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*John Garcia*

# Psychology Is Not an Enclave

# 4

The stuff of my psychology has been all around me since I was born. My mother and father were aliens from a different world, the northwest corner of Spain, a misty mountainous region cut by fjords where rivers drain into the Bay of Biscay and the Atlantic Ocean. Ethnographers refer to the old country as "Celtic Spain." They met and married in northern California while working in the vineyards. Pop was unschooled, but he was intelligent, pragmatic, and candid. Mom left school after the fifth grade, but she was bright, sparkling, and self-educated in English and Spanish. I was a child when she taught me that her chocolate aversion was due to seasickness, foreshadowing my career. As farm workers, we were drawn to compadres from other Hispanic regions and gained empathy for other minorities, including our interned Japanese friends. Mom and Pop raised six strong healthy sons: Frank, John, Ted, Ben, Bob, and Dick. I was Mom's built-in baby-sitter. I learned that babies do not babble, that they talk in sentences before they have mastered words, and that the second decade of life is a confusion of changing social and sexual roles.

I was set in my way of learning by 1929, when I was 12; thereafter, I took from teachers and books only what agreed with my set way. Domestic animals taught me conditioning before I ever heard of Ivan Pavlov. Fishing, hunting, and staring at wildlife through field glasses taught me ethology before I knew Konrad Lorenz. I met Inez Robertson in a dance hall with a big swing band playing in turbulent times circa 1940. I was a mechanic and she was a smart spunky teenager when we decided to go the distance together. Working on company trucks, navy ships, and army planes gave me a knack for devising experimental laboratory equipment. When I was unemployed, I attended Santa Rosa Junior College sporadically. In the Army Air Corps, I was an omnivorous reader like Mom. Leo Tolstoy, Sigmund Freud, Charles Darwin, and

Robert Woodworth attracted me to psychology. In 1946 at age 29, veterans benefits gave me the wherewithal to enter the University of California in Berkeley as a full-time probationary third-year undergraduate majoring in psychology and minoring in zoology. I knew animals had brains, minds, and ways of coping with the natural world; thus, the cognitive behaviorism of Edward C. Tolman and David Krech was infinitely more congenial to my thinking than the eviscerated behaviorism of John B. Watson, B. F. Skinner, and Clark L. Hull. In 1951, goaded by a growing family and tired of PhD exams, wherein I failed to convince academics of true animal ways, I reverted to my old ways of learning on the job for pay.

## *Radiation Research*

For the next 17 years I worked in a succession of multidisciplinary labs: the U.S. Naval Radiological Defense laboratory in San Francisco, the University of California at Los Angeles (UCLA) research facilities in the Long Beach Veterans Administration (VA) Hospital, and the Harvard Medical School facilities in Massachusetts General Hospital. At the Navy radiology laboratory on my first day, I was summoned before the acting director, a biochemist who harangued me with that specious hierarchy of sciences, graded according to their use of the mathematical idiom, with physics and chemistry on top, psychology on the bottom. He also opined that psychology was a weak and useless pawn in radiation research. I had weathered countless such interdisciplinary hassles at Berkeley, so as I stood up to leave I shot back, whether a biochemical fact be vital or trivial depends on perception and judgment, two processes in the purview of psychology, the queen of sciences.

Bob Koelling, a hospital corpsman at the time, became my radiation research partner. Our breakthrough came after we learned what many people already knew: that rats exposed to an extremely low flux of ionizing radiation for hours progressively decrease their food and water consumption during repeated exposures. Because the rats showed no signs of illness, no one was concerned.

The decrease looked like a learning curve to Koelling and me. We suspected the rats were discriminating the subtle difference between the water in plastic tubes used in the radiation room and the water in glass bottles in the animal room. We put plastic tubes in both rooms and flavored the water in the radiation room with saccharin; the result was a strong conditioned taste aversion (CTA) for saccharin in one trial. When a distinctive place was substituted for the distinctive taste, avoidance was very weak and transient. I wrote a review replete with graphs and sketches directed at learning experts who paid no attention whatsoever (Garcia, Kimeldorf, & Hunt, 1961). Ultimately our search for the cause of CTA culminated at Massachusetts General Hospital, where we found that blood serum taken from irradiated donor rats and injected into recipients caused a CTA. The culprit is probably histamine released from the viscera (Garcia, Ervin, & Koelling, 1967).

Early on, others were unable to replicate our CTA effect in their radiation labs. We knew people thought radiation was an implacable force, not realizing that CTA was a learned response subject to distraction. So Caroline Wakefield and I varied the degree of habituation to the bustle of x-ray procedures. Animals given much habituation acquired a CTA in one saccharin-x-ray trial. Less habituation yielded ambiguous data (Garcia, Buchwald, Feder, & Wakefield, 1962). That little study won me a trip to the International Energy Agency in Vienna, Austria. At the reception, I was totally unknown until two senior Soviet scientists spotted my name tag and hustled me to an American who understood their excited jabber. The amazed American informed me that the Russians were saying that they got their information on the effects of extremely low doses of radiation from me. Later at the opening session in the grand United Nations auditorium, where instantaneous translations of foreign languages reached our earphones, the Soviet keynote speaker singled me out, along with my friend C. S. Bachofer, a Catholic priest and scientist, congratulating us for our demonstrations of the effects of low-dose radiation. A young English scientist leaned over to me and whispered, "Damned awkward for you, old man, caught between the commissar and the priest."

At the Long Beach VA Hospital, we discovered that a brief flash of x-ray could warn a rat of a pending shock or arouse a sleeping rat. Probing the rat's head with a 45-mm beam through a tiny homemade collimator indicated that the most sensitive spot was the olfactory area (Garcia, Buchwald, Feder, Koelling, & Tedrow, 1964). Surgical tests by Chester D. Hull verified this implication (Hull, Garcia, Buchwald, Dubrowsky, & Feder, 1965). Tests with my most marvelous and wacky invention, an x-ray tachistoscope, indicated that a blip of less than 10 milliroentgens resulted in odor detection (Garcia, Schofield, & Oper, 1966).

Radiation scientists, like psychologists, are often resistant to new facts and oblivious to historical facts. Circa 1967, I was invited to present a seminar at the National Radiation Facility at Oak Ridge, Tennessee. A radiologist treating patients with low doses of ionizing radiation discounted their comments of olfactory sensations. That was several years after we reported such olfactory effects in *Science* and *Nature*. As I took the podium, my audience of radiation experts was buzzing about the news that astronauts on an Apollo mission reported visual effects from solar flares. That was several years after visual effects caused by cosmic hits on the retina were reported in *Nature* and about 70 years after visual effects of x-ray were historically recorded by Wilhelm Roentgen and others (Garcia y Robertson & Garcia, 1985).

## *Dual Data for a PhD*

In 1965 at age 46, I filed a psychology thesis designed to awaken behaviorists from their dogmatic slumbers. The experimental set-up was simple. The touch of a rat's tongue on a waterspout produced a bright noisy flash and a sweet taste. Rats punished by immediate shock to the feet feared the noisy flash but ignored the sweet taste. Rats punished by delayed nausea ignored the noisy flash but rejected the sweet taste. Two behavioristic "laws" were abrogated: the law of effect, declaring that signals and reinforcers were transituational, and the law of contiguity, declaring that immediate reinforcement was necessary. Two reports based on my thesis were rejected by the *Journal of Comparative and Physiological Psychology*. One reviewer intimated that I did not know what was going on in Europe, so I sent copies to Jerzy Konorski in Poland and Konrad Lorenz in Austria. Konorski said that they were very pleased to see my data; Lorenz said that we had demonstrated what he had merely postulated. Then I sent abbreviated versions to *Psychonomic Science*, which accepted them without review (Garcia, Ervin, & Koelling, 1966; Garcia & Koelling, 1966). *Radiation Research* accepted a report on the ancillary thesis data (Garcia & Koelling, 1967).

A storm of criticism ensued. It was said that the CTA experiments lacked proper controls; were due to conditioned stimulus–unconditioned stimulus (CS–US) similarity; and would be of little use to wild rats, ruminant grazers, or carnivorous predators. All these allegations were proven false when CTA was demonstrated in various species ranging from mollusk to human. I was accused of being the instigator of CTA, but in fact CTA had been reported in 1538 by Juan Luis Vives, in 1690 by John Locke, in 1871 by Charles Darwin, and in 1887 by E. B. Poulton (Garcia & Riley, 1998).

## *Dual Brain Systems*

Soon after I published my thesis data I discovered that the behavioral duality is subserved by a neuroanatomical duality described by C. Judson Herrick (1961). Auditory and cutaneous stimulation converge to a dorsal brain system to evoke motor responses in defense of the skin.

Taste and nauseous stimulation converge to a ventral brain system to evoke autonomic reactions in defense of the gut. Olfaction and vision can access either system through a gating system controlled by the attention of the animal (Garcia, 1990).

My thesis study may have been the most replicated experiment since Pavlov used tone-sour to elicit conditioned salivation in the dog. We conducted several dozen replications ourselves in which we changed conditions radically. Brenda McGowan substituted the size of food pellets for the noisy flash and substituted the flavor of the dry pellet for the sweet water confirming the original results (Garcia, McGowan, Ervin, & Koelling, 1968). Ken Green used a brief bout of illness preceded by one flavor, and followed by a second flavor before recuperation, obtaining a CTA for the first flavor and a conditioned taste preference (CTP) for the second flavor (Green & Garcia, 1971).

The most dramatic use of CTA was initiated by Carl Gustavson, who had a keen mind tuned to the ways of foraging wild animals in his native Utah. Carl essentially fed coyotes mutton paired with nauseous treatment to induce a mutton CTA, which caused coyotes to avoid living lambs (Gustavson, Garcia, Hankins, & Rusiniak, 1974). Carl went on to test baits formed of mutton and sheepskin laced with lithium chloride to reduce lamb predation by wild coyotes on the range (Gustavson, Jowsey, & Milligin, 1982). Mutton CTA was validated by Stuart Ellins in a pristine study completely blocking coyote predation on lambs in Antelope Valley, California (Ellins, Catalano, & Schechinger, 1977). On the flip side, Fred Provenza used CTA theory to study the feeding behavior of goats foraging on pastoral lands, where poisonous plants abound, thereby discovering a subtle form of CTA developed by domestic stock without exhibiting overt signs of toxicosis (Provenza, 1995).

The most fishy use of CTA was by experimental psychologists who aped our experiment in which we gave rats a choice between sweet water in the white arm of a T-maze and tap water in the black arm. After the rats habitually ran to the white arm for the sweet reward, we imposed a CTA for sweet in the home cage. When they recovered, we returned them to the T-maze and they persisted in choosing the white arm as if they were unaware that the sweet water was now disgusting (Garcia, Kovner, & Green, 1970). CTA was renamed "instrumental responding for devaluated reinforcers," a tacit admission of the neural duality, but the perpetrators did not discuss Herrick's neuroscientific explanation (Garcia, 1990).

## *Dual Learning Systems*

When I returned to UCLA in 1973, I recalled Tolman's (1949) distinction of two kinds of learning, which corresponded to Herrick's dual brain systems: (a) cognitions, wherein animals acquire spatiotemporal maps of the environment to acquire needed objects, and (b) cathexes, wherein animals adjust the hedonic values of objects according to the feedback (FB) from object consummation. Tolman used *cathexes* broadly, including hedonic evaluations of food, drink, and sex objects (Garcia, 1989). His choice of a psychoanalytic term that implied the unconscious was prophetic. CTA is produced even when the nauseous FB is injected into an anesthetized animal. Similarly, pain induces an endogenous analgesic FB in anesthetized animals. In thermal regulation, where core temperature provides an FB, our bedmates hog all the blankets on cold nights and kick them off on hot nights without waking up. As for sex, who can say that sleeping with our mates does not increase our attachment to them (Garcia, 1990)?

Many good graduate students were attracted to my UCLA laboratory. With all that youthful energy available in the lab, I spent time in my office doodling on paper attempting to synthesize Darwin, Pavlov, and Tolman into coherency. First I had to abandon a popular opinion, to wit, a US is merely the second term in a CS–US pair and the essential difference between a US and a CS is merely intensity. For example, an extremely loud sound evoking avoidance is a US, whereas a soft sound evoking attention is a CS. This view may seem reasonable, but it is a behavioristic evasion of neuroscience. Auditory sounds of any and all intensities project to the dorsal brain system concerned with skin defense. And all taste stimuli of whatever intensity or quality project into the ventral brain system concerned with gut defense.

Pavlov chose taste as his US and an external stimulus as his CS because he was interested in studying "psychical experience,"or as Tolman labeled it, "cognitions" (Kaplan, 1966). On the other hand, Darwin was interested in affective processes, more akin to Tolman's cathexes. I doodled a three-element combination, CS–US–FB, joining Pavlovian CS–US and Darwinian US–FB; another way of expressing this is that I joined Tolmanian conscious cognitions and unconscious cathexes. UCLA students, particularly Kenneth Rusiniak and Claire Palmerino, provided substantiation for my CS–US–FB doodle. They demonstrated that odor alone is a weak cue for nausea but that odor attended by taste becomes a powerful potentiated cue for nausea. The prime function of a US is to convert an irrelevant stimulus into a relevant CS (Palmerino, Rusiniak, & Garcia, 1980). They also demonstrated that a pain US can block a food odor US by urgently gaining the animal's attention, thus neurally gating the food odor into the skin defense system where it is unavailable for gut defense (Rusiniak, Palmerino, Rice, Forthman, & Garcia, 1982).

Linda Philips Brett took on the daunting task of testing buteo hawks. These large, powerful avian predators were of special interest because they hunt from far above with their keen vision and seize prey with taloned feet without tasting their prey. Black and white mice were used, but the hawks could not use color to avoid the black mouse repeatedly paired with nauseous injections, presumably because lab mice taste alike regardless of color. However, when the black mouse was marked by a distinctive taste, the hawks avoided it from a distance after a single trial, indicating that the taste US had potentiated the color CS (Brett, Hankins, & Garcia, 1976).

Debra Forthman won a Fulbright award and ventured to test CTA as a deterrent to baboons raiding vegetable crops in Kenya, Africa. Under experimental conditions, Forthman achieved CTA for specific vegetables, but the political problems of getting permission from farmers and authorities for a broad program were as formidable as those encountered by Gustavson and Ellins on coyote control on United States sheep ranges (Forthman Quick, 1984).

Three graduates pushed our program into neurophysiology and endocrinology. Stephen Kiefer brought his expertise on odor stimulation to our lab as a postdoctoral fellow. Kiefer placed rats in a glass wind tunnel where odors could be presented and whisked away with the same precision as visual and auditory stimuli (Kiefer, 1985). Janet Coil studied the contributions of the vagus nerve and blood circulation to CTA (Coil, Rogers, Garcia, & Novin, 1978). Coil also showed that antiemetic agents would attenuate CTA. Anne Rice, working with an estrogen-induced CTA demonstrated by Carl and Joan Gustavson, showed that antihistamine reduces estrogen CTA (Rice, 1988). Taken altogether, the Gustavsons, Coil, and Rice provided evidence that the anorexia most prevalent in adolescent girls may be due to premature estrogen onset.

Finally, Federico Bermudez-Rattoni, who earned his PhD in our lab with his research on the roles of the hippocampus and the amygdala on odor potentiation by taste, pushed the program into brain research. In his own lab at the University of Mexico, Bermudez-Rattoni and his students blunted the CTA capacity of adult rats by scooping out tissue from the gustatory neocortex. Subsequently, they implanted homotopic fetal tissue in the lesion and restored the CTA capacity of the brain-damaged adults. Recently, Federico and two other international scientists published a book on CTA emphasizing brain research (Bures, Bermudez-Rattoni, & Yamamoto, 1998).

## *Aversions and Affinities*

I rebelled specifically against the exclusion of neuroscientific explanations advocated by a triumvirate of learning theorists circa 1947 to 1959.

K. W. Spence (1947) wrote, "In the case of learning phenomena, a number of theoretical interpretations have been offered which make little or no use of neurophysiological concepts." W. K. Estes (1959) concurred, "all empirical independent variables (causal variables, antecedent conditions or determinants of behavior) which enter into behavioral laws influence behavior by way of stimulation." B. F. Skinner (1959), displaying cumulative lever presses by three different hungry animals working for their food delivered on a fixed-interval schedule, asked, "Pigeon, rat, monkey, which is which? It doesn't matter." It mattered very much to me! (A more complete discussion with references can be found in Garcia, McGowan, & Green, 1972.)

I cling to an older more inclusive way of thinking. In 1690, physician John Locke asked, "Let us then suppose the mind to be, as we say, white paper void of all characters, without any Ideas. How comes it to be furnished?" (1690/1975, p. 104). Locke's response to his own question was that two sources were involved: (a) sensations of external objects and (b) reflections stemming from internal effects (p. 105). He distinguished associations acquired by chance or custom from natural associations dependent on our "constitution." Later, Locke gave this example: An emetic substance acting on the palate is sweet; when it subsequently acts on the gut it is sickening, a natural association now called a CTA (p. 138). (For more on the neuroscience of Locke, see Garcia, 1981b.)

In 1904 at the end of his Nobel Prize address, Pavlov explained his reason for studying the brain:

> In point of fact, only one thing in life is of actual interest for us —our psychical experience. But its mechanism has been and still remains wrapped in mystery. All human resources—art, religion, literature, philosophy, and historical science—have combined to throw light on this darkness. Man has at his disposal yet another powerful resource—natural science with its strictly objective methods. (cited in Kaplan, 1966, pp. 56–57)

Pavlov's message is as timely today as it was a century ago. Psychology and neuroscience are conjoined twins at the center of the seamless science of life, and the psychologist must follow his empirical path wherever it may lead.

## *Irks, Quirks, and Perks*

The psychological establishment is not a monolith; it is more like a parliament made up of small fractious parties. When I started out it was said that there were 7 psychologies, but now I guess there must be 14 or more. If one journal does not accept a contribution, another one might, or someone editing a book might accept a chapter. That was ever my strategy. I just kept hammering away with more evidence. It was great fun! Circa 1980, a grotesque fate befell me! I was an author on 15 articles in *Science* or *Nature*! Plaques on my walls flaunted establishment status! A perennial dropout made it all the way to the National Academy of Sciences!

In one year, the *American Psychologist* invited me twice to submit articles. In the first review, I tried to defuse the ruckus about my rejections with humor (Garcia, 1981b). That was my most requested article. Many requesters commented that they had also been rejected unfairly. I came to abhor anonymous reviews. I insisted on signing my critiques and met some interesting people in the bargain.

My second review was about mental aptitude testing. That has been my very least cited article (Garcia, 1981a). The notion that any behavior can be divided into two additive components with the main part attributed to heredity is biological nonsense; environment overpowers heredity. The most genetically gifted infant cannot flourish in a dark closet. Evolution teaches us that the environment selects the genes for survival. Paleontology teaches us that genetic constitutions cannot withstand vast environmental changes. Those who attribute performance on tests made up of cultural bits to genetic bits have an overweening faith in genetics, where they know too little, and a pathetic lack of faith in psychology, where they should know much more (Garcia, 1981a).

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