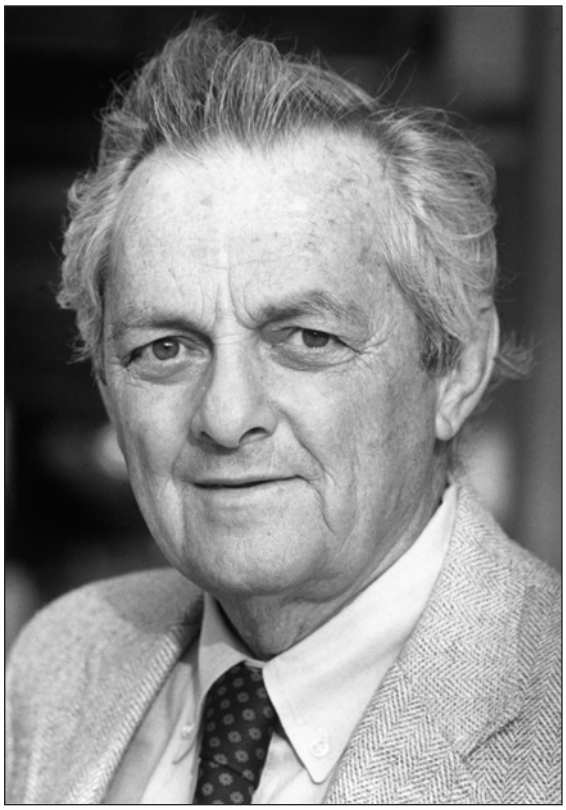
Learning & Behavior
2007, 35 (1), 1-10



Abram Amsel (1922–2006)
In Memoriam

Abram Amsel, the founding editor of this journal, died on August 31, 2006. When the journal was established in 1973, it was titled *Animal Learning & Behavior*. In his editorial in the inaugural issue of *Animal Learning & Behavior*, Amsel stated that he expected the journal would increase communication among investigators with various interests in the study of fundamental aspects of learning and behavior. He noted that, despite their particular orientations, this community of scientists "all share an interest in the learning and behavior of nonhuman, as well as human animals, and each has something to contribute." Thirty years later, this scientific cosmopolitanism was recognized by changing the name of the Journal to *Learning & Behavior*.

Amsel once lamented that Clark L. Hull, a major learning theorist in the first half of the 20th century and an enduring influence on Amsel's work, was typically remembered for one book:

> We have here an example of how great men and women of science often come to be known for restricted portions of their work—in Hull's case, for *Principles* [*of Behavior*, 1943]—while substantial portions of other important facets of their work are "rediscovered" in a more modern context. This is, perhaps, inevitable; but, still, there should be an attempt to keep the historical record alive and straight (Amsel & Rashotte, 1984, p. 11).

Abram Amsel's own scientific contributions spanned the second half of the 20th century, and the following comments are partially aimed at keeping the historical record straight about the many facets of his work. They also provide some perspectives on him as a scientist, teacher, and mentor.

M. E. Rashotte, rashotte@gmail.com

In overview, Amsel's long scientific career can be viewed as comprising three stages. In the first, which began in 1948 and lasted about 20 years, he focused on understanding the interactive effects of reward and nonreward in instrumental learning tasks. In this stage, many of his behavioral experiments with adult rats were strongly guided by his own theory of frustrative nonreward, proposed in the 1950s. Amsel's frustration theory was cast in the framework of Hull–Spence learning theory, a theoretical orientation he acquired during graduate training at the University of Iowa. In the second stage, which lasted about 15 years, he continued to study reward-schedule effects, but from the perspective of ontogeny. This work was guided by hypotheses from developmental neurobiology, but also had implications for theories of reward-schedule effects based on studies with adult animals. In this work, his lab utilized a battery of behavioral tests involving manipulations of reward and nonreward that were applied to infant and maturing rats. The results indicated a clear sequential ordering in the age at which the various reward-schedule effects first appeared. That finding was not anticipated by theories based on adult data, which attributed several of those effects to common processes. The results also posed new questions for him about the effects on adult behavior of memories retained from early learning experiences. The third stage of his career, which also lasted about 15 years, began with behavioral neuroscience studies in which the effects of invasive techniques that damage the hippocampus were studied in reward-schedule experiments with rats at various stages of early development. These studies revealed new information about the neural bases of reward-schedule effects in the developing animal, and ultimately led his lab to carry out purely neuroanatomical studies.

Amsel is often characterized as a key player in behavioral research in the 1950s and 1960s, and as the behavior theorist whose frustrative-nonreward theory made emotional reactions part and parcel of thinking about reward-schedule effects. But such a characterization would be only part of the story. Over the arc of his long career Amsel made striking changes in his research and theoretical approaches that allowed him to probe more deeply into questions that originally intrigued him about the behavioral effects of reward and nonreward.

Amsel's Theoretical Orientation

Abram Amsel's doctoral work was completed at the University of Iowa in 1948, when learning theory was a major focus of psychology. His professor, Kenneth W. Spence, was an important contributor to one of the influential behavior theories of the time. In the 1930s, Clark L. Hull (at Yale) began to elaborate an S–R theory that was deeply grounded on the earlier work of Pavlov and Thorndike. Spence, a student in Hull's seminars at Yale, was one of many young scientists attracted to this type of theory and, for many years, he worked with Hull in developing the details of what came to be called Hull–Spence theory. By the time Amsel went to Iowa for graduate work, the theory was established and highly influential. It is no surprise, therefore, that he completed his doctoral training with Hull–Spence theory as his guiding orientation. It is, perhaps, notable that for the next 50 years of his career he remained committed both to that same orientation and to a suite of behavioral phenomena for which he provided a theoretical "solution" within the framework of Hull–Spence theory soon after his doctoral work was completed.

At its core, the problem Amsel solved for Hull–Spence theory was how the effects of nonreward should be conceptualized. The original theory had assumed that S–R associations are formed when an organism learns, and that "reinforcement" is necessary for strengthening such associations. Several assumptions were also made about how nonreinforcement might affect learned associations and performance, but these proved to be wanting. For example, there were indications in the experimental literature that the omission of reward, or even a reduction in value of reward, can result in aversive emotional responses for the animal. Hull–Spence theory had no good way to conceptualize either the conditions under which nonreward was expected to have an emotional consequence, or the behavioral effects such a consequence might entail.

Amsel proposed a solution in which the precondition for what he called "frustrative nonreward" was the development of an anticipation of reward through prior experience with reward in the training situation. In everyday terms, "frustrative nonreward" occurs when a reward does not materialize that the animal has come to "expect" in the situation; in the theory's terms, an "expectancy" is a Pavlovian conditioned response. Amsel's key assumption was that an occurrence of aversive frustrative nonreward following an instrumental response has the same theoretical status as does a positive reaction to reward in Hull–Spence theory: both reward *and* frustration should become anticipatory through a Pavlovian conditioning process, and both types of anticipated outcome will operate within the organism to influence instrumental behavior. In this way, he expanded the scope of Hull–Spence theory to phenomena that have come to be termed "paradoxical" reward effects because they involve the violation of reward expectancies and the result that more behavior is obtained for smaller or fewer rewards.

The partial reinforcement extinction effect, the most extensively studied of the paradoxical reward effects, was the phenomenon that triggered Amsel's initial theoretical interest. It is characterized by increased persistence of instrumental behavior in extinction following training with intermixed rewarded and nonrewarded trials that occur in no orderly pattern (in comparison with training solely with rewarded trials). However, the paradoxical reward effects also include the overtraining reward effect (faster extinction following more rewarded training), the magnitude of reward effect (faster extinction after larger reward), and the reward-contrast effects (various short-term effects on instrumental behavior of experience with rewards of different magnitudes).

Amsel first described his theoretical solution at a conference in 1951, a time when the role of traditional learning theory in American psychology began to undergo great change. Amsel's (1951) theoretical ideas were finally published in the *Psychological Bulletin* in 1958, after being rejected by the *Psychological Review*, where one reviewer commented, "This is no time to try to revive Hullian theory." Amsel later noted with satisfaction that, in the 30 years after its publication, his *Bulletin* paper proved to have been that journal's second most highly cited article. It later was designated a "citation classic" by the Institute for Scientific Information. In the next decade, as Amsel and his students explored the effects of nonreward in various experimental situations with rats, his "frustration theory" was further elaborated in additional theoretical papers (Amsel, 1962, 1967).

1948–1968: Research and Theory of Reward-Schedule Effects in Adult Animals

During his first academic appointment at Newcomb College, Tulane University (1948–1960), Amsel and his students carried out several lines of research with adult rats to characterize the effects of nonreward. For example, their demonstration that nonreward can have a short-term invigorating effect on a subsequent instrumental response (the "frustration effect") supported the idea that, under some conditions, nonreward has aversive motivational properties (Amsel & Roussel, 1952). The work at Tulane eventuated in publication of his statement of a "frustrative theory of nonreward," as described above (Amsel, 1958).

In 1960, Amsel joined the Psychology Department at the University of Toronto, where, for a decade, his students conducted many experiments to evaluate various aspects of frustration theory. That work helped expand the theory to include the entire suite of paradoxical reward effects, as well as some aspects of discrimination learning, partial-reinforcement effects within individual animals, and the effects of intermittent schedules of aversive events such as electric shock. Adult rats were used in this work.

This was a productive period for Amsel, and, for his Toronto students, it was a stimulating time to be a member of his laboratory. His frustration theory was in healthy competition with other approaches, particularly that developed by John Capaldi which proposed that the memories of reward and nonreward outcomes, experienced in different sequences, provided a key to understanding some of the behavioral effects for which Amsel's conditioning-grounded theory provided a different explanation. During these years, part of the Toronto lab's emphasis was on testing predictions from the two theories. (It also led to distinguishing some behavioral effects of reward and nonreward schedules that were not "paradoxical," such as the patterned alternation of the rat's fast and slow running speeds on successive trials when there was a patterned alternation of reward and nonreward.) There was a vigorous and quite friendly competition between the two laboratories in journal publications and at conferences where the two approaches might be showcased in back-to-back presentations. In the lab, Amsel often characterized the competition with Capaldi in terms that we imagined were a carryover from his being educated at Iowa in the postwar years. Theoretical "camps," "skirmishes," "attacks," "defeats," and "victories" were all part of the discussion. It seemed that the stakes were high. It was a great motivator, and great fun for the students.

In my view, it was Bob Ross's experiment in the early 1960s that galvanized students in the Toronto lab to view Amsel's theory in a broader perspective (Ross, 1964). Ross was a clinically oriented student who came to the lab to conduct a dissertation that would complete his doctoral work. He used Amsel's theory to make a creative prediction. The theory asserted that internal "frustration" cues, originating from the Pavlovian conditioning of frustration reactions, should become associated with a partially reinforced instrumental response (that assertion was the kernel of Amsel's account of the partial reinforcement effect). Ross reasoned that if he first trained groups of rats to learn a distinctive instrumental response (running, jumping, or climbing) on a partial reinforcement schedule, and later trained them to perform a new instrumental response in a different experimental situation on a continuous reinforcement schedule, even under a changed deprivation condition, the previous

conditioning of aversive frustration cues to the original instrumental response should remain intact, if behaviorally silent. He proposed that when the rats later encounter frustration during extinction of their new continuously rewarded response, the internal aversive cues associated with frustration should be activated in the new situation, and, if the physical arrangement permitted, the rats should then "regress" to performing the response learned earlier in the partial-reinforcement phase of the experiment.

This was a bold and creative prediction, and it was supported in an experiment with adult rats that included all the needed control conditions. Ross proudly had others in the lab come to watch some extinction trials when the rats would jump or climb in the new apparatus, depending on their prior partial-reinforcement training. This experiment left the strong impression on students in the lab that Amsel's theory was clearly on the right track. Also, because Ross's experiment involved successive stages of training, it prompted many subsequent discussions about the possibility that reward-schedule experiences at one stage of life would affect performance at a later stage—a kind of clinical psychology discussion that made the lab feel we were possibly working on processes with real-world implications. Amsel's subsequent work on the ontogeny of paradoxical reward effects in infant rats in the framework of developmental psychobiology includes some echoes of the Ross experiment, and of related work done in the Toronto lab in the 1960s.

1969 to Mid-1980s: Ontogeny of Reward-Schedule Effects—Behavioral Studies

In the late 1960s, Amsel made two important changes that influenced the remainder of his career. In 1969, he moved from the University of Toronto to the University of Texas at Austin. This was shortly after the death there of his mentor, Kenneth Spence, who had been at Austin for only a few years before succumbing to cancer at the age of 59. The other change was in the focus of his research. He began to investigate the possibility that theoretical accounts of the paradoxical reward effects (and some nonparadoxical effects, such as the alternation of running speed to an alternating quantity of reward) might be understood more deeply by studying the ontogeny of those effects in young rats. In particular, he asked whether these much-studied behavioral effects occur full-blown very early in life, or whether they depend on capabilities that emerge during early maturation, such as the capacity to anticipate reward as specified in his frustration theory. Amsel (1992, 1994a) acknowledged that this phase of ontogenetic research in his lab was modeled after M. E. Bitterman's studies of evolutionary divergences in learning.

Amsel and his Texas students promptly began working with young rats. This new line of research posed many technical difficulties, and his laboratory showed ingenuity and persistence in devising effective techniques to study the paradoxical reward effects in the young animals. For example, runways were downscaled and procedures for maintaining behavior of preweanling rat pups on partial reward schedules were developed. All this was hard, but invigorating, work for Amsel and the coterie of very good students attracted to his lab for the ontogeny research.

The first experiment (Chen & Amsel, 1975) studied the partial reinforcement effect in rats whose runway training began when they were young adults (30 days postnatal) or at about the weanling stage (18 days). Different groups at each age received food-pellet reward on a partial or continuous reinforcement schedule. To assess the durability of learned persistence, the experiment included subgroups for which extinction occurred immediately after training or at about 100 days of age when the rats were mature. The partial reinforcement effect occurred when training was given at either age. However, the results also indicated that weanling-trained rats showed greater persistence overall than did the older-trained rats, suggesting that young rats have a high level of persistence. The latter finding, which was repeated in many experiments, ultimately indicated one aspect of infant behavior that was not expected from Amsel's original frustration theory.

This first experiment triggered much additional work on the ontogeny of the partial reinforcement effect, and of other paradoxical reward effects such as those related to large and small reward magnitudes. These subsequent experiments included important procedural refinements and a wider range of training and testing ages for the rats. Innovative procedures were developed to train very young rats, such as those 10 or 11 days old, to traverse a short runway and obtain reward in the form of suckling on a lactating dam. As the lab's work accelerated through the early 1980s, the results showed a pattern in the order of appearance of the various effects that was in line with many, but not all, of the theoretical ideas that Amsel had developed about those effects in research with adult rats. His last book includes a comprehensive summary of this large body of work and its theoretical implications (Amsel, 1992). The general idea that emerges from this work is that, from approximately 10 to 25

days of age, there is a fixed sequence of first appearance of the reward-schedule effects in the rat that indicates increasing levels of functioning.

Specifically, nonparadoxical effects appear initially (Day 11, or earlier): the simple acquisition and extinction of responses; the patterned alternation of response speed to the patterned scheduling of reward and nonreward on successive trials. These kinds of early instrumental learning in the rat can be understood as instances of the simple, reflex-type learning characterized by Pavlov, Thorndike, or Hull's S–R learning theory. Paradoxical reward effects begin to appear later (around Day 14): the partial reinforcement extinction effect (Days 12–14); increased persistence in extinction following training in which the magnitude or delay of reinforcement is varied (Days 16–18); enhanced vigor of locomotion during training when reward occurs on a partial reinforcement schedule (Days 18–20); reduced persistence in extinction after training with high magnitude of reinforcement (Days 20–21); the temporary suppression of responding following an abrupt shift to a reduced magnitude of reward, known as the successive negative contrast effect (Days 25–26). In Amsel's original frustration theory, all the paradoxical effects rely on the rat's capacity to anticipate a reward outcome and to respond emotionally when that anticipation is not met. The theory did not expect that the paradoxical reward effects would appear sequentially; they were assumed to be mediated by a common set of processes. More broadly, the extensive data on these effects with adult rats provided no basis for expecting that this kind of sequential pattern would occur in ontogeny.

Beginning in the 1970s, Amsel's publications reflect a notable shift toward neurological considerations. Indeed, his behavioral ontogenetic data forced consideration of brain development as an organizing principle and guide for his research, rather than the learning-theoretic framework that had guided the first stage of his research and within which he elaborated his frustration theory. The following passage from his last book is explicit (Amsel, 1992, p. 176):

> The hypothesis that has guided our developmental work is that the infant rat is like the hippocampally damaged adult, that the fact of the largely postnatal occurrence in the rat of a significant part of the hippocampal cell development and circuitry offers the investigator a natural animal model for gradually increasing levels not only of hippocampal structure, but also of function, as reflected in our case of the developing paradoxical effects. (Amsel, 1986; Amsel & Stanton, 1980)

Amsel's willingness to adopt a neurobiological perspective in the 1970s was a challenge for someone with such established credentials in the behavioral science community. In a letter written in the mid-1970s, he expressed his feelings about the change in research focus:

> I can sympathize with the difficulties of getting into a new field and more or less giving up an area of expertise. It is difficult to convince people, at least it has been in my case, that a learning theorist can suddenly become a psychobiologist and (for God's sake!) look at brain cells. The result in my case, so far as the practicalities of federal funding are concerned, is that perhaps I should have been safe rather than sorry. *(Abram Amsel to Michael Rashotte, 21 March, 1976)*

This excerpt from Amsel's letter includes a small hint of his good sense of humor, which was often on display in his relaxed times with students and colleagues. Here is another example. As noted above, his research with young rats led him to emphasize "developmental stages" as a powerful factor determining the order of appearance of the reward-schedule effects. At one presentation of his research, a member of the audience commented that Amsel seemed to have become "the Piaget of rats," referring to Jean Piaget's work on stages of cognitive development in children. He greatly enjoyed repeating this story, accompanied by a hearty bout of his distinctive laugh. Actually, the "Piaget" comment is not a bad way to characterize a main contribution of Amsel's research career in its second stage.

Early 1980s–1999: Behavioral Neuroscience of Dispositional Learning and Memory

The passage quoted above from Amsel's last book indicated that his program of behavioral research on the ontogeny of reward effects was guided by literature on postnatal development of neural structures in young rats, particularly the hippocampus. In the early 1980s, his lab began to use invasive techniques designed to damage the hippocampus and, thereby, assess more directly its role in the sequence of reward-schedule effects found at different stages of early development.

In the first of these experiments, an electrolytic lesion was made in the hippocampus of infant and adult rats (Lobaugh, Bootin, & Amsel, 1985). The infants were lesioned about Day 10, at a time when the hippocampus is undergoing rapid development. They received behavioral training shortly afterward, at a stage when the rats can show both the nonparadoxical patterned alternation effect and

the first paradoxical reward effect (the partial reinforcement extinction effect). It turned out that the lesion did not affect patterned alternation behavior, but that the partial reinforcement extinction effect was eliminated. The same result was found in the adults. The partial reinforcement effect failed to occur in young or older rats with hippocampal lesions because extinction after training with continuous reinforcement did not show the usual rapid decrease in responding that is thought to reflect aversive frustrative reactions to nonreward. The implication was that lesioned animals were functioning at the stage of 11-day-olds, being able to show the nonparadoxical patterned alternation behavior but not the partial reinforcement extinction effect. This first lesion study pointed to a differential role of hippocampal involvement in the nonparadoxical and the paradoxical reward effects. Possibly, a functional hippocampus is needed for development of a reward expectancy (which frustration theory asserts is the necessary precondition for nonreward to result in an aversive emotional reaction). Later lesion studies in the lab provided many more details, including the finding that lesioned rats *would* show a deficit in the nonparadoxical patterned-alternation task if the intertrial interval was lengthened. The latter result indicated an effect of the lesions on memory.

The findings of the first lesion experiment were pursued in an active program of research using invasive procedures to probe the role of the hippocampus in reward-schedule effects during early development. In addition to lesions, the lab utilized other invasive techniques intended to damage the hippocampus and affect neural development. These techniques included pre- and postnatal exposure to ethanol, early postnatal exposure to X-irradiation, and postnatal treatment with an NMDA receptor/channel blocker (MK-801). Some of the lab's papers from the late 1990s included no behavioral testing, but reported the outcome of neuroanatomical investigations showing the effects of the various treatments at different ages. Such papers, coming very late in Amsel's research career, indicate another striking, but highly systematic, evolutionary step in his research program. The invasive research done through the early 1990s is summarized in his final book and in a later précis he wrote based on that book (Amsel, 1992, 1994a). Research papers from his lab continued to appear in the literature into the early 2000s (e.g., Nixon, Hughes, Amsel & Leslie, 2004).

There was another important change in this stage of Amsel's career that had to do with the way in which he conceptualized his research work and frustration theory itself. Those of us who trained with him in the 1960s, when frustration theory was taught in the language of Hull–Spence theory, were jolted to see the title of the book in which he summed up his career's work, *Frustration Theory: An Analysis of Dispositional Learning and Memory* (Amsel, 1992). In the preface of that book he described this new perspective as follows:

> The experimental animal of choice is overwhelmingly *Rattus norvegicus*, and its study is designed to further the comprehension of what I have called dispositional learning and memory—systems that ordinarily have a long-term historical etiology and in which learning is relatively reflexive and memory implicit and not strongly episodic (Amsel, 1992, p. vii)

A passage that he wrote later, in response to comments on his précis of the book, adds this:

> I am *not* interested in the rat's cognitive abilities; in fact, I am not, as most investigators are not, interested in the rat, except as a model system of noncognitive, dispositional function, the kind of function that exists in humans—nonverbally, implicitly rather than explicitly. If one wants to study cognitive function, the rat may not be the worst of all possible subjects, but it is surely not even close to being the best. That is why I find the rat a reasonable subject for the study of the development of dispositions; of reward-schedule effects that do not depend primarily on particular sequences [of reward and nonreward]; of transfer of persistence effects over days and months with interpolated phases of continuous reinforcement. (Amsel, 1994b, p. 330).

Can these passages have been written by the same Abram Amsel known for his strong commitment to traditional learning theory? That question is examined below.

Amsel As a Standard Bearer for S–R Learning Theory

Amsel was known to complain, in person and in print, that S–R learning theory is often described opportunistically—without proper nuance, as a straw man that can be dismissed easily in favor of a new approach. His complaint was usually directed at parts of the animal cognition and the constraints-on-learning literature, both of which had became popular in the wave of changes in psychology that began in the 1960s. Amsel acknowledged that such characterizations could be seen as a backhanded compliment: It was Clark Hull's S–R theory that usually came in for such treatment, thereby indicating a significant role for that theory. But, it was the apparent willingness of authors to throw out the

S–R baby with the S–R bathwater that rankled him most. For years, he was particularly vigilant about omissions, errors, or perceived slights in characterizations of S–R learning theory that appeared in the literature, and he corresponded with many authors on these matters, often in a feisty manner. (His professional correspondence, now housed in the Archives of the History of American Psychology at the University of Akron, should provide historians with a detailed portrait of his enduring commitment to accuracy and completeness in scientific writing about the history of learning theory.)

This aspect of Amsel's behavior could easily leave the impression that he was stuck in a part of psychology's past, a standard bearer for what was sometimes called "traditional learning theory," long superseded by new theoretical approaches and research problems. Indeed, he could easily irritate colleagues with his remarks on this issue, and he likely turned off interest in his work in some quarters by making such remarks. But any impression that he was a "dinosaur," in the sense of his theoretical commitment and research approach, is not supported by the facts of his career. As I have noted above, his own work demonstrated flexibility in ideas that guided his research (developmental neurobiology came to replace behavior theory as a source of his hypotheses), in his willingness to utilize new methodologies to address the questions that interested him (procedures from developmental research and neuroscience were added to his behavioral methods), and in the way he framed his work (the Hull–Spence frustration theory of his beginnings was subsumed under the "dispositional learning and memory" of his endings).

I think the answer to this paradoxical aspect of Amsel's reputation lies in the following considerations. It is true that his writings included a strong respect for the old, as well as for the new, when both contributed to the problem of interest to him. For example, in describing his thinking and research about reward schedule effects over his long career, he did not shy from including a full account of the early language and diagrams of frustration theory, although he acknowledged some concern about doing that:

> The courage even to contemplate writing a book such as this one—to spread "old fashioned" Ss and Rs across the pages of a volume in the face of the steamrolling cognitive revolution—came to me in the serenity of the rustic setting of the Center for Advanced Study in the Behavioral Sciences at Stanford. (A major reason for proceeding with this task was the hope that it might strengthen the resolve of, or at least express solidarity with, those who think this level of analysis of behavior is useful and even important.) (Amsel, 1992, pp. vii–viii)

And it is also true that he became entirely comfortable in explaining that the behaviors of interest to frustration theory are nonverbal behaviors that, in cognitive psychology, are "a kind of 'memory' that requires many exposures or trials in which awareness plays little or no part—memory without record" (Amsel, 1994a, p. 281). There is really no contradiction here. In Amsel's view, the level of behavior that interested him for research was one in which both the concepts of frustration theory and the concepts of dispositional learning and memory can be useful. In this sense, he was a thoughtful advocate of keeping a proper place for the older S–R psychology at the table of modern theoretical approaches to behavior.

Amsel As Teacher and Mentor

He was called "Dr. Amsel" by the many graduate and undergraduate students who worked in his laboratory ("Abe" after graduation). His laboratory proved to be a superb place to learn theory (and, by definition, an appreciation of history), research design, research techniques of many sorts, data analysis, writing—the skills needed to be a good scientist. In lab meetings, Amsel brought a sense of high integrity, thoughtfulness, and a willingness to consider proposals for new directions in the experimental work. Experimental plans and ongoing results were proposed and intensely discussed in group meetings, and many papers had multiple student coauthors, reflecting the collaborations among students excited by a direction of research. Of course, Amsel had his "grumpy" days too, which students assumed were related to Departmental or professional events that were beyond their grasp. On such days, students trod rather lightly in their demeanor and comments. For example, most of Amsel's research at Toronto was carried out with rats in runway apparatuses, constructed to his own specifications, which provided convenience and flexibility in the running of large numbers of animals and in rapidly changing the experimental conditions. One day in the early 1960s, the students were recruited to tour a prospective new faculty member through the lab. After our last runway apparatus was proudly shown, this fellow, newly trained in a premier operant research program, spontaneously blurted out: "This is straight out of Wilhelm Wundt!" The students decided that it would be best not to report that comment to Amsel.

During graduate seminars, students learned to watch Amsel's body language for signs that a presentation might be in difficulty. One sure sign of trouble was when he leaned forward, with arms on the table, and raised one hand to cover his mouth. When that hand gradually moved up to cover his eyes, and when he began to look out with one eye between two opened fingers, it was clear that the presentation would be followed by comments that were cool (in the old fashioned sense of that term).

Key figures in the field visited the Amsel lab, and students at all levels of advancement were involved in the ensuing scholarly discussions. Such visits often included social events at restaurants or, most enviably, at the Amsel home where he and his gracious wife, Tess, warmly welcomed the students to evenings of memorable socializing, cuisine and music. For many students, these parts of life in the Amsel lab were models for future behavior.

Amsel provided first-rate training in editorial commentary. When a draft of a paper or thesis was returned, it was typically marked by a welter of penciled editorial symbols and substantive suggestions for revision. When an Amsel student's master's thesis or dissertation was deemed ready for the public defense, the student felt entirely ready to go it alone with the examining committee. In the defense, Amsel usually let the student do the talking, but he became fiercely supportive when needed. One student recalled how Amsel helped her in the opening moments: "When I went into the room to defend my dissertation with all those men's eyes staring at me, I, who am naturally anxious by temperament, was terrified. The first question someone asked me was: What is the nature of reality? I froze. I couldn't answer, think, or move. Abe stood up and very tactfully but firmly told the questioner that the question was inappropriate. I started to relax, as much as one can in that situation, and I was able to successfully defend my dissertation." Another student noted that, part way through his defense, a committee member began a line of aggressive questioning that asked why the dissertation used such sloppy terminology as "frustration." The candidate struggled until Amsel heatedly chimed in to inquire how the questioner could pursue this line of inquiry when he worked in the field of signal detection theory where a key term was "false alarm." Before the questioner could answer, Amsel called a 10-min halt to the defense, took the candidate into his nearby office and vented about "grandstanding" by some faculty. The defense resumed after the delay, without further incident.

In a different context, Amsel often expressed concern that his theory did not get a fair shake. He commented in print that difficulties arose in some cases when grant proposals were cast in terms of "frustration theory" (Amsel, 1994b, p. 331), noting that some laboratories had stopped using the term "frustrative nonreward" in favor of "aversive nonreward." This change in terminology was seemingly in response to comments from grant study sections that did not favor "frustration" as an acceptable term, no matter how well defined that term might be in experimental operations. Although Amsel himself was well funded by federal granting agencies throughout his long career, it grated on him that animal research on such topics as "superstitious behavior," "prisoners' dilemma," and "self-control" seemed to escape the negative scrutiny that he thought "frustration" attracted.

On the occasion of Amsel's 70th birthday, many of his former students came to Austin to celebrate with him and Tess, and with their family. A day of presentations revealed the diverse career paths chosen by his students after leaving his lab—clinical psychology, behavioral research, behavioral neuroscience, industry. Even some students who had worked in his lab as undergraduates at Toronto came to the event. It was a very nice gathering of Amselians.

After I graduated from Amsel's lab and had begun working in a different area of research, he asked me if I would like to join him as coauthor on a historical chapter concerning Clark Hull's work. Knowing Amsel's deep connections to Hull, and realizing that I might learn a great deal from such a project, I agreed. For several years in the late 1970s and early 1980s, we collaborated by letter and phone. The most significant outcome of that collaboration was a book that appeared in 1984, the centennial of Clark Hull's birth (Amsel & Rashotte, 1984). That book began as a project to collect and reprint Hull's 21 theoretical papers that had appeared in the *Psychological Review* from 1929 to 1950. As the project developed, we ended up preparing commentaries on those papers that were included in the book. Many of Hull's papers were devoted to topics that would be characterized as problems in cognitive psychology today, and we argued that a proper appreciation of Hull's contributions would include those theoretical ideas, as well as others found in his many papers. In reencountering Amsel in a writing project more than a decade after my graduation, I was reminded again of many of the excellent characteristics he displayed as a scientist, and of the great respect his students had for his breadth of historical knowledge in learning theory. When we worked together on this project in Austin, I was also reminded of the warmth with which he and Tess had treated his students.

Amsel As Journal Editor

In the 1970s, Amsel took on the duty of Editor for two journals published by the Psychonomic Society. One was a newly segmented section of the journal *Psychonomic Science*. Amsel and Richard F. Thompson were joint editors of *Psychonomic Science: Section on Animal and Physiological Psychology*, which was published for two years (1971–1972). Beginning in 1972, however, the Society fielded an array of separately named journals targeted toward subsections of the field, and Amsel became the founding Editor (1972–1976) of *Animal Learning & Behavior*.

Amsel's first statement of his editorial policy for *Animal Learning & Behavior* expressed an inclusive and interactive view of the papers that he wished to publish. Coming from such an apparently committed Hull–Spence theorist, this may have been a surprise to some. But, behind the single-mindedness of his own approach, Amsel was intellectually broad in his understanding of how psychology was configured. He was partially influenced in this regard by his admiration for the entire body of Clark Hull's work, which he often remarked was highly inclusive and respectful of the historical scope of research and theory that had gone before. Amsel's first editorial in *Animal Learning & Behavior* included this statement:

> This is not a highly specialized journal; and the fact that it is not makes it reasonable to expect, and provides an opportunity for, increased communication among, say, investigators whose work is with Pavlovian conditioning, operant conditioning, and other paradigms for the study of learning and motivation, as well as investigators of animal development and animal behavior from a number of different orientations. All share an interest in the learning and behavior of nonhuman, as well as human, animals, and each has something to contribute to at least some of the others. We sense that some breaking down of intellectual and terminological barriers in these and other areas already has been accomplished, and feel that an acceleration of this process is desirable. (Amsel, 1973, p. 1)

Thirty years later, the present journal's statement of editorial policy continues to endorse his first Editorial as an orienting position.

Abram Amsel: Biographical Sketch

Abram Amsel was born in Montréal, Québec, December 4, 1922, the son of Aaron and Annie Levitt Amsel. He received his higher education in Canada (BA, Queen's University, 1944; MA, McGill University, 1946), and at the University of Iowa (PhD, 1948). At McGill, he worked under the direction of Robert Malmo; at Iowa, under Kenneth W. Spence. He became a naturalized citizen of the United States in 1957.

Amsel's first academic position (1948–1960) was at Newcomb College, Tulane University, where he rose from assistant professor to professor of psychology. He joined the psychology faculty of the University of Toronto as professor in 1960, at a time when that department was undergoing a significant expansion in experimental psychology. Along with Endel Tulving, Daniel Berlyne, George Mandler, and others, Amsel helped guide the Toronto department to a new level of prominence on the international scene. In 1969, he joined the psychology faculty at the University of Texas, Austin, where he remained until his retirement in 1999.

During his long academic career, Amsel had several temporary appointments at other institutions, including University College London (1966–1967), the University of Pennsylvania (1974–1975), and the Center for the Advanced Study in the Behavioral Sciences (1986–1987). His research was supported by the National Science Foundation, National Research Council of Canada, National Institute of Mental Health, National Institute of Child Health and Human Development, National Institute on Alcohol Abuse and Alcoholism, and the Hogg Foundation for Mental Health.

He was elected to membership in the Society of Experimental Psychologists (1965), which, in 1980, awarded him the Howard Crosby Warren Medal for Outstanding Research. In 1992, he was elected to the National Academy of Sciences. He was also a Fellow of the American Association for the Advancement of Science and of the American Psychological Society, and he held memberships in the Society for Neuroscience, the International Society for Developmental Psychology, the International Brain Research Organization, and the Pavlovian Society.

Amsel served on the Governing Board of the Psychonomic Society (1973–1978). In 1992, he was selected as the University of Iowa's Distinguished Graduate, and in 1994 as a Distinguished Graduate of McGill University's Department of Psychiatry. In addition to his editorial positions for journals of the Psychonomic Society noted above, he served as consulting editor for the *Journal of Experimen-*

tal Psychology (1964–1969) and as a member of the Editorial Board of the *International Journal of Psychophysiology* (1982–1988).

Abram Amsel died of Alzheimer's disease in Austin, Texas, on August 31, 2006. He is survived by his wife of 59 years, Tess Steinbach Amsel, three children (Steven David, Andrew Jay, Geoffrey Neal), their spouses, and several grandchildren.

He is also survived by a large number of undergraduate and graduate students who experienced his dedicated approach in the classroom and the laboratory, and who learned much from his expertise in experimental design, research methods, and theoretical analysis. They also benefited from his skillful editorial pen, with which he helped them improve numerous drafts of manuscripts and theses during his 50 years in the laboratory. Finally, they learned the importance of perspective and accuracy in portraying the history of a field.

Acknowledgement: I thank Darryl Bruce and Michael Domjan for comments on an earlier version of this contribution.

Michael E. Rashotte
Emeritus Professor of Psychology & Neuroscience
Florida State University
(*PhD 1966, University of Toronto*)

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