

## From fringe to mainstream: the Garcia effect

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**ABSTRACT:** The rejection of research results is sometimes thought to be justified in cases of individuals embracing fringe ideas that depart significantly from prevailing orthodoxy, or in cases of individuals who lack appropriate expertise or credentials. The case of John Garcia exhibits both of these dimensions, and illustrates that such rejection can delay scientific advancements. Garcia's work decisively challenged what was the orthodoxy in psychology in the midcentury: behaviorism. Behaviorist learning theorists suffered from theory-entrenchment insofar as they failed to acknowledge Garcia's anomalous research findings that ran counter to their theoretical expectations. The case study also illustrates that theories on the margins can become embraced as a result of advancements in adjacent research fields. Studying how Garcia's work moved from fringe to mainstream results in lessons for the philosophy of science and epistemology more generally. Only when we see the mechanisms of exclusion at work can we understand how science and other knowledge production systems can inadvertently act counterproductively via gatekeeping practices that filter out unorthodox points of view.

*Keywords:* fringe science, theory change, cognitive revolution, behaviorism, learning theory, conditioned taste aversion

### 1. Introduction

John Garcia's work challenged fundamental principles of the 20<sup>th</sup> century's mainstream behaviorist learning theory. A Spanish-American farm worker, mechanic, soldier, and G.I.-Bill funded UC-Berkeley dropout, Garcia was entirely unknown and without a Ph.D. in 1955, when his first publication demonstrating the conditioned taste aversion effect entered the mainstream generalist journal *Science*. He and his colleagues found that rodents that drank a saccharin-flavored liquid (conditioned stimulus, hereafter CS) and were exposed to radiation (unconditioned stimulus, hereafter US) that caused noxious effects subsequently came to exhibit an aversion to the saccharin liquid. In other words, the animals learned to associate a taste with radiation illness, despite the delay between the taste and the onset of illness, and after only a single pairing.

These results were not just anomalous to the behaviorist learning theory; they contradicted its central principles. No one seems to have noticed or cared, perhaps in part due to the fact that Garcia himself explicitly tried to fit the results into a conditioning paradigm. It was long believed that repetitive spatiotemporal contiguity between a CS and an US is required to produce a conditioned association, which strengthens with the frequency of CS and US pairings and becomes extinct without subsequent recurrence of CS and US pairings (Garcia, McGowan, & Green, 1972, p. 24). This idea had been around since the time of Locke. As Hume (1739/1896, pp. 10-13, 282-284; Treatise I.i.4; II.i.4) put it, learned associations must have “contiguity in time or place” and “succession” and the strength of such an association depends on repetition or “constant conjunction.” In other words, CS-US pairings must be (1) contiguous and (2) repeated in order for an animal to form a conditioned association. Garcia’s CS-US pairing surely was neither contiguous nor repeated, and yet an association was formed.

It wasn’t until 1966, when Garcia succeeded in publishing two groundbreaking papers in the little-known “maverick” (Lubek & Apfelbaum, 1987, p. 73) and “nonrefereed” (Revusky, 1977, p. 63) journal *Psychonomic Science*, that the ears of young learning theorists beginning to make a name for themselves perked up. This pair of papers (Garcia & Koelling 1966; Garcia, Ervin, & Koelling, 1966) took a turn by making a drastic proclamation of the taste aversion effect and its consequences for behaviorist learning theory. Garcia and his colleagues claimed that visual and gustatory stimuli are differentially associable with pain from a shock and gastric illness from a toxin. This called into question a third principle of the traditional behaviorist learning theory, namely, (3) stimulus equipotentiality—that all classes of stimuli have equal potential to become conditioned.

Garcia encountered resistance, especially from psychologists of his own generation. Throughout the ‘60s and ‘70s, his work was continuously rejected from mainstream journals, including *Nature*, *Science*, *Journal of Comparative and Physiological Psychology (JCPP)*, *Psychological Review*, *Psychological Bulletin*, and *Journal of Experimental Psychology* (Lubek & Apfelbaum, 1987). He also received personal attacks from reviewers. This saga went on for about two decades before, with much ado, he became the award-winning psychologist we know him to be today for his discovery of the Garcia effect.

## **2. Garcia’s Deviant Experimental Results**

By the mid-20<sup>th</sup> century, behaviorist learning theory dominated psychology. It stated that learned behaviors are the result of forming direct associations between a stimulus and a response, as in classical conditioning, or between a response and reinforcement, as in operant conditioning. In other words, learned behaviors can be explained simply in terms of association between stimuli and responses, or responses and reinforcements, without any need to posit intervening unobservable internal states. Behaviorist learning theory denied the biological organism of any instinct by asserting that all behavior—even breathing—becomes conditioned shortly after birth. In John B. Watson's (1924) *Behaviorism*, he put this bluntly: "There are for us no instincts—we no longer need the term in psychology..." (as quoted by Gould & Marler, 1987, p. 4). In B. F. Skinner's (1953, p. 157) *Science and Human Behavior*, he stated that instinct is a "flagrant example of an explanatory fiction" and "an appeal to ignorance" because "If the instinct of nest-building refers only to the observed tendency of certain kinds of birds to build nests, it cannot explain why the birds build nests." Evolutionary considerations were thus cast out of psychology.

Garcia's work on conditioning grew out of the successful work he'd done on radiation's effects on the brain. After enlisting in the Army Air Corps during WWII, Garcia used a G.I. Bill to pay for his college tuition at Santa Rosa Junior College, and later his tuition for his Master's and Ph.D. degrees (1965) from UC Berkeley. He originally left Berkeley without finishing his doctorate after failing a statistics exam (Bolles, 1993, p. 333) and when his advisor Edward Tolman left (Lubek & Apfelbaum, 1987, p. 67) and took his first research position at the U.S. Naval Radiological Defense Lab, where he studied the effects of radiation on the brain. He discovered the taste aversion effect by chance when studying the effects of ionizing radiation on the rat brain. During these experiments, he noticed that rats avoided water from the plastic bottles in the radiation chambers, and hypothesized that the rats associated the taste of the water with the radiation illness.

The experimental results of Garcia's hypothesis constitute the initial findings Garcia published in *Science* in 1955, "Conditioned Aversion to Saccharin Resulting from Exposure to Gamma Radiation" along with Donald Kimeldorf, a radiation researcher, and Robert "Bob" Koelling, then a hospital corpsman (Garcia, 2003, p. 68). Garcia and Kimeldorf (1957) then published "Temporal relationship within the conditioning of a saccharine aversion through radiation exposure" in *JCPP*. Both of these studies showed that the taste aversion effect could be

established in a single trial. The anomalous results went against the behaviorist principle of repetition.

But the authors didn't say so explicitly. In fact, they said quite the opposite. In this earlier paper that was accepted by *JCPP*, Garcia and Kimeldorf (1957, p. 182) safely said that their results were "consistent with accepted concepts of conditioning despite the differences in stimulus duration required by low-intensity radiation experimentation."

By 1966, Garcia had his Ph.D. and changed his tune about behaviorist learning theory. Garcia and Koelling (1966, p. 124) explicitly rejected the principle of stimulus equipotentiality: "It seems that given reinforcers are not equally effective for all classes of discriminable stimuli. The cues, which the animal selects from the welter of stimuli in the learning situation, appear to be related to the consequences of the subsequent reinforcer." Then, in the same year, Garcia and Koelling along with Frank R. Ervin, a clinical psychiatrist with a background in neurology, rejected the principles of repetition of and contiguity between CS-US pairings, saying, "It is considered axiomatic in theory and practice that no learning will occur without immediate reinforcement... However, our data indicates that immediate reinforcement is not a general requirement for all learning" (Garcia, Ervin, & Koelling, p. 121) and add that, "These data indicate anew that the mammalian learning mechanisms do not operate randomly, associating stimuli and reinforcers only as a function of recency, frequency and intensity" (p. 122).

Garcia and his colleagues struggled to publish these 1966 papers, which were both rejected, for example, by *Science* and *JCPP*. The association of taste with radiation illness was made especially robust in Garcia & Koelling's (1966) paper.<sup>1</sup> Again, these results challenged the principle of stimulus equipotentiality—that any neutral perceptible stimulus (any visual, auditory, olfactory, gustatory, or touch cue) can become associated with an unconditioned stimulus or response through conditioning. Pavlov (1928; as quoted by Garcia, McGowan, & Green, 1972) emphasized the equivalence of conditioned stimuli:

if our hypothesis as to the origin of the conditioned reflex is correct, it follows that any natural phenomenon chosen at will may be converted into a conditioned stimulus... Any visual

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<sup>1</sup> This was a product of Garcia's dissertation work at UC Berkeley (Lubek & Apfelbaum, 1987, p. 68).

stimulus, any desired sound, any odour, and the stimulation of any part of the skin, whether by mechanical means or by the application of heat or cold...

But Garcia and Koelling demonstrated that at least some perceptible stimuli have a selective effect on what is learned and that not all stimulus elements in an acquisition situation always become conditioned stimuli. Shock to the feet (US) produced avoidance of a sight cue—bright, noisy water—but not a taste cue—sweet water—whereas illness caused by X-rays (US) produced avoidance of sweet water, but not bright, noisy water. This suggests that rats are prepared to form associations between flavor and illness but not between sound and illness. Garcia called it the “belongingness”—the selectivity—of certain stimulus pathways. The equipotentiality of stimuli would predict that flavor and sound should have equal potentials to become conditioned stimuli. Garcia’s interpretation was instead that there are selective pathways involved, as with taste (CS) and illness (US). These results suggested that evolutionary considerations as well as internal states needed to be reintroduced into behaviorist psychology.

In Garcia, Ervin, and Koelling’s (1966) paper, they demonstrated the same effect with a long delay between taste and illness. Again, visual and gustatory stimuli had selective potential to become conditioned stimuli. Garcia and his colleagues demonstrated that immediate (seconds or fractions thereof) and repeated pairings or reinforcement is sometimes unnecessary for an animal to learn an association, and for that association to become relatively resistant to extinction. Rather, an animal can develop a conditioned taste aversion even with a relatively long delay (hours) between a CS (taste) and an US (gastric illness).

### **3. Rejection**

In the 1960s, Garcia was sometimes able to publish his studies on conditioned taste aversion, but not in APA journals, which represent the mainstream in psychology. His findings were not heavily cited and did not overturn the principles of learning theory that they contradicted. One investigator who had worked for many years on delayed reinforcement publicly stated of Garcia’s results, “Those findings are no more likely than birdshit in a cuckoo clock” (Gould & Marler, 1987, p. 4). Why did learning theorists resist Garcia’s refutation of these core principles?

Garcia himself, during his 1980 acceptance speech for the APA's prestigious Distinguished Scientific Contribution Award, diagnosed the rejection of his papers as the "neophobia"<sup>2</sup> of journal editors and editorial consultants (Garcia, 1981, p. 149) and admonished the disparaging feedback he received as "pseudocriticism" (p. 152). Garcia's acceptance speech, titled "Tilting at the Paper Mills of Academe"—a reference to his troubles getting his papers published<sup>3</sup>—was his first publication on taste aversion learning in an APA journal since 1962. Before 1962, he published "some 20 papers in prestigious journals and volumes without a single rejection" (Garcia, 1981, p. 149). These successful papers contained novel results, but were either not related to learning theory, or carefully framed in the language of the dominant behaviorist learning paradigm (as also noted by Lubek & Apfelbaum, 1987, p. 68).

This success took a turn in 1965, when Garcia decided to take a stand against the traditional learning theory that saw the animal as an unbiased *tabula rasa*. As noted, he challenged the equipotentiality of stimuli by showing that some conditioned stimuli are easier to associate with negative consequences than others (sounds with shock and tastes with illness): "given reinforcers are not equally effective for all classes of discriminable stimuli" (Garcia & Koelling, 1966, p. 124). Moreover, he provided an evolutionary interpretation: "natural selection may have favored mechanisms which associate gustatory and olfactory cues with internal discomfort since the chemical receptors sample the materials soon to be incorporated into the internal environment" (Garcia & Koelling, 1966, p. 124). This explicit denial of stimulus equipotentiality seems to have gone too far, and Garcia found it difficult to publish. Again, his groundbreaking findings were

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<sup>2</sup> It seems likely—keeping in tone with the rest of his address (see fn. 3)—that this "diagnosis" is satirical, in reference to the dismissive interpretation of Garcia's results as attributable to neophobia rather than conditioning.

<sup>3</sup> "Paper mills" is a play on Don Quixote's demonization of the "threatening...multiple arms" of giant windmills and Sancho Panza's milder view that "They are only windmills." In the speech, Garcia allegorically embodies Sancho Panza, and satirically presents evidence for the view that journals are not "governed by Janus-faced demons" but actually "operated by timid but tractable organisms" (p. 149). The speech is brilliantly funny and easy-going in spite of Garcia's palpable resentment. Garcia included nine illustrations poking fun at the unwarranted criticism he received by mainstream learning theorists (Lubek & Apfelbaum, 1987, p. 60); the APA removed five of these before publishing the speech in *American Psychologist* (see Garcia, 1981).

rejected by leading journals, including *Science* and the APA's *JCPP*. This was the first rejection from mainstream journals that Garcia had received in the past decade. An abbreviated version of this paper was ultimately published in *Psychonomic Science*: Garcia and Koelling's (1966) "Relation of cue to consequence in avoidance learning." *Psychonomic Science* was a generalist physiological psychology journal that had more concern for psychometrics than for learning theory and animal behavior.

Following this first rejection, his work on conditioned taste aversion was frequently rejected by mainstream journals—up until his 1980 award. To understand this, it will help to get a flavor of the reasons given for rejection.

These rejections were not particularly pleasant according to Garcia (1981, p. 149) himself: "Often, the critique is embellished with gratuitous personal insults. One consultant, in an ill-worded passage, informed the editor that one of our recent manuscripts would not have been acceptable even as a term paper in his or her learning class." He (1981, p. 150) recalls, "Some editorial consultants said we used too many treatments. Some said we used too few," indicating that reviewers may have been floundering to find something wrong with this study. According to the reviewer of one of his submissions to *Science*, the results were "interesting," but provided no explanation for "how the X-ray reinforcement produced its effect" (as quoted in Lubek & Apfelbaum, 1987, p. 73), and thus, the paper was rejected. Garcia (1981, p. 150) rebuffed, "Apparently, this consultant was satisfied that we all know how shock reinforcement works." Neither was there an explanatory mechanism for classical and operant conditioning. As late as 1976, there still was no accepted mechanism for the Garcia effect: "There is at present no clearly correct explanation of the mechanism of long-delay learning" (Rozin, 1976, p. 38).

Another rejected paper showed that rats acquired an association between a taste (CS) and radiation illness (US) even when these were presented hours apart (*sans* contiguity), and after a single pairing (*sans* repetition). This paper was also ultimately published in *Psychonomic Science*: Garcia, Ervin, and Koelling's (1966) "Learning with prolonged delay of reinforcement" after being rejected by mainstream top-tier journals where Garcia's work had been previously accepted—again, *JCPP* and *Science*.

The editor of *JCPP*, William Estes, promptly sent a rejection letter citing complaints from an anonymous reviewer about the methodology used in the radiation-induced aversion study. Specifically, the reviewer complained about the "unsuitability of experimental design, lack of

temporal control of stimuli, lack of pseudoconditioning controls, and an alternative explanation of conditioned nausea” and said that the results lacked “general theoretical relevance” (Lubek & Apfelbaum, 1987, p. 72). As Garcia pointed out in a reply to the rejection, there was an inconsistency in rejecting the paper for theoretical reasons when all the specific remarks related in the rejection letter were related to methodology. To this, “Estes promptly responded, defending his consultant as someone well versed in the literature of conditioning and aware of relevant Polish and Russian studies, ‘although I’m not sure you are’” (Lubek & Apfelbaum, 1987, p. 73). This is blatantly a pro and ad hominem argument, for the reviewer, and against Garcia.

Indeed the methodology implemented by Garcia in this study ran against paradigmatic expectations. As Lubek and Apfelbaum (1987, p. 67) detail:

As the conditioned stimulus in this study, Garcia chose not to use the narrow range of traditional stimuli (tone, light, air-puff, or electric shock) but instead employed long-duration ionizing radiation. Also, Garcia did not focus on the usual motor responses used by Pavlovian, Skinnerian, or neo-behaviorist conditioners (running, jumping, blinking, pressing, etc.) but on the organism’s alimentary and olfactory response systems. The novel choice of stimulus combined with an unusual response system helped highlight behavioral anomalies not seen in other studies.

This isn’t entirely accurate, since Pavlov didn’t focus on motor responses, but used conditioned salivation responses. Either way, this study conveyed no clear-cut unconditioned response: “while it is reasonable in a strictly operational sense to designate X ray as the US it is difficult to point to an unconditioned response (UR)” (Garcia, McGowan, & Green, 1972, p. 28). Garcia conceived the radiation-induced noxious effects not as an UR, but rather suggests, “This ‘radiation sickness syndrome’ is the most likely reinforcing stimulus (US),” but notes, “this formulation leads to still other theoretical difficulties” (Garcia, McGowan, & Green, 1972, p. 30), though he doesn’t say what these are. In other words, it was difficult to fit the experimental design of the radiation-induced taste aversion studies into the conditioning paradigm.<sup>4</sup>

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<sup>4</sup> In his 1950s papers, Garcia designates radiation itself as the unconditioned stimulus, perhaps as an attempt to fit the experiment into a conditioning paradigm: “The processes through which radiation is capable of



Indeed, Garcia's experimental techniques were not viewed as particularly rigorous when weighed against the dominant standards. He was not trained in traditional learning theory nor its methodologies and the effect was that his experiments made use of relatively simple techniques (Revusky, 1977, p. 63): "Garcia's original techniques involved production of flavor aversions by means of x-irradiation and measurement of the aversion by a simple test of consumption: weighing bottles. This meant that elaborate learning methodologies were not necessary and hence reduced the value of the educational investments made by members of the in-groups." And, as the *JCPP* editor mentioned, Garcia did not assume the need to use pseudoconditioning controls—namely, unpaired control trials in which the CS (saccharin water) was not paired with the US (radiation illness) or vice-versa. The famous learning theorist, Morton Edward "Jeff" Bitterman (1975, p. 708) saw the lack of pseudoconditioning controls as a red flag, and conveyed this in the well-read pages of *Science*:

Problems of control abound in these aversion experiments, perhaps because they are not always uppermost in the minds of the investigators. The view actually has been expressed that it "doesn't matter" whether a food aversion is the product of conditioning or pseudoconditioning, that what is important is that "behavior shows astonishingly organismic properties."

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operating as an unconditioned stimulus are unknown" (Garcia, Kimeldorf, & Koelling, 1955, p. 158); "These effects were described as conditioned aversions or avoidances, and it was suggested that gamma radiation acted as an unconditioned stimulus" (Garcia & Kimeldorf, 1957). Allowing the radiation rather than the radiation illness to be the unconditioned stimulus might have enabled the experiment to pass as classical conditioning, since radiation illness could then be designated the unconditioned response (although this is a bit odd, since in classical conditioning experiments, the unconditioned response is the same as the conditioned response). By 1966, Garcia was less clear about what the US was. Garcia & Koelling (1966) mention "the reinforcer, i.e., radiation or toxic effects" (p. 123) and say, "Apparently when gustatory stimuli are paired with agents which produce nausea and gastric upset, they acquire secondary reinforcing properties which might be described as 'conditioned nausea'" (p. 124). But in the next article, Garcia, Ervin, & Koelling (1966) designate the illness as the US: "The omnivorous rat displays a bias, probably established by natural selection, to *associate gustatory and olfactory cues with internal malaise* even when these stimuli are separated by long time periods" (p. 122, italics mine).

On this point, Bitterman cited Garcia, McGowan, and Green (1972), but said nothing of *why* these controls do matter. Revusky and Bedarf (1967) did not use pseudoconditioning controls in their replication study either, in which they used a novel food instead of saccharin liquid (Revusky, 1977, p. 55). Perhaps Bitterman's reputation as an accomplished psychologist was enough to convince readers that it was an embarrassing oversight to exclude a pseudoconditioning control, but this display of incredulity looks excessive on reflection. A pseudoconditioning control would be needed if there was a chance that sweet water might cause aversion without irradiation—that is, it could rule out the possibility that the animals would have avoided the water even if it hadn't seemed to make them sick. But there is no reason to take that possibility seriously, since rats prefer sweet water under normal circumstances. Bitterman seems to find the very idea of an organismic contribution preposterous, but, instead of explaining why in careful detail, he simply presents the authors own words as if it will be obvious that such an idea should be rejected out of hand as absurd.<sup>5</sup>

These methodological departures do not mean that Garcia's work was shoddy, or lacking in scientific merit. Recall that his early work, too, had gone through peer review, and his critical results were replicated multiple times by him and his colleagues (e.g. Garcia, McGowan, Ervin, & Koelling, 1968; Garcia, Ervin, Yorke, & Koelling, 1967; Garcia & Ervin, 1968, Revusky & Garcia, 1970) and, later, by others (Smith & Roll, 1967; Revusky, 1968; Rozin, 1969; Seligman, 1970; Seligman et al., 1970; Shettleworth, 1972). Garcia just deviated from prevailing methodological designs. Garcia joked, "It should not be surprising that I employed this hackneyed learning design. After all, my professors at Berkeley, Tolman, Ritchie, and Krech, insisted that I take elementary

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<sup>5</sup> It is also worth noting that Bitterman (1975, p. 708) provides an alternative theoretical interpretation of the results of Garcia et al.'s (1968) taste aversion studies:

The results for irradiation may be attributed to the fact that gustatory stimuli persisted in the interval between irradiation and illness whereas visual stimuli, of course, did not. The results for shock may be attributed to the fact that the visual stimuli antedated shock by a short interval favorable to conditioning (since the animal saw the food before taking it), whereas the gustatory stimuli were at best simultaneous with shock and may have even followed it (since the animals were shocked immediately upon taking the food). Testing conditions also were confounded with modality; since the visual stimuli antedated the criterion response (eating) and the gustatory stimuli followed the response, it should not be surprising that the visual group hesitated much longer than the taste group before taking the food.

experimental design and statistics courses, despite all rumors to the contrary” (Garcia, 1981, p. 153).

Sam Revusky (1977, p. 63), a student of *JCPP* editor Estes and well-versed in traditional learning theory, mentions hearing the gossip on multiple occasions that “Garcia indeed discovered the effect but did not know how to do a rigorous experiment to prove it” and that “Paul Rozin and/or Sam Revusky allegedly did the definitive experiments” after Garcia. Rozin’s (1969) “Central or peripheral mediation of learning with long CS-US intervals in the feeding system” and Revusky’s (1968) “Aversion to sucrose produced by contingent x-irradiation: Temporal and dosage parameters” replicating Garcia and Koelling’s (1966) results were both published in *JCPP*. Revusky himself awards Garcia the credit for both discovering the effect and doing the definitive experiments to prove it. When Revusky first became acquainted with Garcia’s work, he wrote to Estes with the excitement that Garcia’s results had “fundamental and revolutionary” implications (Lubek & Apfelbaum, 1987, p. 73). According to Rozin, Garcia did the critical study, and he realized that it was a solution to a set of problems he was facing in his own work on vitamin deficiency and food choice (personal communication, June 2021).<sup>6</sup> Rozin’s work on the effect contributed a line of research on how long-delay learning might work through adaptive specialization and specific, penetrable modules (Rozin, 1976). He provided a feasible mechanism for the effect.

As argued by Revusky, (1977, p. 62), Garcia’s results were “too threatening to bear” and “...there certainly was an atmosphere of fear of obsolescence on the part of many concerned with animal learning, and I conjecture that it was a factor in suppressing a new discovery.” Garcia’s work may have appeared to undermine the establishment insofar as it made the educational and methodological investments of the older generation of mainstream learning theorists seem obsolete, not to mention the high prestige enjoyed by its leading practitioners. In this period, figures such as Skinner were world famous, and regarded as authorities across a wide range of

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<sup>6</sup> Learned specific hungers (e.g. during vitamin deficiency) conflicted with basic learning principles well before the discovery of long-delay taste aversion learning. Basically, a rat is deficient in some vitamin and feels sick, and encounters a food containing that vitamin and starts feeling better. The rat is reinforced for eating the food containing the vitamin. Rozin says, “In spite of experiments by Harris et al. (1933) and Scott and Verney (1947) demonstrating something like this, the conflict with basic learning principles was too great to convince psychologists that some specific hungers were learned” (Rozin, 1976, p. 40).

domains, including child rearing, pedagogy, industrial psychology, and cultivating societal well-being and success. Rozin (1969, p. 421), who was one of the first to recognize the revolutionary implications of Garcia's work, says,

While the notion of specialized long-delay learning mechanisms as a consequence of natural selection is very appealing to biologists (after all, there is every reason to assume that special types of learning can be selected for by special environmental pressures), such an idea has been somewhat offensive to psychologists, whose experimental data and theories highlight the critical importance of close temporal contiguity between CS and US.

A challenge to the ideas that behavior is malleable in any direction and training requires contiguity and repetition was heretical, and threatened to undermine entrenched beliefs and practices. Revusky (1977, p. 64) says,

(1) When the Garcia effect was finally recognized, Paul Rozin was invited to address the convention of the American Psychological Association about it instead of John Garcia. (2) In 1967 and in 1968, with only one or two published papers on food aversion learning, I was invited to contribute chapters to two prestigious volumes on learning instead of Garcia. (3) *The Journal of Comparative and Physiological Psychology*, which regularly prints a list of its guest reviewers, has never, as far as I know, asked Garcia to review a paper for it in spite of publishing many papers in taste aversion learning. Indeed, as I remember, Garcia told me in 1974 that he had only been asked once to review a paper for a journal of the American Psychological Association.<sup>7</sup>

This lack of invitations to contribute papers and referee may have been exacerbated by the fact that Garcia did not have an academic appointment. He worked in labs at Berkeley and Harvard's Massachusetts General Hospital, but was not a Professor, and got his degree when he was in his 40s. His main collaborator, Koelling, was a hospital corpsman at the time of their crucial discovery,

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<sup>7</sup> By this time, Garcia had completed his PhD, without which he ordinarily would have been disqualified from those kinds of assignments, such as acting as a peer reviewer or writing chapters for anthologies.

which may have been the wrong pedigree to enter the conference circuit, edited volumes, and journal refereeing, each of which would have given him an opportunity to remind his peers in the academy of their novel results, and make them answerable to their findings.

In addition, it's not unlikely that Garcia suffered marginalization from being a first-generation Spanish-American (*Skagit Valley Herald*, 2013). This is another way in which he may have been perceived as an outsider, during a time when there were no prominent Hispanic psychologists. This is not mere speculation; Rozin recalls that some of his colleagues went to visit Garcia at his office, and thought he was the janitor (personal communication, June 2021). Ian Lubek mentioned to me that one editor incorrectly regarded Garcia as a “Chicano outsider” (personal communication, June 2021). Lubek and Apfelbaum (1987, p. 79) also report hearing some other similar anecdotes, though present no view on the matter given the lack of systematic data about whether any of the resistance to Garcia's results was due to bias.

#### **4. Acceptance**

In 1978, Garcia was elected to the highly prestigious Society of Experimental Psychologists and was the recipient of the 44<sup>th</sup> H. C. Warren Medal for Outstanding Research in Psychology (Lubek & Apfelbaum, 1987, p. 59). Even while Garcia's papers continued to be rejected by mainstream journals, including the APA's *JCPP*, the APA awarded him the Distinguished Scientific Contribution Award in 1979. His acceptance speech appeared in print in the APA's *American Psychologist*, which, at the time, was “the most widely distributed mainstream psychology journal” (Lubek & Apfelbaum, 1987, p. 60; though this is a bit misleading, since all members of the APA received a copy of this journal). By 1983, Garcia was elected to the National Academy of Sciences. Paul Rozin reported to me in personal communication that Estes admitted that his rejection of Garcia's 1966 papers was the biggest mistake he ever made as an editor of *JCPP*. So what was the turning point?

While behaviorist learning theorists thought the effect might be an artefact—given that it was only a couple of studies from an unknown person—it was confirmed within the next five years by respected psychologists Paul Rozin (Rozin, 1969; Rozin & Kalat, 1971), Martin Seligman (1970), and Sara Shettleworth (1972). Notably, all of these psychologists are from a generation younger than Garcia, Bitterman, and Estes. I would conjecture that their youth, and therefore relative lack of theory-entrenchment, played a role in their ability to hear out the anomalous and

novel findings. As Kuhn (1970, pp. 301-302) quotes Max Planck: “A new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it.”<sup>8</sup>

While Garcia’s work showed that contiguity and repetition were not necessary for all conditioned learning, Leon Kamin’s work showed that contiguity and repetition is not sufficient for learning. Kamin (1969) showed that a classically conditioned association between a CS (tone) and a US (shock) blocks an animal’s learning of an association between a second CS (light) and the same US (shock) when the second CS is presented following the first CS. He began by training rats to press a lever for food. Then, tone and shock were presented, and fear was conditioned to the tone, and this suppressed the lever-pressing for food. But when a both a tone and a light were presented paired with a shock, the rats did not learn to associate the light with the shock, i.e. the light on its own did not suppress the lever-pressing. Kamin concluded that the previous conditioning of fear to the tone blocked the conditioning of fear to the light. In other words, two stimuli presented contiguously to one another repeatedly is not enough for an animal to form an association between a second CS (light) and the US (shock). The tone already makes the animal expect shock. So when the shock is presented with both the light and the tone, the occurrence of the shock is not surprising enough for the animal to form a new association between the light and the shock. Thus the animal does not present the conditioned fear response to the light.

Robert Rescorla furthered Kamin’s findings. He showed that the change in the association between a CS and an US depends on how surprising the US is—to what extent the animal doesn’t predict the US. Animals only learn associations when those associations violate their expectations. In Rescorla’s (1968) conditioning study, different control groups of rats each had a fixed chance of being shocked in the presence of a CS when lever-pressing for food, and fear was conditioned to the CS in each of these groups. The experimental groups had a fixed chance of being shocked in the presence of the CS, but also had a chance of being shocked without the CS. For the experimental groups, the CS was not associated with shock—i.e. there was no conditioned fear response to the CS—since shock occurred whether or not the CS was present. Again, the results

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<sup>8</sup> Ian Lubek and Erika Apfelbaum (1987) treat Garcia’s results in a Kuhnian vein “as a case study of a mainstream, ‘normal science’ paradigm’s supporters’ attempts to block, marginalize, or reject a deviant set of scientific ideas anomalous enough to merit a paradigmatic shift” (p. 60).

showed that contiguity was not sufficient for conditioning. If the animal doesn't predict a US based on a contiguous CS, then the animal does not learn to associate the CS with the US.

During this time, too, Keller and Marian Breland famously established Animal Behavior Enterprises, a business in which they conditioned behaviors in various animals using operant conditioning techniques. Behaviors were conditioned across a wide range of animals, from reindeer to whales, and were exhibited at zoos, storefronts, fairs, on television shows, and in commercials. Rabbits and ducks were conditioned to play the piano. Cats were conditioned to dial an old telephone. And, by accident, chickens were conditioned to turn on a jukebox and dance:

In the attempt to create quite another type of demonstration which required a chicken simply to stand on a platform for 12-15 seconds, we found that over 50% developed a very strong and pronounced scratch pattern, which tended to increase in persistence as the time interval was lengthened... However, we were able to change our plans so as to make use of the scratch pattern, and the result was the "dancing chicken" exhibit... (Breland & Breland, 1961, p. 682)

This venture, although successful, taught that operantly conditioned animals will sometimes revert to their instinctual behaviors. Some behaviors could simply not be operantly conditioned, namely, those contrary to a species' instincts. For example, they attempted to condition a raccoon to pick up coins and deposit them in a "piggy bank." Raccoons are relatively easy to condition, but the Brelands encountered a problem. First, they conditioned the raccoon to pick up the coin, then the metal container was introduced into which the raccoon had to drop the coin to get his reward. However, "he seemed to have a great deal of trouble letting go of the coin. He would rub it up against the inside of the container, pull it back out, and clutch it firmly for several seconds... he would finally turn it loose and receive his food reinforcement" (Breland & Breland, 1961, p. 682). When they tried to condition the raccoon to pick up two coins and put them in the container, things got even worse:

Not only could he not let go of the coins, but he spent seconds, even minutes, rubbing them together (in a most miserly fashion), and dipping them into the container. He carried on this behavior to such an extent that the practical application we had in mind—a display featuring a raccoon putting money in a piggy bank—simply was not feasible. The rubbing behavior

became worse and worse as time went on, in spite of nonreinforcement. (Breland & Breland, 1961, p. 682)

In other words, some animals are contraprepared to learn certain associations. This called the Pavlovian principle that all perceptible stimuli are equally malleable into question, as did Garcia's work. Regarding the assumptions of traditional learning theory, Breland and Breland (1961, p. 684) concluded:

Three of the most important of these tacit assumptions seem to us to be: that the animal comes to the laboratory as a virtual *tabula rasa*, that species differences are insignificant, and that all responses are about equally conditionable to all stimuli. It is obvious, we feel, from the foregoing account, that these assumptions are no longer tenable. After 14 years of continuous conditioning and observation of thousands of animals, it is our reluctant conclusion that the behavior of any species cannot be adequately understood, predicted, or controlled without knowledge of its instinctive patterns, evolutionary history, and ecological niche.

Furthermore, Martin Seligman, who was rising to fame for his work on learned helplessness (Seligman & Maier, 1967)—and would later serve as the president of the APA—was enthusiastic about Garcia's work. In a 1970 *Psychological Review* article, "On the generality of the laws of learning", which was read by both Garcia and Rozin in manuscript, Seligman criticizes equipotentiality. He cites Garcia's work five times (more than any other author), details and defends Garcia and Koelling's (1966) findings at length (pp. 409-410), using the work of Rozin (1967, 1968), amongst others, to show that the Garcia effect is indeed "remarkable." He also cites Breland and Breland (1966) as challenging the "general process view of learning" (Seligman, 1970, p. 408)—insofar as they demonstrate the contrapreparedness of animals to learn specific conditioned associations that go against what is evolutionarily advantageous to them. He sums up the ideas under his "preparedness continuum:"

organisms are prepared to associate certain events, unprepared for some, and contraprepared for others. A review of data from the traditional learning paradigms shows that the assumption of equivalent associability is false: in classical conditioning, rats are prepared to associate tastes with illness even over very long delays of reinforcement, but are contraprepared to



associate tastes with footshock. In instrumental training, pigeons acquire key pecking in the absence of a contingency between pecking and grain (prepared), while cats, on the other hand, have trouble learning to lick themselves to escape, and dogs do not yawn for food (contra-prepared).

Shortly thereafter, Seligman and Joanne Hager included four of Garcia's papers in their seminal 1972 volume *Biological Boundaries of Learning*. Garcia is clearly the star in this volume, and listed amongst names like Jean Piaget and Edward Thorndike. This certainly gave Garcia's work a professional boost. As a result of all of this, learning theorists were forced to accept that animals are biologically prepared and contraprepared to learn certain associations.

There is evidence that Garcia's work surfaced even prior to this, however. Sara Shettleworth, also an author included in Seligman and Hager's (1972) volume, and frequently cited alongside Garcia, notes that profound intellectual changes were beginning to take place during her first year in graduate school in the Psychology department at the University of Pennsylvania, presumably 1966.<sup>9</sup> Shettleworth (2010) says:

My first year in grad school (at the University of Pennsylvania) coincided with the appearance of a handful of seminal findings that would deeply change how we think about 'animal learning' and its relationship to the rest of behavioral biology. The most important was the 'Garcia effect'...

There was a general shift taking place away from behaviorist animal learning principles. The idea that animals are completely malleable to make conditioned associations was put into question by the ethology movement that was taking place in Europe. Ethologists emphasized the instinctual nature of species' behaviors as prepared by evolution by natural selection. Karl von Frisch,

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<sup>9</sup> Shettleworth would have begun her graduate studies at Penn before 1967 (she transferred to the University of Toronto in 1967)—so presumably in 1966. Interestingly, Seligman, now at Penn, also received his Psychology Ph.D. from Penn, and Rozin was teaching in the Psychology department beginning in 1963. Though the two knew each other, they did not suspect each other of working on it, and happened to both replicate Garcia's findings unbeknownst to each other. Bolles (1993, p. 341) points out that Kamin, Seligman, and Rescorla were all students of Richard Solomon who was at Penn for many years.

Nikolaas Tinbergen, and Konrad Lorenz won the 1973 Nobel Prize in Physiology or Medicine for “discoveries concerning organization and elicitation of individual and social behaviour patterns” (Nobel Media AB, 2021). Lorenz’s (1937) work investigating the mechanisms of imprinting, for example, made famous that a gosling will form an attachment to the first large moving thing it sees upon hatching, rather than to its mother. This demonstrated that animals can be pre-prepared to learn certain associations, at least during the critical period after birth, and is also an example of one-shot learning—not unlike Garcia’s rats that experienced only one pairing between taste and illness and formed an association. Although there was no temporal gap, Lorenz’s work called into question the core assumption of learning theory that repetition or constant conjunction is necessary to form a conditioned association.

A more speculative factor that contributed to the acceptance of Garcia’s work was the broader shift happening in the sciences of the mind. At the same time Garcia was publishing, the cognitive revolution was taking place (Neisser, 1967; Gardner, 1987). One might even argue that he was active in its precipitation. Fields such as artificial intelligence and cybernetics were providing a vocabulary for internal processes. Garcia and Garcia y Robertson (1985, p. 191) mention George Miller’s (1956) *The Magic Number Seven, Plus or Minus Two* on memory span as demonstrating a “psychobiological” fact about humans. Miller’s work proved that there are ways to observe and measure inner states. Rozin (1976) was developing a modular mechanism for the Garcia effect in terms of ‘programs’ and ‘circuits’. Noam Chomsky’s (1959) review of Skinner’s (1957) *Verbal Behavior* was a general critique of the behaviorist position, and laid the foundation for his later (1980) “poverty of the stimulus” argument—that language learning required domain-specific resources, and that the mind supplied structure to learning beyond the surface form of linguistic inputs.<sup>10</sup> It is clear that the intellectual landscape was changing within the United States towards recognition of internal cognitive states and processes. There was a new emphasis on inner representations, and also on innate mental structures (as opposed to the *tabula rasa* view). Both of these ideas conflicted with the behaviorist paradigm.

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<sup>10</sup> Chomsky was also at the University of Pennsylvania. Rozin is still there. Rescorla was there before leaving for Yale, where he trained Garcia’s collaborator Koelling, and then later went back to Penn. Although Rozin didn’t know Seligman was working on it at the time, these close institutional ties between the key players involved in the reception of Garcia’s work is noteworthy. Perhaps there was a shared attitude of openness for the new frontier amongst them.

It seems likely that the development of computing and information storage provided a language and framework for understanding cognitive processes. We can see this in Seligman's work, for example. He argues for a "cognitive representation" between stimulus and response, and refers to training contingencies as "information" (Seligman, 1975, p. 47). Similarly, Rescorla started talking about the changing representation of an US during the course of extinction (Rescorla & Cunningham, 1978).

Within animal studies, computational language may have been less popular, but there was still a move towards thinking about cognition rather than just conditioned behavior. Shettleworth (2010) says that the traditional animal learning theory broadened in scope to include new problems that concern the previously-set-aside cognition, while also becoming more specialized:

the biggest change has been the transformation of 'animal learning' into the contemporary interdisciplinary study of comparative cognition. Of course this has happened along with tremendous changes in all the rest of the biological and cognitive sciences, including the development of cognitive psychology and neuroscience, behavioral ecology, and genetic and evolutionary studies of behavior. There continue to be worthwhile research programs on limited problems confined to single fields, but unlike the days when the study [*sic.*] nonhuman species was kind of a backwater in psychology, it seems to me a broad integrative approach to cognition and behavior is very much in the atmosphere.

In other words, the behaviorist paradigm of animal learning theory, once restricted by classical and operant conditioning methodologies, expanded to include cognition. However, Shettleworth insists that the Garcia effect had nothing at all to do with the cognitive revolution (personal communication, June 2021). But this could be a product of professional divisions, given she left the University of Pennsylvania and her specific focus was on animal studies, whereas the cognitive revolution was focused on humans. Rozin, on the other hand, believes that Jerry Fodor's (1983) *Modularity of Mind* reveals a correspondence between the cognitive revolution and what Garcia was doing in psychology (personal communication, June 2021). Even before Fodor's seminal book, Rozin suggested that Garcia discovered a module or functional "program" that works to solve a specific problem (Rozin, 1976)—namely, learning to avoid foods that cause gastrointestinal upset.

Any reference to belief, expectation, or anticipation had been dismissed from behaviorist learning theory, which, again, worked according to the assumption that there were no intervening states between stimulus and response. Exposure to these various new theoretical developments may have made Garcia's results more palatable. These researchers were preparing a groundwork for the revolutionary view in psychology that learners are the products of evolution by natural selection, and thus adapted and predisposed to respond differently to different stimuli, and can store information about stimuli without emitting an immediate response.

Garcia's results were incorrectly dismissed. The ideas developing during this time, however, enabled the Garcia effect to be taken up along with the broader story that there was more to learning than simple association between stimuli, namely, there were intervening states that impacted whether and how an animal can learn. The Brelands' widespread impact opened the way for the ideas that there are biological constraints on animal learning and that conditioning isn't a simple associative phenomenon, but rather is more cognitive and involves predictive anticipation. Seligman's endorsement along with replications demonstrating the taste-aversion effect gave Garcia's ideas professional respectability, enabling his work to be widely recognized, accepted, and ultimately celebrated.

I would suggest that rather than one theory wholly displacing another, Garcia's novel experimental procedures wedded the traditional behaviorist learning and ethological paradigms of research. According to this interpretation, the Garcia effect did not lead to the abandonment of classical and operant conditioning, but only overturned the imperialism of their central principles of learning: (1) repetition, (2) contiguity, and (3) stimulus equipotentiality. Prior to the celebration of Garcia's work, Barry Schwartz (1974, pp. 183-184), in his review of Seligman and Hager's *Biological Boundaries of Learning*, said:

Ethology and the experimental analysis of behavior are both fundamentally concerned with the origins of adaptive behavior, and they should be able to contribute to each other's development. Fortunately, a rapidly growing set of laboratory observations over the last few years may provide the basis for a new dialogue between ethologists and psychologists. These observations suggest a significant contribution by species-specific behavioral characteristics to the phenomena obtained within the context of the experimental analysis, and help bridge the

methodological gap by providing a substantial data base for the interconnection of ethological principles with the principles derived from the experimental analysis.

Schwartz (1974, p. 185) says that the “hallmark study” in this area is Garcia and Koelling’s (1966) study on taste-aversion learning, which demonstrated differential associability and the biological preparedness of associations. Garcia used learning theory’s experimental analysis to demonstrate an ethological principle: that organisms have evolved to associate illness stimuli selectively with gustatory and olfactory stimuli—the modalities used to sense food. Garcia’s ideas brought with them the ethological merit of recognizing there was more to the objective picture of learning behavior than classically and operantly conditioned associations. In Seligman’s (1970) article, he speculated that the laws of learning vary with the organisms degree of biological preparedness for an association. In James Gould and Peter Marler’s (1987, p. 4) letter *in Scientific American*, they argue that Garcia’s work was a “severe blow” to the Skinnerian behaviorist paradigm, and “welcome the new synthesis that is now developing between ethology and modern psychology.” This is a reply to James Todd’s (1987) letter, which recognizes that Watson and Skinner were more open to instinct than appears to surface, and argues that behaviorism has earned its keep, and that the synthesis is occurring between behaviorism and ethology. Indeed, through Garcia’s experimental results, the behaviorist’s experimentalist tactic merged with principles of ethology. Even so, core principles of behaviorist learning theory were rejected in light of the Garcia effect.

However, even after Garcia was celebrated in 1980, the old guard remained dismissive. Skinner (1983) dug in his heels, insisting that, “There is nothing in the Garcia Effect that contradicts any part of an operant analysis or throws into question any established facts” (p. 14). Rozin conveyed to me that Bitterman never made the conversion either (personal communication, June 2021). This suggests that Garcia’s ultimate reception may have been driven by younger scholars, rather than by those who were already committed to behaviorism. Garcia’s own generation did not seem to appreciate the importance of his work, while those without prior commitments ultimately embraced him as one of the most impactful psychologists of all time.

I have not offered conclusive proof of the thesis that Garcia was marginalized; but I hope to have provided enough grounds for reflection. My modest goal here has been to motivate further inquiry into similar cases of apparent stagnation to understand the way that science responds to ideas and individuals falling outside the scope of mainstream respectability. The evidence

presented here, while incomplete, suggests that views departing significantly from current orthodoxy are sometimes marginalized and met with resistance and await uptake from more receptive future generations. The philosophy of science would do well to examine such cases in detail to better understand the mechanisms of scientific change.

## **5. Conclusion**

The case of Garcia demonstrates that theories on the margins can suddenly become embraced due to transformations of thought in adjacent subfields that make the marginalized theory more welcome. In this case, Garcia's interpretation of his results and their import for behaviorist learning theory were supported by developments in not just animal behavior studies and ethology, but also in computational science, artificial intelligence, cybernetics, and the emerging field of cognitive science. Mainstream learning theorists resisted Garcia's results because his results were unexpected and new: they contradicted central tenets of mainstream learning theory. It is not just that Garcia was ahead of his time. His findings clashed with his times, and the intellectual environment needed to evolve under other pressures before a receptive audience could be found. For younger researchers with less entrenched ideas, Garcia's break from behaviorist orthodoxy seems to have met with less resistance, and their efforts to extend his work with more professional polish clearly made a crucial difference. Garcia's rejection of behaviorist assumptions in 1966 produced only a trickle of citations initially, mostly from emerging researchers, but, with their endorsements and extensions, the following decade would see a steady rise of interest, and by 1980, Garcia was recognized as a trailblazer.

Garcia was not an expert in the field that his work revolutionized. When he first made the observation that landed him his hypothesis, he was without a Ph.D., was working as an assistant at a naval research laboratory, and was not trained in traditional learning theory (Revusky, 1977, p. 62). The reception of the Garcia effect reveals how slow and clumsy the uptake of theoretical advancements that originate with an amateur working on the margins of a scientific field can be. Mainstream learning theorists suffered from theory-entrenchment to the extent that they generally failed to acknowledge evidence that ran counter to their theoretical expectations. As Revusky (1977, p. 54) put it in his case study of Bitterman's resistance to Garcia's results, "If a need to disbelieve radically new findings which extends far beyond rational conservatism can be

demonstrated in one leading scientist, it becomes tenable to suppose that similar needs are common among influential scientists and tend to interfere with scientific progress.” I believe this supposition merits further investigation into episodes of theory change in the history of science.

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