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The Garcia-Koelling Selective Association Effect:
A Historical and Personal Perspective

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The first and most prominent study of selective associations was the so-called bright-noisy-water experiment by Garcia and Koelling (1966). This study was a landmark in the development of thinking about biological constraints on learning and remains the most highly cited study of selective associations, even though it lacked important controls. I first describe the original experiment and initial criticisms of it. I then discuss the various control issues that were ignored in the original experiment but addressed in subsequent research. In this account, I rely primarily on research conducted in my laboratory, because the problems have not been addressed by any other investigator. Along the way, I discuss the discovery of a selective sensitization effect related to the Garcia-Koelling findings, ways to rule out selective sensitization, and studies of selective associations in pre-weanling rats. I conclude with a look back at the impact of the Garcia-Koelling experiment and recommendations for new generations of students in the field.

Biological constraints on learning became a major concern of learning psychologists during the 1960's in response to a series of phenomena that challenged the validity of general-process learning theory. The central claim of general-process learning theory was that learning phenomena and principles were universal and therefore could be discovered by studying learning in any *standard* learning preparation, such as lever-pressing in rats or key-pecking in pigeons.

The first of the major challenges to the general process approach emerged from the work of Keller and Marian Breland, former students of B. F. Skinner, who stepped outside the bounds of the proverbial Skinner box by using instrumental conditioning to train raccoons, ducks, chickens, and piglets for amusement park displays. During the course of their work, they identified numerous ways in which the various species they tried to train violated basic principles of operant conditioning. Their observations were first reported in an article in the American Psychologist titled, "The Misbehavior of Organisms" (Breland & Breland, 1961), which was a take-off on Skinner's seminal volume, *The Behavior of Organisms* (Skinner, 1938).

The examples of *misbehavior* described by the Brelands were soon followed by various other forms of *misbehavior* reported by other investigators. These were

compiled and analyzed in two major edited volumes, *Biological Boundaries of Learning* (edited by Seligman & Hager, 1972) and *Constraints on Learning* (edited by Hinde & Stevenson-Hinde, 1973), as well as three highly influential review papers (Rozin & Kalat, 1971; Seligman, 1970; Shettleworth, 1972). Thus, about a decade after the initial report by the Brelands, the idea was well established that learning was subject to significant *biological constraints*.

One of the major contributors to the biological constraints movement was John Garcia, who, like the Brelands studied learning phenomena outside the range of common learning paradigms. Garcia got involved in a project whose goal was to document the biological effects of low doses of radiation. He and his colleagues soon encountered a major problem, namely that exposure to low doses of radiation did not seem to have any major effects on a range of physiological and behavioral measures. However, it seemed to depress drinking. But this only occurred when the rats were in the radiation chamber drinking from a plastic bottle rather than in their home cage drinking from a glass bottle. This suggested that a subtle difference in the flavor of the water might be the critical factor. In pursuit of that hypothesis, Garcia and his colleagues gave the rats water that they explicitly flavored with saccharin in the radiation chamber. Under those circumstances a major suppression of drinking occurred. This observation, and a long progression of refinements of the experiment, led to Garcia's establishment of what we now know as the conditioned taste aversion learning paradigm (Garcia, Kimeldorf, & Koelling, 1955). (For a more detailed account of this history, see Freeman & Riley, 2009.)

Garcia contributed two major phenomena to the development of the biological constraints movement. One of these was the phenomenon of selective associations (Garcia & Koelling, 1966) and the other was his discovery of long-delay taste aversion learning (Garcia, Ervin, & Koelling, 1966). These phenomena had a huge impact on discussions of biological constraints on learning. The discoveries were made with laboratory rats and thus could not be dismissed on the grounds that unconventional species were being tested. The phenomena provided insights into major biological problems such as food selection and specific hungers (Rozin & Kalat, 1971). Although vigorous criticisms were voiced, none has stood the test of time. Selective associations and long-delay learning are now well established and well accepted learning phenomena, routinely included in introductory psychology and other textbooks.

Of these two phenomena, the selective association effect is by far the most important one in forcing us to think about constraints on general-process learning theory. Although long-delay taste-aversion learning is well established, learning with delays of 4-6 hours between the conditioned stimulus (CS) and the unconditioned stimulus (US) has not been found in other situations. Thus, one can treat long-delay taste-aversion learning as an exception to general-process learning. Because of its double-dissociation design, the phenomenon of selective associations is not as easily dismissed. Therefore, my remaining remarks will focus on the selective association effect.

The Bright-Noisy-Water Experiment

The experiment that launched the study of selective associations (Garcia & Koelling, 1966) has come to be called the *bright-noisy-water experiment* because the audiovisual cue that was used as one of the conditioned stimuli was activated by having the rats lick a drinking spout. During the conditioning trials, the drinking spout contained water that was mildly flavored with saccharin. Thus, it would be more accurate to call this the *bright-noisy-saccharin experiment*. One group of rats was made sick with either an injection of lithium or radiation after exposure to the bright-noisy-saccharin, whereas another group received foot shock. After a number of conditioning trials, the rats were tested separately with the audiovisual cue and the taste cue. The reported results are now highly familiar. The shocked rats showed much more of an aversion to the audiovisual cue than the taste cue. In contrast, the illness rats showed the reverse, a much stronger aversion to the taste cue than to the audiovisual cue.

The Garcia-Koelling experiment had two exemplary features and one serious shortcoming. The first exemplary feature was that both the audiovisual CS and the taste CS were presented in the same fashion, as a consequence of licking a drinking spout. Thus, one could not argue that one CS was presented passively (as is common with audiovisual cues) whereas the other CS was presented actively (as is common with taste cues). This control for the method of CS presentation was not as well carried out in a subsequent experiment by Garcia, McGowan, Ervin, and Koelling (1968), in which the CSs were taste and the size of food pellets. In the 1968 experiment, the rats received the visual CS in a more passive manner than the taste CS. However, that was probably not a critical issue, since Domjan and Wilson (1972) subsequently showed that a robust selective association effect occurs with passive presentation of both taste and non-taste conditioned stimuli.

Ironically, the 1966 Garcia-Koelling study that reported the phenomenon of selective associations for the first time was published in *Psychonomic Science,* whereas the less well controlled 1968 Garcia et al. experiment was published in *Science*, which has always been a much more highly regarded publication. I never heard John Garcia talk about the 1968 paper, but he complained bitterly that the 1966 paper was initially rejected by a more prestigious journal.

The second outstanding feature of the original Garcia-Koelling experiment was that it involved a double dissociation design, showing a reversal in the strength of aversion learning to different types of cues as a function of the type of unconditioned stimulus (US) that was used. This is a critical feature for all studies of selective associations. To my knowledge, no prior study of a biological constraint (by the Brelands or anyone else) involved a convincing double-dissociation design. John Garcia deserves a great deal of credit for that. He set the standard, which has since been followed by all investigators who aim to demonstrate a selective association effect.

The advantage of the double-dissociation design is that the strength of aversion learning cannot be attributed to either the CS or the US that participates in an association. One cannot argue that tastes are generally more easily conditioned or more salient than audiovisual cues or that illness is a stronger US than foot-shock. Either of these hypotheses would not predict the crossover in the relative strength of aversions to the two types of CSs as one moves from using one type of US to the other.

The double-dissociation design, and the Garcia-Koelling results have a symmetry about them that has often gone unrecognized. People often talk about the selective association effect as showing something special about taste–illness learning. However, because of the symmetry of the design, the selective association effect also shows something special about learning aversions to audiovisual cues paired with shock. Thus, if one regards the selective association effect as showing a biological constraint in poison-avoidance learning, we should also conclude that it shows a biological constraint in fear conditioning. (Others have subsequently demonstrated that rats can learn an aversion to taste paired with shock, but it is important to keep in mind that such taste-shock learning was obtained under conditions very different from those employed in the selective association experiments.)

Proper Controls for Selective Associations

Although the Garcia-Koelling study was exemplary in having a double-dissociation design and in dealing with possible confounds due to the method of CS presentation, it fell short on another dimension – a shortcoming that subsequently generated extensive debate. The experiments lacked any control groups. Both the 1966 study and the 1968 *Science* paper included only groups of rats that receive paired presentations of the conditioned and unconditioned stimuli. As a consequence, the results did not show what baseline responding would have looked like in the absence of any aversion conditioning. The results also did not indicate whether the aversions were due to a CS-US association or to sensitization or pseudo-conditioning effects of exposure to the shock and illness unconditioned stimuli.

In my initial foray into the study of selective associations (Domjan & Wilson, 1972), our experiments replicated the Garcia-Koelling double dissociation design, but also included saline-injected control groups that allowed us to measure baseline responses to the conditioned stimuli in the absence of any aversion conditioning. This did not satisfy the critics. One of the most outspoken skeptics was M. E. Bitterman, who launched a vigorous attack on the field of taste-aversion learning in a review of the comparative study of learning that appeared in *Science* about 10 years after the Garcia-Koelling discovery (Bitterman, 1975). By that time, John Garcia had gained a great deal of notoriety and respect, and he did not take Bitterman's criticisms lying down. A spirited exchange was subsequently published in *Science,* with Garcia and his colleagues taking on Bitterman's criticisms of lack of proper controls in his taste aversion experiments (Bitterman, 1976; Garcia, Hankins, & Rusiniak, 1976).

As a relatively young Assistant Professor, I was pleased to have the Domjan and Wilson (1972) study cited by both Bitterman and Garcia in their public battle. At the time, I thought Bitterman was misguided because he failed to properly credit the non-conditioned control groups that we had included in the Domjan-Wilson experiments. I favored Garcia's side of the argument because at the time I did not properly appreciate how sensitization and pseudoconditioning might have contributed to the results. Garcia no doubt understood those issues better than I did, but he had little respect for them. In his acceptance speech for the Distinguished Scientific Contributions Award from the American Psychological Association, he included a section titled "Pseudoconditioning and Pseudocriticism" and wrote, "I am sensitive about sensitization, and I am sick of pseudoconditioning" (Garcia, 1981, p. 152).

I did not understand why a non-trained saline control group was insufficient for a demonstration of selective associations until I read a non-combative and well-reasoned discussion of the topic by Rescorla and Holland (1976). In that review, they raised the possibility that selective associations are observed because different unconditioned stimuli induce different types of selective attention or orientation. Shock may activate differential attention or sensitivity to audiovisual cues and illness may produce a differential sensitivity to taste cues. This proposition made a lot of sense to me because I had been studying taste neophobia or the tendency of rats to avoid ingesting something because of the novelty of its flavor. A major component on my research was my discovery of poison-induced neophobia (Domjan, 1977). I found that rats that have been made sick displayed much more flavor neophobia than saline-injected controls. However, the enhanced neophobia dissipated as the rats recovered from the illness.

Demonstration of Selective Sensitization

The phenomenon of poison-induced neophobia seemed exactly what Rescorla and Holland were talking about when they suggested that a US can induce selective attention or sensitivity to certain types of cues. However, I had only been studying the sensitizing effects of illness on taste reactivity. This is just one of the four cells in a full double dissociation design. Missing were examinations of the effects of illness on reactivity to audiovisual cues, and the effects of shock on reactivity to taste and audiovisual cues.

There are two approaches to dealing with the selective sensitization hypothesis of Rescorla and Holland. One is to see if such selective sensitization effects actually exist. The other is to test experimental designs in which such effects cannot account for the results. Van Miller and I decided to pursue both approaches.

To determine whether foot-shock caused sensitization to auditory and visual cues, Miller and Domjan (1981a) first habituated rats to drinking from a spout that activated either a noise or a light stimulus. Half the rats were made familiar with the noise cue, whereas the others were made familiar with the visual cue. After this familiarization phase, the rats were given foot-shock and tested for their preference in a chamber that had two drinking spouts. Licks at one of the spouts activated the light stimulus and licks of the other spout activated the noise. We found that shocked rats showed an aversion to whichever exteroceptive cue was novel for them (noise habituated rats avoided the light and vice versa). Thus, shock did create increased reactivity to exteroceptive cues. However, this effect was observed only if the shock occurred immediately before the test session. If the shock was administered 5 min earlier, sensitization did not occur.

In another experiment, Miller and Domjan (1981a) demonstrated that lithium-induced illness sensitized rats to the taste of a novel saccharin solution. Rats showed a lower preference for drinking saccharin compared with familiar water if they had been injected with lithium 35 min earlier. However, an injection 6 hours earlier did not produce the effect.

In their third and most important experiment, Miller and Domjan (1981a) compared reactivity to exteroceptive and taste cues following shock (given immediately

before the test) and illness (induced by lithium injected 35 min before the test). This experiment showed that shock sensitizes reactivity to visual and auditory cues but not to taste whereas illness sensitizes reactivity to taste but not to auditory and visual cues. Thus, Rescorla and Holland were correct in thinking that there may be a selective sensitization effect related to the Garcia-Koelling phenomenon. However, it is important to keep in mind that the sensitization effects identified by Miller and Domjan (1981a) were short-lasting and could not be detected six hours after either unconditioned stimulus.

Selective Associations with One Conditioning Trial

The only way that the kind of short-lasting selective sensitization discovered by Miller and Domjan (1981a) could produce the Garcia-Koelling effect is if conditioning was conducted with multiple conditioning trials spaced reasonably close together. Under those circumstances, the US presented on one trial could influence reactivity or attention to the CS that occurred on the next trial. A selective sensitization effect could then produce selective associations. One way to rule out this possibility is to test for selective association learning in a one-trial learning experiment. Miller and Domjan (1981b) conducted a series of such one-trial conditioning experiments.

It is interesting to note that it took until 1981 for anyone to demonstrate selective associations in a one-trial learning experiment. After all, we all knew that taste aversions could be learned in a single trial. Fear conditioning with one trial may have been less familiar but not unheard of. Nevertheless, as of 1981, the Garcia-Koelling effect had only been reported in three published papers (Domjan & Wilson, 1972; Garcia & Koelling, 1966; Garcia et al., 1968), and each of those studies employed multiple conditioning trials.

To make a long story short, Miller and Domjan (1981b) and Miller (1984) replicated the Garcia-Koelling effect, showing robust selective associations, with just one conditioning trial in numerous experiments. These studies put to rest any concern that selective sensitization produced on one conditioning trial might have influenced what cues the subjects were paying attention to on subsequent conditioning trials.

Equating Exposure to USs in Selective Association Experiments

As gratified as we were to see robust selective associations in just one conditioning trial, those experiments left open the possibility that the US presented during the conditioning trial might have biased attention or responding to the taste or audiovisual cues during the subsequent test trial. To rule out that possibility we employed an experimental design advocated by Rescorla and Holland. They suggested that the best way to control for differential sensitization effects is to present both shock and illness to all of the participants, but pair the CSs with just one of those USs.

The experimental design that controls for selective associations by presenting both USs is outlined in Figure 1. Six groups of rats were tested. During the single conditioning trial, the rats were allowed to lick a drinking spout that produced either a flash of light or the taste of saccharin. Exposure to this CS was then followed by an

injection of lithium chloride (LiCl) to induce illness, footshock to create peripheral pain, or a saline injection (which constituted a non-conditioned control). One day after the conditioning trial, animals previously shocked were given a lithium injection and animals previously injected with lithium were administered shock. A series of context extinction sessions were then conducted to eliminate any possible contribution of context conditioning. Finally, on Day 6, a test session was conducted in which the rats could drink either from a spout that produced their CS or from a spout that provided plain water.

Group	Day 1	Day 2	Days 3, 4, 5	Day 6
1	Light ⬜ LiCl	Shock	Ctx Ext	Light Test
2	Light ⬜ Shock	LiCl	Ctx Ext	Light Test
3	Light ⬜ NaCl	NaCl	Ctx Ext	Light Test
4	Sacc ⬜ LiCl	Shock	Ctx Ext	Sacc Test
5	Sacc ⬜ Shock	LiCl	Ctx Ext	Sacc Test
6	Sacc ⬜ NaCl	NaCl	Ctx Ext	Sacc Test

Figure 1. Design of Experiment 3 by Miller and Domjan (1981b).

The results, presented in Figure 2, showed a robust selective association effect. As expected there was virtually complete suppression of intake for the visual cue that had been paired with shock and the taste that had been paired with illness. In contrast, there was no evidence of learning for with taste paired with shock and visual cue paired with illness. These results convincingly rule out the possible role of pseudoconditioning or selective sensitization in the Garcia-Koelling effect.
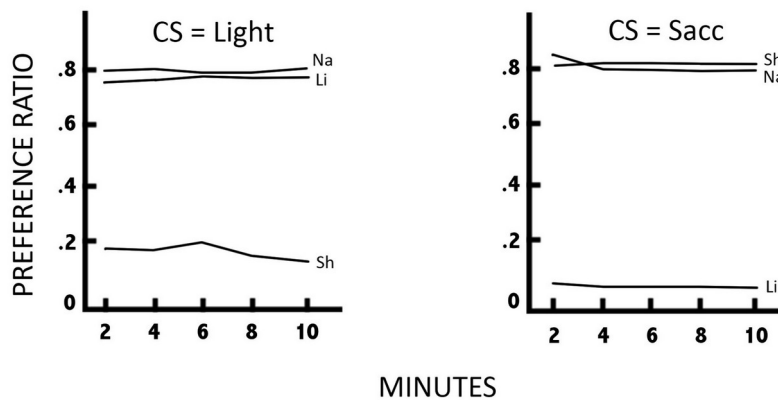
Figure 2. Results of Experiment 3 by Miller and Domjan (1981b).

The strategy of equating all conditioned groups in terms of their history of exposure to shock and illness USs was employed in three experiments by Miller and Domjan (1981b) and two additional experiments by Miller (1984). Thus, demonstration of a selective association effect under these circumstances is now well established.

Response Specificity of the Selective Association Effect

Starting with the original Garcia-Koelling study, in all experiments on selective associations learning was measured by recording either the amount ingested or the rate of licking a drinking spout. For taste tests, the drinking spout provided water flavored with saccharin. For tests of response to exteroceptive cues, the drinking spout contained water and contacts with it activated an auditory and/or visual stimulus. Van Miller (1984), a doctoral student in my lab, inquired whether the selective association effect would be evident if non-ingestive responses were measured.

In his first experiment, Miller (1984) found that a robust selective association effect is evident not only in suppression of drinking but also in how much time rats spend near the location of a drinking spout after it is removed. Illness-conditioned rats avoid the location of the spout if licks previously provided the taste CS but not if licks previously activated a light CS. In contrast, shock-conditioned rats avoid the location of the spout if licks previously activated the light but not if licks previously provided the taste of saccharin.

In his second experiment, Miller (1984) departed entirely from the drinking spout methodology by presenting the taste passively through an oral cannula during both conditioning and test trials. The comparison exteroceptive cue was pulsed white noise, also presented independently of behavior. Following conditioning, Miller measured a wide range of responses during oral infusions of saccharin and presentations of the white noise. Rats previously conditioned with shock showed increased freezing and decreased rearing when tested with the auditory CS. These responses were not evident in illness-conditioned rats. Illness-conditioned rats showed increased gapes, chin wipes, and headshakes when tested with the taste CS, but these responses were not evident in shock-conditioned rats.

Miller's (1984) experiments demonstrate that the basic selective association effect does not reflect a constraint on how associations are manifest in behavior. A variety of response measures show the same results. This suggests that the Garcia-Koelling effect is indeed a reflection of selectivity in the underlying association that is learned and not due to performance artifacts.

Developmental Perspectives on Selective Associations

Another argument that was raised concerning the Garcia-Koelling effect is that it reflects the product of ontogenetic experience (Testa, 1974). All of the studies I have described so far were conducted with adult rats. By that age, the animals no doubt had numerous learning experiences that could have biased how they formed new associations. It is possible that during the course of growing to adulthood, the rats encountered stronger correlations between taste and illness episodes than between taste and cutaneous pain. After all, each instance of eating and drinking involves

orosensory cues followed by interoceptive postingestional consequences that are unrelated to exteroceptive stimuli the animals might encounter at the time. Correspondingly, the auditory and visual cues they are exposed to are probably better correlated with other exteroceptive events than with illness. Such selective correlations may be responsible for the selective association effects that are observed with adult rats.

There are two ways to test the past experience hypothesis. One is to alter the rearing environment of the rats, so to make all forms of cue-consequence relations equally possible. Unfortunately, that is not easy to accomplish. Another, more tractable approach, is to see if selective associations are evident early in life, before much ontogenetic experience has taken place. We opted to pursue this second strategy. This line of research was inspired by the work of Abram Amsel and others who had great success in the 1970's and 1980's in developing learning procedures that could be successfully carried out with pre-weanling rats.

We began with a series of studies of long-delay taste aversion learning in pre-weanling rats (Gregg, Kittrell, Domjan, & Amsel, 1978), but, of course, long-delay taste aversion learning is just one cell of the 2 x 2 design required to demonstrate selective associations. Our first attempt to demonstrate selective associations was conducted in 5-day-old rats (Gemberling, Domjan, & Amsel, 1980). To provide the taste CS, a saccharin solution was infused into the oral cavity of the rat pups. Since rats at this age do not have their eyes or ears open yet, to provide an exteroceptive CS, we placed the pups on a smooth cardboard surface. Different groups received cutaneous shock or an injection of lithium chloride as the US. Aversion to the taste was tested by measuring how much of an infused saccharin solution the pups swallowed. Aversion to the cardboard tactile cue was measured by placing a rat pup in the middle of a chamber that had cardboard on one side and terrycloth on the other. We could then measure preference (or aversion) by how much time the pup spent on the cardboard surface.

A robust selective association effect was evident in the five-day-old pups. Illness produced an aversion to the taste of saccharin but not to the cardboard surface, whereas shock produced an aversion to the tactile cue but not the taste cue. Having demonstrated the viability of our conditioning procedures at five-days of age, we decided to push the boundary and test selective associations in one-day-old rat pups (Gemberling & Domjan, 1982). Using methods similar to what we used with the five-day-old pups, we found similar results with pups one day post-partum. These findings provide convincing evidence that extensive post-natal experience is not required for the selective association effect to appear. To press the past experience hypothesis, one would have to argue that pre-natal experiences create selective associations. However, a more plausible interpretation at this point is that selective associations have a strong genetic basis, a hypothesis that is explored in another contribution to this special issue.

In addition to testing for selective associations in one-day-old rats, Gemberling and Domjan (1982) looked for possible long delay learning. Separate groups of pups were injected with lithium immediately, 30 min, or 90 min after exposure to the saccharin flavor. Interestingly, only the immediate-injection procedure produced a taste aversion. A corresponding experiment with cardboard-shock learning showed that only if the shock was administered while the pups were on the cardboard surface did they learn an aversion to that texture. Providing shock immediately after the tactile CS or 1 min

later did not work. These findings are significant for two reasons. First, they confirm that an aversion does not develop simply because of the presentation of the US. Rather, the US has to be paired with the appropriate CS. (Other types of controls included in the Gemberling-Domjan study led to the same conclusion.) Second, these results show that long-delay taste aversion learning and selective associations do not inevitably go hand-in-hand. One can have one without the other.

We typically think of selective associations and long-delay learning as related. In fact, they are integrally related in the concurrent interference theory of long-delay learning (Revusky, 1977). According to that theory, long-delay learning occurs with a taste CS because selective associations preclude the conditioning of other types of cues that may be encountered during the delay interval. The developmental dissociation of selective association and long-delay learning found by Gemberling and Domjan suggests that selective associations are not sufficient for the occurrence of long-delay learning. In other research in collaboration with the Amsel lab, we plotted out the developmental time course of long-delay taste-aversion learning and found it to match the ontogeny of other forms of working memory (Gregg et al., 1978).

Impact of Research on Selective Associations

As I mentioned at the outset, references to the original demonstration of selective associations by Garcia and Koelling (1966) are common in textbook descriptions of learning. Everyone seems to be familiar with what they call the Garcia bright-noisy-water experiment. By that measure, the phenomenon has had a huge impact. The wide dissemination of the phenomenon gives the impression that it is well established. I suspect that most students who learn about the phenomenon these days would be surprised to also learn that the original experiment lacked any control groups. Subsequent research that included a variety of controls is typically ignored when the Garcia-Koelling experiment is described. Therefore, students are not informed about some of the complexities of a proper demonstration of selective associations.

Students and textbook writers might be also surprised if they knew how few studies have been conducted on the Garcia-Koelling selective association effect. By now there are about three thousand published reports of conditioned taste aversions (Freeman & Riley, 2009). Investigators have also examined aversion learning to non-gustatory cues with illness and aversions to taste conditioned with shock. However, such experiments include only one cell of the 2 x 2 design required to demonstrate a selective association effect. Garcia and his colleagues published just two papers with the complete 2 x 2 design. My colleagues and I have published five such papers. Peter Holland demonstrated the existence of a mediated form of the selective association effect (e.g., Holland, 2009). Allowing for several other relevant papers that might have escaped my attention, the entire corpus of work on the bright-noisy-water effect is less than a dozen. That is remarkably few for such an important phenomenon.

Why has the Garcia-Koelling effect stimulated relatively little empirical effort in spite of its considerable impact? As I pointed out at the outset, the phenomenon played a major role in encouraging thinking about how learning processes may have been shaped and constrained by evolution. These considerations also encouraged efforts to integrate laboratory studies of learning with a more ethological approach that

emphasized how learning may operate in an animal's natural environment to increase its fitness. However, if one begins with an ethological/naturalistic approach to the study of learning, one would never conduct an experiment comparing how animals learn to avoid illness as compared with shock or exteroceptive pain. There is nothing in the natural environment that makes that a meaningful comparison. Learning about postingestional illness is ecologically relevant to studies of foraging and food selection. In contrast, learning about shock is ecologically relevant to learning about predatory and other forms of defensive behavior. These are distinctively different topics in the study of animal behavior. Looking back, I was curious to realize that I also failed to discuss selective associations in some of my own more recent writings specifically concerned about ecological factors in learning (Domjan 1998; Domjan, Krause, & Cusato, 2004).

It is no accident that the Garcia-Koelling experiment was designed by experimental psychologists who had little concern for the natural environment of rats at the time the experiment was conducted. The experiment was a product of efforts to better understand how rats might learn about radiation exposure and as a test of general-process learning theory. The results quickly gained prominence because they provided evidence challenging the general process approach. Only a concern about the generality of learning could have encouraged putting two disparate ecological issues (poison avoidance learning and fear conditioning) into the same experiment.

Although the Garcia-Koelling effect is widely recognized as a feature of associative learning in textbook overviews of learning, it has not received much attention in scholarly reviews of Pavlovian conditioning that did not have an ecological focus. Most such reviews have had little, if anything, to say about selective associations. In a review of Pavlovian conditioning that appeared in the *Annual Review of Psychology*, Rescorla (1988) noted that "although there was some concern about the adequacy of the original demonstrations of this phenomenon, subsequent work has placed it on sound ground" (p. 348) and cited the experiments by Miller and Domjan (1981b). He went on to comment that the phenomenon may represent *differential sensitization.* However, for some reason he did not credit Miller and Domjan for effectively dealing with that issue. Rescorla also noted that "the animal may come to the experiment with a preexisting association between certain pairs of stimuli," (p. 349) but he did not recognize our research on selective associations with pre-weanling rats as relevant to this question (Gemberling & Domjan, 1982).

In a subsequent review titled "Classical conditioning since Pavlov," Bitterman (2006) did not provide any details about taste-aversion learning or selective associations but noted that the procedures used in these studies "varied widely, some of them terribly crude. In experiments on conditioned food aversion, an animal ingests some food that waits in its gut for a poison given later, or sometimes even before the food, to take effect. In any case, there is no meaningful control either of the CS, the US, or the CS-US interval." (p. 368).

Bitterman's comments about the lack of precise control over the CS and the US are a bit odd, considering his own extensive research on conditioning with free-flying honeybees.   In those experiments, visual and olfactory conditioned stimuli are encountered as a bee lands on a feeding station and the US requires the bee to ingest sucrose that is provided. That methodology involves weak experimental control of the CS, the US, the CS-US interval, as well as the intertrial interval. However, I doubt that

Bitterman would claim that those features of his methodology negate the validity or importance of the results of those experiments.

Are there lessons in all this for the next generation of scientists? First, I think this story suggests that it is well worth taking a close look at phenomena that are widely accepted by the field. Phenomena that are treated as firmly established may not be on as strong empirical footing as people assume. In considering any phenomenon, no matter how prominent, it is useful to dig into the primary sources, think critically about control conditions, and look for what might be needed to extend the generality of the phenomenon. This story also points to the importance of effectively marketing one's research. I naively assumed that I just had to publish my work in well-regarded journals to reach the relevant audience. Unfortunately, that was not always enough. Evidently, effective dissemination of one's work requires more than that.

Effectively marketing one's work has become more challenging with the proliferation of journals in contemporary science. In addition, much of science news these days travels as much through blogs, well-designed personal web sites, and research networks such as ResearchGate rather than through traditional publication outlets. Prominence in science depends as much on mastering these new tools of communication as on conducting the best experiments.

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