS2 and no shock. In a final phase we presented the CS2 while the dogs were avoiding, and the CS2 markedly inhibited avoidance behavior.

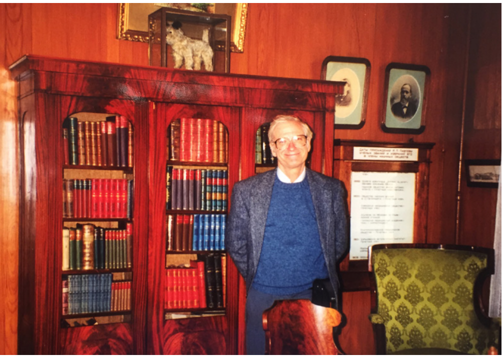
In subsequent experiments Bob and I showed that other arrangements of CS2 and UCS in which the CS2 could be described as signaling no shock in a situation where shock otherwise occurred also made that CS2 into an inhibitor of fear. Among those arrangements was one that had been used as a control for non-associative effects in studies of Pavlovian excitatory conditioning. In this procedure, called the explicitly unpaired procedure, the CS occurred on some trials, and the UCS on others. The CS did not become an associatively neutral stimulus; it became a conditioned inhibitor of fear.

This result led Bob to ask what arrangement of CS and US would make the CS associatively neutral. He reasoned that if CS-UCS pairings, in which occurrence of the UCS is positively contingent on occurrence of the CS, make the CS into an excitor, and the explicitly unpaired procedure, in which the UCS is negatively contingent on the CS, makes the CS into an inhibitor, then the absence of a contingency between the two events—when they both occur repeatedly but are uncorrelated in time—should leave the CS associatively neutral. This reasoning was at the heart of a theoretical paper that Bob wrote for the *Psychological Review*. From that time on, his ideas, more than those of any other scholar, drove theorizing in the field of associative learning.

In that 1967 *Psychological Review* article, "Pavlovian Conditioning and Its Proper Control Procedures," Bob went beyond Pavlov by suggesting a theoretical framework for the results I have just described—the idea that the contingency between CS and US, not their temporal pairing, determines the nature and extent of conditioning. Soon afterward he showed that, in the so-called zero-contingency case, no conditioning occurs, no matter how frequently the US is paired with the CS. Thus temporal contiguity of CS and UCS, long thought to be sufficient for conditioning, was not sufficient.

Bob's experiments in manipulating the contingency between CS and UCS and the theoretical framework that arose from those experiments had enormous impact, paving the way for exploration of Pavlovian conditioning as a model for how an animal makes sense of the structure of events in the world, sorting out which events can be used to predict which other events. Like all great experimental discoveries, this one raised profound and heretofore unasked theoretical questions, one of which was exactly how to define contingency.

In that regard, it should be noted that Bob never argued that the animal directly apprehended the contingency—for example, learned that the CS and UCS were uncorrelated.



Bob in Pavlov's office.
(Photo Shirley Steele.)

That fact makes one think again about the extent to which he was a cognitive psychologist. Given his focus on how the temporal arrangement of CSs and UCSs affected the CS's ability to evoke a central representation of the UCS, one might imagine that he had become a thoroughgoing cognitive psychologist. I don't think that was so. Consider a phenomenon first studied by Bruce Overmier, Martin Seligman, and Steve Maier in Dick Solomon's lab at about the same time that Bob was investigating the effects of the contingency between CS and UCS on Pavlovian fear conditioning. In brief, they found that prior exposure to a large number of inescapable shocks in one situation left dogs unable to escape shocks in a new situation in which escape was possible. The authors interpreted this interference effect as manifesting learned helplessness arising from the dogs' learning that nothing they did in phase one affected the probability of shock ending.

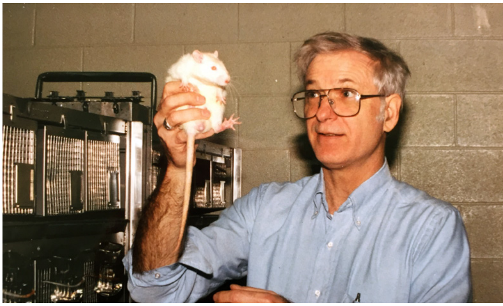
The learned helplessness hypothesis made a big splash because of its implications for understanding depression, but also because it was a cognitive account, stipulating that the dogs acquired knowledge…the knowledge that there was no contingency between any response they made and shock ending. Bob never liked the learned helplessness hypothesis, though to my knowledge he never criticized it in print. It was more that he ignored it, despite the centrality of contingency in his thinking at that time. I suppose this is less than surprising, given his unwillingness to embrace even the learned irrelevance hypothesis, which argues that rats exposed to uncorrelated CSs and UCSs learn that they are uncorrelated. That hypothesis has been difficult to confirm.

While he was a graduate student Bob, along with Solomon, wrote a second article for the *Psychological Review* titled "Two-Process Learning Theory: Relationships Between Pavlovian Conditioning and Instrumental Learning." The two men considered ways in which Pavlovian-conditioned emotional and motivational processes could play a role in the control of instrumental responding. They proposed that the interaction of the Pavlovian and instrumental processes could best be understood by superimposing Pavlovian CSs upon instrumental responding, as Bob and I had done by presenting signals for shock and for the absence of shock to dogs that were engaged in avoiding shock.

Another example of a Pavlovian-to-instrumental transfer (now called PIT) experiment is the widely studied conditioned-emotional-response procedure, in which rats (1) are trained to perform an instrumental response for food reinforcement and (2) receive Pavlovian fear conditioning, and then (3) are presented with the Pavlovian CSs while they are engaged in the instrumental response. The CS suppresses the food-getting behavior. Because a Pavlovian CS can be either excitatory or inhibitory, a US can be appetitive or aversive, and an instrumental response can be either positively or negatively reinforced, all the Pavlovian-to-instrumental transfer experiments can be fitted into an eightfold table. Moreover, one can use this design to ask about control of instrumental learning by a Pavlovian CS that has the same motivational valence as the instrumental reinforcer but is qualitatively different, as in my own dissertation, which showed that a signal for an aversive loud noise facilitated shock avoidance learning.

The response to Bob and Dick's paper, which became a Citation Classic, was very positive, and experiments on PIT abounded. At first these experiments were purely behavioral, but in recent years behaviorally sophisticated neuroscientists have added PIT to their methodology, and the number of studies using the technique to identify essential neural circuits for learning in striatum, amygdala, and habenula has increased dramatically.

Early in his third year at Penn, Dick surprised Bob by telling him that he done first-rate research and should be able to finish his degree requirements by the end of the year and move on to an academic job. Bob was startled; he didn't know of anyone who had finished in three years, and he knew the degree requirements included a grueling written exam that lasted all day, followed a month later by a long oral exam in which the candidate and all the faculty sat around a seminar table, and questions could range from "What is the constant error in psychophysics?" to "What did Hull's and Freud's theories have in common?" In response to Dick's suggestion, Bob blurted out something like, "Gee Dick, I like it here and I want to stay the full four years." Dick said okay.



Bob with a rat in his lab, University of Pennsylvania 1992.
(Photo Shirley Steele.)

Bob received his Ph.D. from Penn in 1966. His dissertation was titled "Inhibition of delay in Pavlovian fear conditioning." In the PIT procedure that we had developed, dogs received Pavlovian fear conditioning with a long-duration CS ending with a shock. Then, in the absence of shock, the long CS was superimposed upon a previously trained avoidance baseline. The rate of avoidance responding fell below the baseline for the early part of the CS and then increased as the CS continued, finally exceeding the baseline toward the end of the CS. This elegant demonstration of inhibition of delay in fear conditioning revealed the generality of yet another phenomenon discovered by Pavlov for salivary conditioning with food.

With his doctorate in hand, Bob took a position as assistant professor of psychology at Yale University. In those days Yale did not promote any of its assistant professors to the rank of associate professor with tenure, but they made an exception in his case, and he was a full professor when he left Yale in 1981.

Bob's empirical work during his first few years at Yale, along with important experiments by Leon Kamin and Allan Wagner, further extended the domain of Pavlovian conditioning to the case where several stimuli are present on a trial. He pointed out that if we think of the background stimuli in the conditioning chamber—call them context—as being present throughout the conditioning session, then the zero-contingency case, in which the CS does not become conditioned, boils down to a mixture of context-alone trials and trials in which context is accompanied by the discrete CS, and the US is equally likely in both kinds of trials. He further saw that this case was formally similar to the case in which a CS is repeatedly followed by the US, and then a second CS is added to the first as conditioning continues with the same US.

Kamin showed that the second stimulus does not become conditioned, and he called this the blocking effect. These findings led Bob, along with his Yale colleague Allan Wagner, to develop a mathematical model, published in two elegant chapters in the early 1970s, that computed the change in associative strength accruing to each CS present on a Pavlovian conditioning trial. The key was the idea that the amount of conditioning that would accrue to a CS on a trial depended on the discrepancy between the combined associative strength of all the CSs present on that trial and the maximum associative strength that the US would support.

Of course, the model predicted blocking, Wagner's relative validity effect, the effects of manipulating the contingency between CS and US, and many other standard outcomes. Moreover, the model suggested several new experiments and predicted their results, too.

Results that were discrepant with the model led others to propose modifications and alternatives, and they are still being developed, but the Rescorla-Wagner (R-W) model left its lasting mark on the form that theorizing in associative learning would take. It is unquestionably the most influential, widely cited, and widely built-upon theory of associative learning.

One of the themes shared by many of the stories about the creation of the R-W model is that it involved very little face-to-face interaction between the authors. For example, one account says that the two men didn't interact very much at Yale, and that only when they boarded a plane for the flight to Halifax for the Dalhousie Conference on Associative Learning in June 1968 did casual conversation reveal that they were going to set out very similar theoretical accounts. I recently reread the two conference papers, and the two sets of speculations do have important common elements. Nick Mackintosh, who drove the two from the airport to their hotel, later reported that they spent the half hour ride talking animatedly about how they could model their ideas. Bob never confirmed this story, nor did he deny it.

Although by the mid-seventies his theorizing, and reactions to it, had become a primary focus of activity in the field of associative learning, Bob thought of himself as primarily an experimentalist, and the designs of his experiments on Pavlovian conditioning and instrumental learning were beautiful. Most of his nearly 200 incisive empirical papers included multiple experiments with replications, so the findings were ironclad. As far as I know, he never had a paper rejected.

Bob summarized much of the research he conducted at Yale after his focus shifted away from the R-W model in a book titled "Pavlovian Second-order Conditioning: Studies in Associative Learning." The focus of this research was not on the conditioning of responses, but instead on the association of stimulus representations—that is, mental events. The responses were viewed primarily as indicators of those associations.

Second-order conditioning, first observed by Pavlov in salivary conditioning, refers to the case where a CS, call it CS1, is repeatedly followed by a UCS until the former reliably evokes a response. Then a second CS, call it CS2, is repeatedly followed by the CS1 (in the absence of the UCS) until the CS2 reliably evokes the response. Bob noted that the response to CS2 could only be attributed to an acquired association between the representations of CS2 and CS1 if control groups given pairings in only one of the two phases failed to acquire the response to CS2. After demonstrating second-order conditioning in several preparations with rats and pigeons, Bob went on to use the procedure as a tool for studying diverse aspects of associative learning. A couple of examples will reveal how he did this.

If a hungry pigeon receives repeated trials on which a small disk on the wall is illuminated for five seconds (CS1) and then some grain is made available (UCS), the bird will learn to approach and peck the disk, thereby revealing that it has associated the lighted disk with food. This effect is called autoshaping. However, if a tone is used as the CS1, then the bird does not learn to peck, raising the possibility that it has not associated the tone and food. Bob used second-order conditioning procedure to assess this possibility. After first-order pairings of the tone CS1 with food, then in second-order conditioning the disk was illuminated with either red or green light, and only one of those colors was followed by the tone CS1. The birds learned to peck the color paired with the tone, but made few pecks to the other color. This result indicates that during the initial tone-food pairings the birds did acquire the first-order association, though it was ”silent” at the time.

Bob suggested that the impact of temporal contiguity in establishing an association between two events can be modulated by their similarity. He and David Furrow demonstrated this phenomenon in the second-order autoshaping procedure. Four visual stimuli could be projected on the disk: blue, green, horizontal black line, and vertical line. In first-order conditioning all birds were treated alike: blue and horizontal were followed by food; green and vertical were not, and the birds came to peck only blue and horizontal. Then in second-order conditioning one group received trials on which the first- and second-order CSs were similar; green was followed by blue, and vertical was followed by horizontal. For the second group the members within each pair were dissimilar; green was followed by horizontal, and vertical was followed by blue. Pecks per minute at the disks revealed that birds acquired a much higher rate of pecking at the second-order CS when it was similar to the first-order one—that is, that similarity enhanced association by temporal contiguity. Again, second-order conditioning was a valuable tool for the study of association.

Shortly after the publication of his book on second-order conditioning, Bob returned to the University of Pennsylvania, where he was the James M. Skinner Professor of Science from 1986 to 2000 and then the Christopher H. Brown Distinguished Professor of Psychology from 2000 to 2009. Bob always believed that a faculty member, no matter how eminent as a researcher, should do his or her fair share in contributing to the running of the department and the university. He acted on this belief, serving as chair of the department from 1985-1988 and Dean of the College of Arts and Sciences from 1994-1997. Throughout this period he continued to publish important research at a high rate. I'll discuss just a couple of examples.

Bob turned his attention to the associative structure of instrumental learning, using a within-subjects design that he had used previously in Pavlovian conditioning. In the early days of research on animal learning, Edward Thorndike had argued that what was learned was an association between the stimulus situation and the response—that is, that the outcome was not part of the associative structure. Bob used what has come to be called a devaluation procedure to show that, on the contrary, the outcome is associated with the instrumental response. Rats were trained to make two different instrumental responses, chain pulling and lever pressing, each followed by a different food outcome. Then one of the outcomes was devalued by making the rat ill after he consumed it, while the other outcome was not devalued. During this treatment the chain and lever were absent, so the instrumental responses could not be directly associated with illness. In the test phase the rats were allowed to make the instrumental responses, but the outcomes were not presented. If the rats had acquired response-outcome associations, then they would make the response that had formerly led to the devalued outcome less frequently than the response that had led to the non-devalued outcome, and that is what they did.

Later Bob and Ruth Colwill extended the analysis of instrumental learning to the more complex case where the response produces an outcome only in the presence of some stimulus, called a discriminative stimulus. In this case there is the possibility of hierarchical learning—that is, an association between the discriminative stimulus and the response-outcome association. In one experiment two discriminative stimuli were presented separately. In S1 response 1 produced outcome 1 and response 2 produced outcome 2, but in S2 response 1 produced outcome 2 and response 2 produced outcome 1. Then the rats were divided into two groups for a devaluation treatment as above. One group was made ill after outcome 1, and the other, after outcome 2. In a test a few days later the rats were allowed to make the responses in the presence of the discriminative stimuli, and they displayed knowledge that the response-outcome associations were conditional upon which discriminative stimulus was present. For example, if outcome 1 had been devalued, then in S1 they made response 2 more than response 1, but in S2 they made response 1 more than response 2. This result and others from the same report indicate that the rats knew which outcome went with which response in each of the stimuli. Hierarchical learning had occurred.

Toward the end of his research career Bob displayed his unparalleled mastery of experimental design in some experiments designed to assess how the associative strengths of two equally salient CSs with different initial associative strengths would change as a result of a trial on which they were conditioned in compound. Notice that the R-W model, which includes a common error term—that is, the discrepancy between the maximum associative strength that the UCS will support and the sum of the associative strengths of all the CSs present on the trial, predicts that the two CSs will gain the same amount on a reinforced trial, and lose the same amount on a non-reinforced one.

In Bob's first experiment, one of the CSs was an excitor and the other, an inhibitor. There were three phases. In the first he established two excitors, A and C, and two conditioned inhibitors (B and D from an X+, XB-, XD-treatment). Then he reinforced a compound of an excitor and an inhibitor, so AB+. In the final compound-test phase he presented two compounds, AD and BC, each containing an excitor and an inhibitor. Notice that without the middle AB+ phase, one would expect equal responding to the two compounds (because they each included an excitor and an inhibitor). So in the experimental group, with the AB+ treatment, the difference in responding to AD and BC tells us which of A or B gained more strength.

In several experiments using different conditioning preparations, animals responded more to BC than to AD, indicating that the inhibitor gained more strength than the excitor. Analogous experiments with the AB compound non-reinforced in the middle phase showed that B, the inhibitor, lost less associative strength than A, the excitor, from non-reinforcement. Bob used this compound test procedure to answer other vexing questions—for example, is the growth of associative strength over conditioning trials negatively accelerated?

The elegance and importance of Bob's research and scholarship were widely recognized. He was elected to the National Academy of Sciences in 1985, received the Distinguished Scientific Contribution Award from the American Psychological Association in 1986, became a William James Fellow of the American Psychological Society in 1989, and was awarded the Howard Crosby Warren Medal of the Society of Experimental Psychologists (1991) and the Horsley Gantt Medal of the Pavlovian Society (2005).

As a young scientist Bob was very fired up, almost to the point of being ill, whenever he was about to give a lecture at an important conference or convention, and during the lecture this state manifested as an excitement for the science that audiences found very engaging. This quality carried over to the lectures in his undergraduate class on learning and, along with the relentless logic of his presentations and his extraordinary ability to explain complicated concepts, made that class a favorite among the Yale and Penn undergraduates. The College of Arts and Sciences at Penn awarded Bob the Ira Abrams Teaching Award in 1999. Bob liked having undergraduate honors students in his laboratory, and published a number of papers with undergraduates as co-authors.

In his administrative roles Bob emphasized the importance of undergraduate education. As undergraduate dean in the School of Arts and Sciences at Penn, he advocated for inclusion of active research experience in the undergraduate experience. He maintained that more attention should be paid to the quality of teaching when faculty are being evaluated. Moreover, when he was chair he resisted a bit when eminent colleagues asked to buy out of their teaching in order to devote more time to their research enterprise.

From his early days at Yale, Bob was also actively engaged in training graduate students and postdocs, many of whom have made major contributions to the study of animal learning, behavioral neuroscience, and related fields. In August 2011 these people gathered at Penn to honor Bob and present their own current research.

Bob was a very intense person. He was relentlessly logical, didn't tolerate loose thinking, and liked challenging other confident people. But he recognized that not everyone was happy with the give and take, and he wasn't a bully. I recall one example of his fair-mindedness, at a time in our first year of graduate school when a visiting speaker at our departmental colloquium, which was organized by the graduate students, talked about his method of interpreting Rorschach inkblots. The content of the talk was a mismatch for the Penn department and the speaker was shy and a bit intimidated, but he persevered. In the question period one of our senior clinicians, a blustery individual, made a personal attack on the speaker. Bob was outraged, and afterwards told the fellow that if he ever pulled a stunt like that again we graduate students would ban him from colloquium.

Paul Rozin, a colleague of Bob's for many years, reports that Bob did not aggressively promote hiring in his own area and, given his very high standard for what constituted evidence for a conclusion in animal learning, was amazingly tolerant of the work of scholars in areas of psychology with less experimental control.

Bob and I were close friends for nearly sixty years, and over the last eight years or so I flew down to Austin once or twice a year to spend some time with Bob and his wife, Shirley. We never did much, just sat around and talked, usually with a little Jack Russell/

Dachshund cross named Tater on Bob's lap. On each visit I marvelled at my friend's extraordinary mind, enjoyed exchanging quips (I always liked to make him laugh), and appreciated what a good person he was. He had a strong moral sense, and despite his ferocious intellect he could be a very kind person…very kind to me, and to a succession of scruffy little dogs over the years.

Vincent LoLordo (left) and Bob, at his 2011 retirement celebration at the University of Pennsylvania.
(Photo Shirley Steele.)

Robert Arthur Rescorla, emeritus professor of psychology at the University of Pennsylvania, died in Austin, Texas, on March 24, 2020, following a fall in his home. He was 79. Bob is survived by his spouse of 28 years, Shirley Steele, of Austin, and by his sister Barbara Rescorla Brandt, of Gallup, New Mexico. He is also survived by his first wife, Marged Lindner, and by his sons, Eric and Michael Rescorla from his marriage to the late Leslie Rescorla; by Eric's spouse, Lisa Dusseault, and grandsons Darwin and Lincoln; and by Michael's spouse, Melanie Schoenberg Rescorla, and grandsons Alexander and Nicholas.

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