Tilting at the Paper Mills of Academe

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ABSTRACT: *Errant authors of empirical learning reports are often dashed to earth by editorial reviewers, who castigate them for trivial departures from the orthodox associative paradigms subserving a vacuous general-process notion, which allows no consideration of specialized structures in humans or in beasts. Paradigmatic illustrations of immediate reinforcement were not subjected to the same zealous scrutiny, thus some classic experiments proved to be classic blunders. Immediate-reinforcement notions owe more to simplistic ideas of profit in a "free-market" economy than to empirical associationism. John Locke did not assume an unstructured neonate mind; he recognized natural connections as well as nurtured ones. He proposed neurological specificity of sensations over a century before J. Müller and noted the existence of specialized taste-illness pathways nearly three centuries before J. Garcia.*

My material is drawn from the field of learning and my title from a misadventure of that legendary knight, Don Quixote de la Mancha (Cervantes, 1605/1867?). Sighting 30 or 40 large windmills newly established in South Central Spain during an energy crisis, when long droughts had stilled the waterwheels on the Rio Zancara, Don Quixote perceived them to be giant demons, threatening him with their multiple arms. He spurred his steed, Rocinante, charging the mills in God's good service to wipe so evil a breed off the face of the earth. The errant knight and his noble steed were swept into the air by the giant arms and dashed to earth again. The good squire, Sancho Panza, picked up his befuddled master and offered him a mechanistic and pedestrian explanation: "They are only windmills." But Don Quixote defended his more global dynamic theory, proclaiming, "The same evil influence which reduces my monetary support and denies me books and space would like us to believe that its monstrous lackeys are mere grinders of corn in order to rob us of the glory of exposing their true malignant nature." Some researchers feel the same way about journal editors and their consultants. But, like Sancho Panza, I hold a simpler view. I have studied editorial behavior for years, and I have come to the conclusion that journal editors are neophobic creatures of our own kind.

The author's confrontation with the editors often begins not with paranoid delusions, but with great hope and expectation. The author submits the final product of an arduous writing and rewriting process and receives a warm note of thanks from the editor. Then, after many months, the second editorial response finally arrives. It is apt to be a supercilious sophistry bearing so tenuous a relationship to the manuscript that the author concludes the consultants must have been out to lunch when the paper was being reviewed. 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. (Unfortunately, since the review was anonymous, I was unable to properly congratulate the consultant on his or her high academic standards.) The dissonance produced by the first courteous response and the second caustic one leads many authors to believe journals are governed by Janus-faced demons, but I present evidence indicating that journals are actually operated by timid but tractable organisms.

On the Neophobia of Editorial Consultants

After a decade of successful radiobiological research, during which my associates and I published a series of some 20 papers in prestigious journals and volumes without a single rejection, I felt I was

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The first three illustrations in this article were taken from Cervantes's *The History of Don Quixote* (J. W. Clark, Ed.; G. Dore, Illus.; 1867?). The fourth illustration was drawn by Professor Garcia's son John E. Garcia, a student at the University of California, Los Angeles.

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ready for the big leagues. Our radiation research convinced me that two classic principles of conditioning were of limited generality; although not wrong, I felt they were simply not necessary for all conditioning. I wanted to record my views for posterity in the annals of my first scientific love, the field of learning.

First, I felt that unconditioned stimuli (or reinforcers) have a selective effect on what is learned. We tested this proposition in a double-dissociation design in which two cues, a sweet taste or a clicking light, were made contingent upon the licking of a drinking spout by thirsty rats. Drinking under these conditions was punished by either shock to the feet or illness produced by X rays. Shock produced an avoidance of bright, noisy water but not of sweet water; conversely, illness produced an aversion for sweet water but not for bright, noisy water (Garcia & Koelling, 1966). Our selectivity hypothesis was supported, but our paper was rejected by several journals. Some editorial consultants said we used too many treatments. Others said we used too few. One said we did not know how X-ray reinforcement worked. Apparently, this consultant was satisfied that we all know how shock reinforcement works.

So we went back to the laboratory and repeated the experiment in a different guise. The two cues were now the size or the taste of food pellets presented to hungry rats. The two punishments were the same. So were the results. Shock produced an avoidance reaction to the size cue but not to the taste cue, whereas illness produced an aversion for taste but not for size (Garcia, McGowan, Ervin, & Koelling, 1968). This paper was promptly accepted by a journal that had refused the first paper, indicating that journals are operated by neophobic creatures that habituate in one trial.

The second classical principle that I felt has limited generality is contiguity. Immediate reinforcement is simply not necessary for learning when illness is the reinforcer. We tested this proposition by giving rats a drink of sweet water and injecting them with an emetic drug after various delays. Delays of up to 30 minutes had little effect on the strength of the taste aversion (Garcia, Ervin, & Koelling, 1966). Our hypothesis on the effectiveness of delayed-illness reinforcement was clearly demonstrated, but this paper was also rejected by two journals on the first trial.

Again, we gave the editors a second trial. This time thiamine-deprived rats drank sweet water and then received thiamine after various delays. Because we used a beneficial injection rather than

SCIENCE rejected our paper again

a noxious one, the rats increased their consumption of saccharin, but again delays of 30 minutes had little effect on learning (Garcia, Ervin, Yorke, & Koelling, 1967). This paper was promptly accepted by a journal that had rejected the first paper. Editorial neophobia followed by rapid habituation approached the status of established law.

The Veneration of Procedure

More recently, we discovered some limitations of a third established principle of conditioning called overshadowing or blocking. This phenomenon is observed when two cues are combined into a compound signal; the stronger, more reliable cue usually overshadows, or blocks, conditioning to the weaker, less reliable cue. This makes perfectly good sense; if an animal has valid, reliable information, why should it bother to learn invalid, unreliable information? (I always use anthropomorphism and teleology to predict animal behavior because this works better than most learning theories. I could rationalize this heresy by pointing to our common neurosensory systems or to convergent evolutionary forces. But, in truth, I merely put myself in the animal's place. I cannot think in the cryptic jargon of learning; obviously, neither can editorial consultants.)

In our laboratory, Ken Rusiniak found that when a weak odor cue was combined with a strong taste cue and followed by illness, taste did not overshadow odor. Taste did exactly the opposite; it potentiated odor, converting it into a strong cue (Rusiniak, Hankins, Garcia, & Brett, 1979). Linda Brett observed a similar effect in hawks eating mice. The black color of the mouse's coat was a useless cue for poison. After eating a black mouse and suffering a toxic injection, hawks rejected both black and white mice. However, when the black mouse also tasted different, the poisoned hawk retreated wildly from the next black mouse on sight, but accepted white mice avidly (Brett, Hankins, & Garcia, 1976). Chris Clarke and his associates in Australia designed an elegant experiment using blue and/or salty water to demonstrate the potentiation effect in pigeons (Clarke, Westbrooke, & Irwin, 1979).

The species differences fascinated us and our Australian colleagues, so in our first publication attempt, we combined the rat and pigeon data into one paper and sent it off to a journal. We showed that almond-scented water was a very weak cue for the poisoned rat but that sweet water was a strong cue and that blue water was a poor cue for the poisoned pigeon but that salty water was a good one. When the weak cues were combined with the strong taste cues, however, the rat acquired a strong aversion for almond-scented, unsweetened water, and the pigeon acquired a strong aversion for blue, unsalted water. The data were clear, but the paper was rejected.

The species differences did not fascinate the editorial consultants as much as the methodological differences between the rat and pigeon studies disturbed them. One editorial consultant said we used unorthodox procedures, complaining that our methods were neither blocking nor overshadowing procedures. In the former case, one stimulus is made stronger than the other by prior association with the reinforcing unconditioned stimulus, and in the latter, two stimuli differing in salience are presented together. These procedural criticisms are irrelevant on two grounds. First, the principle of overshadowing and blocking is a general rule that states a relationship between signal elements of different effectiveness. Our study belonged to that category. Second, there are no empirical or theoretical grounds for expecting that our departures from orthodox procedures would turn overshadowing into potentiation, nor did the consultant provide any such evidence or logic.

Another editorial consultant said I had a history of presenting important but flawed research and wrote, "The danger here is not especially severe if the initial report is in error. That will be quickly discovered by others. The real danger comes just when the work is substantially correct but the original work is flawed. For others will lose time and effort tracking down those flaws." Apparently, it never occurred to him that if the research proved to be substantially correct, then the flaws would be proven quite trivial.

The third referee was impressed because we replicated the potentiation phenomena with different species, in different sensory systems, with different methods, and in different laboratories on different continents; all this, he said, attested to the reliability of the phenomena, even if the study was a bit untidy. But we lost the split decision, so we published the rat and pigeon data in separate papers, side by side, in a journal that welcomes species differences and anatomical explanations (Clarke et al., 1979; Rusiniak et al., 1979).

By this time we had become adept at habituating editors. Claire Palmerino designed an experiment complete enough to satisfy the most fastidious learning methodologist (Palmerino, Rusiniak, & Garcia, 1980). She used a single acquisition trial flanked by pre- and posttests and conducted two experiments employing 28 groups of rats. She balanced order effects and precluded explanations on the basis of prior odor–taste associations (sensory preconditioning) or nonassociative effects of illness (pseudoconditioning), demonstrating that odor alone has a steep delay-of-illness gradient and that odor aversions are possible with immediate reinforcement but are completely abolished by delays of 30 minutes. However, when odor was potentiated by taste, the gradient of odor tested alone in extinction resembled the long delay gradient of taste; a two-hour delay had no effect on the aversive reaction to a potentiated odor. This paper was accepted by a journal after one prior habituation trial.

The Ritual of Quantification

Learning consultants are obsessed with measurement and quantification, often to the detriment of psychological variables. Rats can learn to use flavor to avoid shock, although they use the odor component more effectively than the taste component; in this case, the flavor becomes a sign for shock. When flavor is followed by illness, inhibition of drinking is much simpler because rats simply do not like the flavor; an affective change or hedonic shift occurs (Garcia & Rusiniak, 1980). This distinction cannot be inferred from the quantity of fluid consumed, but it is quite obvious from the behavior of the rat: It gapes, retches, and rubs its chin on the floor. But such vulgarities upset those who prefer pristine numbers. One consultant said that he did not understand the meaning of "hedonic change." I doubt that he was totally ignorant of the long psychological tradition that stretches from Bentham's (1823/1907) hedonic calculus to Young's (1966) elegant isohedonic contours. I suspect he was trying to exorcise the rat's feelings (and sensory physiology) out of the "objective" approach to learning.

The one-bottle taste test is often criticized on the grounds that it is a less sensitive measure than the two-bottle test. This point is trivial and untrue, as a moment's thought reveals. While pigeons and other animals are able to make the more direct simultaneous discrimination visually, the rat always makes successive discriminations with its tongue. In the two-bottle test, the rat controls the order and duration of stimulus presentation, and this can be a disadvantage.

Another quantitative criticism reflexively elicited by comparisons of foot shock vis-à-vis internal

I should have run one more control group.

illness is that the two punishments are not delivered under equivalent stimulus parameters. In other words, internal nausea differs from peripheral shock pain in its quantitative pattern as well as in its sensory properties. A number of researchers have varied the anatomical locus and time–intensity patterning of electrocutaneous shock to mimic the effects of illness on licking, but to no avail; the selective effect of the reinforcers is unchanged (see Garcia, Palmerino, Rusiniak, & Kiefer, in press, for a discussion). Therefore, the logic that calls for equivalent quantitative parameters must be suspect. So is the logic that calls for adherence to orthodox paradigms and procedures. They stem from a compulsion to snare an ephemeral phenomenon called "general-process learning" in a trap composed of operational definitions and control groups without regard to the biological structure of the learning beast.

Pseudoconditioning and Pseudocriticism

By this time it must be obvious that I am peevish about procedures, that I am sensitive about sensitization, and that I am sick of pseudoconditioning. The very term *control group* is a misnomer. All groups are given a specific experimental treatment to establish a specific point. Not long ago, a taste aversion paper was published which presented findings that were neither novel nor interesting, yet the paper led to a prolonged series of letters to the editor (Mitchell, Scott, & Mitchell, 1977). Almost everyone in taste aversion research got into the act (see Notes and Comments, *Animal Learning & Behavior*, 1978, *6*, 115–124; 1979, *7*, 562–563). The above authors presented a six-group experimental design. Half the groups received an associative treatment, namely, sweet water followed by injection. The other half received a so-called nonassociative treatment, namely, unsweetened water followed by injection. Three injection parameters were employed: a zero dose, an immediate toxic dose, and a delayed toxic dose. Significant differences were claimed for the associative treatment; no significant differences were reported for the so-called nonassociative treatment. What caused the furor was that the authors chided other researchers in the field for failing to use nonassociative control groups.

At about the same time, a similar criticism appeared in a letter to *Science* (see reply by Bitterman in Garcia, Hankins, & Rusiniak, 1976): "There were no pseudoconditioning controls, yet illness might have produced aversion to saccharin, and shock might have produced aversion to the sound of a buzzer, quite independently of pairing" (p. 265). Actually, our first paper on taste aversion learning published in *Science* (Garcia, Kimeldorf, & Koelling, 1955) employed the same six-group design; half the groups received an associative treatment (sweet water and radiation), and half the animals had the so-called nonassociative treatment (unsweetened water and radiation). The associative treatment produced a dosage-dependent aversion for the sweet fluid; the so-called nonassociative treatment did not produce a pseudoconditioned saccharin aversion. 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 experimental design and statistics courses, despite all rumors to the contrary.

I say "so-called nonassociative" because nonassociative learning procedures do not prevent the rat from making associations. If an unpaired stimulus is presented to the rat, it will associate that single stimulus with a specific point in time and space, if not with a prior stimulus event. In the study just cited, we tested all groups with two bottles (sweetened vs. unsweetened water). And the water groups displayed an aversion for water despite its familiarity, attesting to the prejudicial associative bias of the rat; for them water was, after all, the fluid paired with radiation illness.

At that time, I wanted to point out the water aversion in our report, but a wiser head prevailed. Kimeldorf said that we would have enough difficulty getting editorial consultants to accept a saccharin aversion induced by such low dosages; neither the learning experts nor the radiation experts, he said, were quite ready for a water aversion. Fortunately, no one noticed, or at least no one mentioned, the dosage-dependent increase in the saccharin preference of the water group clearly visible in Table 1, though it was constrained by the high preference ceiling for saccharin (Garcia, Kimeldorf, & Koelling, 1955).

Sensitization is generally assumed to be a nonselective arousal process that primes the animal to learn just about anything that comes its way (Malmo, 1959). However, it seems rather obvious that if an animal has the capacity to selectively connect taste to illness given the impoverished information afforded by a single trial in which the two sensations are separated by hours, it will also be selectively sensitized to novel tastes when that information is reduced even further to illness without prior taste stimulation (Domjan, 1977). After suffering foot shock, rats also prefer familiar surroundings to strange places (Aitken, 1972; Aitken & Sheldon, 1970).

Recently, Miller and Domjan (in press) demonstrated that rats suffering from lithium illness indeed showed a reduced preference for a novel sweet taste but not for a novel noisy light flash contingent upon licking. This taste sensitization was present 35 minutes, but not 6 hours, after the toxic injection. Conversely, shocked animals feared the noisy light flash but not the sweet taste. This external sensitization was present immediately after, but not 5 minutes after, the shock. The selectivity and the temporal relationships of the two forms of sensitization correspond to those of the two forms of conditioning. Both the nonassociative procedures and the associative procedures reveal the selective bias of the rat's mind and/or brain.

Much is made of novelty in taste aversion learning. While it is undoubtedly facilitatory in some cases (e.g., Revusky & Bedarf, 1967), it is no more necessary for taste–illness learning than for any other form of associative learning. Taste aversions for familiar fluids have been demonstrated in rats (Garcia & Koelling, 1967), for familiar prey in coyotes (Gustavson, Kelly, Sweeney, & Garcia, 1976), for familiar prey in hawks (Brett et al., 1976), and for familiar foods in children (Bernstein, 1978). In fact, animals suffering from chronic illness often exhibit neophilia, a preference for novel diets (Bernstein & Sigmundi, 1980; Rozin & Kalat, 1971). In any case, nonassociative procedures with their impoverished information produce only transient effects compared with the robust effects produced by associative information, as Miller and Domjan (in press) point out. Taste and illness can be separated by hours in a single acquisition trial, and the test can be conducted days after the illness (Garcia, Hankins, & Rusiniak, 1974). In fact, animals tested for the first time one month after illness exhibit no apparent loss in the strength of the aversion (Garcia, McGowan, & Green, 1972).

Uncontrolled Orthodoxy

Control groups do not seem to be required if the experimental results conform to the prevailing zeitgeist of immediate reinforcement and general-process learning. For example, the Guthrie and Horton (1946) experiment with one group of cats became a classic. These authors simply placed a succession of cats in a transparent box with a pole standing upright in the center. Time after time, cats rubbed up against the pole in a stereotypic manner, tripping the latch and releasing themselves from the box. By Guthrie and Horton's account, the rubbing response was an accidental one, learned because it was the last response to the locked box and thus was not subject to interference by further responding to that same situation. On returning to the locked box, the cat merely retrieved the last response made in the situation. By most other instrumental accounts, the rubbing response was learned because it was immediately reinforced by release from annoying confinement.

Both accounts were wrong, but for over 30 years, Guthrie and Horton's cats adorned our learning textbooks in stereotypic outline before Moore and Stuttard (1979) asked what would happen if cats were placed in a transparent box without escape or any palpable reward for rubbing the upright pole. The cats rubbed like all our pet cats do when we come home. Cats rub ecstatically against our shins or displace the greeting onto any pole, edge, or surface that is handy. We had the "control" group before us all those years, but failed to recognize its relevance, so well had the zeitgeist prepared us to accept the general associational power of immediate reinforcement.

The paper by Moore and Stuttard was not exactly welcomed by editorial consultants (Moore, Note 1). The simplicity and ingenuity of Guthrie and Horton's explanation were said to be convincing, even if their experimental evidence was not. One consultant simply could not understand that the cats in this study had no problem to solve, no response to learn, and no reward for rubbing against the vertical pole. He said that "very few psychologists would be interested in whether the *cat's learning in the puzzle box* is to be explained in terms of a modified law of effect or as a special case of species-specific response emerging in an unusual environment under unusual conditions" (Moore, Note 1, italics added). Another editorial consultant quoted Guthrie and Horton's (1946) comment: "To predict what a cat will do when it is placed in a puzzle box requires familiarity with cats (p. 37)." This damning statement leaves us with two alternatives: Either (a) Guthrie and Horton failed to recognize the most common social response of the cat, or (b) Guthrie and Horton deliberately put an artificial shinbone in the center of the box for the cat to rub against without discussing the expected response or providing a baseline control measure. Others argued that even if the rubbing response was a natural behavioral pattern, its rate of emission was no doubt increased by escape from the box. But as Moore and Stuttard (1979) pointed out, it was the shape of the response pattern that was at issue, not its rate; Guthrie and Horton provided no baseline rate, explaining only that the stereotyped form of the response was stamped into the cat's repertoire by immediate release from the box. "With such a powerful technology," said Revusky (Note 2), "one could, no doubt, teach a fish to swim." Much to their credit, the editors overruled their consultants and published Moore and Stuttard's paper (I feel that our treatment for editorial neophobia may have played a modest role here).

The Alien Wind From the "Free Market"

The prevailing zeitgeist was skillfully harnessed by Skinner (1938) when he rejected classical conditioning in rather blunt tones; real learning could not be studied in a restrained and passive dog whose attention was aroused by a bell and whose saliva was evoked by a squirt of sour water into the mouth. Pavlov, of course, was not primarily interested in drooling behavior. Conditioned saliva evoked by the bell was merely the peripheral evidence that a new element of learning had been

Friendly, No!
Conditioned by Contingent Reinforcement, Si!

established in the recesses of the dog's brain, a new pathway between the auditory analyzer and the salivary mechanism. But Skinner (1950) rejected the need for unspecified biological processes or gratuitous theoretical explanations. Behavioral change was the end point of learning, and reflexes automatically evoked by identifiable stimuli were not important for that end.

Skinner gave us a conceptual organism much more compatible with the active and pragmatic spirit of the American frontier. His pigeon, unrestrained though confined to a box, freely emitted a variety of responses on a stage set by environmental stimuli. The key light signaled only that the food was available. The pigeon had to work for it by depressing the key. The response was defined not by its internal neural connections, but by the excursion of the key, that is, by the effect the pigeon produced in the environment. And the way to modify that response product and its rate of output was to arrange a reinforcing payoff for the desired production goals. If the desired response was not in the array of products originally displayed by the pigeon, it could be shaped by immaculate conception, through reinforcing successive approximations to the desired product. Just as profit operates selectively to produce desired goods and services in a free market according to Smith (1776/1937), so reinforcement selectively shapes the productive behavior of a free organism according to Skinner (1938).

Any stimulus applied immediately after the response which, by empirical test, would increase response production was deemed a reinforcer. The nature of reinforcement and its effect on the pigeon's behavior were left unexamined; this was the fatal flaw that would ultimately dismantle Skinner's system. The general procedures were said to be applicable to any and all reflexes, in any and all organisms. There was no need to concern ourselves with species differences, with brain differences, or with reinforcer differences. The payoff schedule's the thing wherein we'd capture control of the organism.

So pervasive was this wind that blew in from economics that 30 years passed before Brown and Jenkins (1968) pulled the plug connecting the key to the feeder, thereby converting the Skinner box into a Pavlovian chamber. The key light came on and food followed with no work requirement, as in a social welfare program. But the pigeon labored at the lighted key anyway. If grain was signaled, the pigeon pecked at the key as if it were grain, and if water was forthcoming, the pigeon pumped at the key as if it were water. Apparently, Skinner's pigeon could no more resist pecking the signal when grain was imminent any more than Pavlov's dog could stop slobbering when vinegar was signaled (for a review, see Garcia, Clarke, & Hankins, 1973).

I am not arguing that no animal will work for pay. In the wide wide world of animal behavior one can probably find a behavioral metaphor for any socioeconomic system. The name of that game is *sociobiology*, wherein genetic investment strategy is invented for a social trait and the world is then searched for an animal metaphor, preferably an insect (Dawkins, 1976). For the middle-class free enterprise ethic, I recommend the California scrub jay (*Aphelocoma coerulesceus*). A pair of these wild blue jays appear promptly when my wife or I open the garden gate in the morning. They work for peanuts. They postpone gratification and save for the future. They match their energy expenditures to value received. Given a small discolored or cracked peanut, they hide it perfunctorily under a nearby leaf. Given a large, clean peanut well suited for storage, they fly off to deposit it in some faraway secret safe. Over the years, they have developed a mystical concept of us as the benign peanut givers and display toward us social behaviors unrelated to peanut acquisition. They greet us on our walks far from home where they never received a goober, and in the evening after work is done, they come to the garden and perch on the back of a chair. They fluff out their feathers in relaxed comfort and join us in conversation with soft chirps and warbles. The two birds have driven off all competitors and formed a stable partnership that has endured for over six years. It is apparently a small "mom and pop" peanut business, but thus far we see no evidence that they are willing to share it with their relatives or that they are preparing to pass it on to their children.

My complaint is that operant conditioning did not teach us much about pigeons. It remained for biologists to investigate the most fascinating behavior of pigeons. When they suspected that birds were using geomagnetic cues to navigate home, a magnetic sensor in the pigeon's brain was sought out (Walcott, Gould, & Kirschvink, 1979). Pavlov certainly would have approved. Those interested in learning should return to Pavlov's task, the search for detailed information on how new connections are made in the brain when an animal is subjected to training procedures. Those interested in unlearned adaptive behavior should search for detailed information on how that specific behavior is articulated to the niche and how that program of interaction is specifically encoded into the genes. Without such specification, Wilson's (1975) sociobiology is about as useful as McDougall's (1908) socioinstinct theory.

The Nature of John Locke

To resist the alien winds of our time, learning psychologists must look to our own historic roots in empirical associationism. There is no better place to begin than in Locke's "An Essay Concerning Human Understanding" (1690/1975). First, let us dispense with the canard that he was a radical environmentalist wedded to the "tabula rasa." This false notion stems from an assumption he made for the sake of argument in a passage in which he stressed that experience stems from two sources, nurture and nature. He wrote,

> 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? . . . Whence has it all the materials of Reason and Knowledge? To this I answer, in one word, From *Experience*: . . . Our Observation employ'd either about *external, sensible Objects; or about the internal Operations of our Minds, perceived and reflected on by our selves, is that, which supplies our Understandings with all the materials of thinking.* (Locke, 1690/1975, p. 104)

He went on to describe this second "fountain of knowledge" as an internal, natural source that contains the basis for the law of effect:

> This source of *Ideas*, every Man has wholly in himself: And though it be not Sense, as having nothing to do with external Objects; yet it is very like it, and might properly enough be call'd internal Sense. But as I call the other *Sensation*, so I call this REFLECTION, the *Ideas* it affords being such only, as the Mind gets by reflecting on its own Operations within it self. . . . The term *Operations* here, I use in a large sence, as comprehending not barely the Actions of the Mind about its *Ideas*, but some sort of Passions arising sometimes from them, such as is the satisfaction or uneasiness arising from any thought. (p. 105)

When Locke discussed the association of ideas, he distinguished between natural associations and acquired associations:

> Some of our *Ideas* have a natural Correspondence and Connexion one with another: It is the Office and Excellency of our Reason to trace these, and hold them together in that Union and Correspondence which is founded in their peculiar Beings. Besides this there is another Connexion of *Ideas* wholly owing to Chance or Custom; *Ideas* that in themselves are not at all of kin, come to be so united in some Mens Minds, that 'tis very hard to separate them. . . . I say most of the Antipathies, I do not say all, for some of them are truly Natural, depend upon our original Constitution, and are born with us. (pp. 395–396)

The Biology of Empirical Associationism

At times, John Locke wrote like a psychobiologist, anticipating the doctrine of specific energies of nerves over a century before it was elaborated by Johannes Müller and Von Helmholtz. And in the bargain he put forth a kinetic hypothesis of heat nearly half a century before Bernoulli:

> If we imagine *Warmth*, as it is *in our Hands*, to be *nothing but a certain sort and degree of Motion in the minute Particles of our Nerves, or animal Spirits*, we may understand, how it is possible, that the same Water may at the same time produce the Sensation of Heat in one Hand, and Cold in the other; . . . if the Sensation of Heat and Cold, be nothing but the increase or diminution of the motion of the minute Parts of our Bodies, caused by the Corpuscles of any other Body, it is easie to be understood, That if that motion be greater in one Hand, than in the other; if a Body be applied to the two Hands, which has in its minute Particles a greater motion, than in those of one of the Hands, and a less, than in those of the other, it will increase the motion of the one

Hand, and lessen it in the other, and so cause the different Sensations of Heat and Cold, that depend thereon. (Locke, 1690/1975, p. 139)

As one might suspect by this time, John Locke knew a great deal about taste aversion learning. He clearly distinguished between the effect of food on the taste receptors and its effect on the internal visceral receptors:

Ideas of *Sickness and Pain are not in the* Manna, but Effects of its Operations on us, and are no where when we feel them not: . . . *Sweetness and Whiteness are not really in Manna*; which are but the effects of the operations of *Manna*, by the motion, size, and figure of its Particles on the Eyes and Palate; as the Pain and Sickness caused by *Manna*, are confessedly nothing, but the effects of its operations on the Stomach and Guts, by the size, motion, and figure of its insensible parts. (p. 138)

Furthermore, Locke knew that a single taste–illness trial could produce a lasting aversion by operating on the emetic mechanism. And he knew that a conditioned aversion could endure even though the memory of the actual association was beyond recall:

A grown Person surfeiting with Honey, no sooner hears the Name of it, but his Phancy immediately carries Sickness and Qualms to his Stomach, and he cannot bear the very *Idea* of it; other *Ideas* of Dislike and Sickness, and Vomiting presently accompany it, and he is disturb'd, but he knows from whence to date this Weakness, and can tell how he got this Indisposition: Had this happen'd to him, by an over dose of Honey, when a Child, all the same Effects would have followed, but the Cause would have been mistaken, and the Antipathy counted Natural. (p. 397)

I am thankful that he left something for us to find; he apparently was not aware that food odors are potentiated by the taste of food. Finally, he warned those who simplistically proclaim "Mankind" as the superior being and apply a general scale of intelligence to all "creatures" in this "Fabrick," by which he meant "this vast and stupendous Universe":

He that will not set himself proudly at the top of all things; but will consider the Immensity of this Fabrick, and the great variety, that is to be found in this little and inconsiderable part of it, which he has to do with, may be apt to think, that in other Mansions of it there may be other, and different intelligent Beings, of whose Faculties, he has as little Knowledge or Apprehension, as a Worm shut up in one drawer of a Cabinet, hath of the Senses or Understanding of a Man. (p. 120)

All this psychobiological structure was wiped away by the alien wind, and what remains of John Locke in our time is the specious and empty tabula rasa. As Sancho Panza might have said to Don Quixote, "Stop, they are but windmills. They flail their arms and crank out gruel in obedience to an ill wind, *el espirtu del tiempo*. In truth's sake, you must level your lance at the wind. Charge the wind, Don Quixote, charge the wind!"

REFERENCE NOTES

1. Moore, B. R. Personal communication, March 1980.
2. Revusky, S. H. Personal communication, March 1980.

REFERENCES

Aitken, P. P. Aversive stimulation and rats' preference for familiarity. *Psychonomic Science*, 1972, *28*, 281–282.

Aitken, P. P., & Sheldon, M. H. Electric shock and rats' preference for the familiar areas of a maze. *British Journal of Psychology*, 1970, *61*, 95–97.

Bentham, J. *An introduction to the principles of morals and legislation*. Oxford, England: Clarendon Press, 1907. (Originally published, 1823.)

Bernstein, I. L. Learned taste aversions in children receiving chemotherapy. *Science*, 1978, *200*, 1302–1303.

Bernstein, I. L., & Sigmundi, R. A. Tumor anorexia: A learned food aversion? *Science*, 1980, *209*, 416–418.

Brett, L. P., Hankins, W. G., & Garcia, J. Prey-lithium aversions. III: Buteo hawks. *Behavioral Biology*, 1976, *17*, 87–98.

Brown, P. L., & Jenkins, H. M. Autoshaping of the pigeon's key-peck. *Journal of the Experimental Analysis of Behavior*, 1968, *2*, 1–8.

Cervantes Saavedra, M. de. *The history of Don Quixote* (J. W. Clarke, Ed.; G. Dore, Illus.). London: Cassel, Petter & Galpin, 1867? (Originally published, 1605.)

Clarke, J. C., Westbrook, R. F., & Irwin, J. Potentiation instead of overshadowing in the pigeon. *Behavioral and Neural Biology*, 1979, *25*, 18–29.

Dawkins, R. *The selfish gene*. New York: Oxford University Press, 1976.

Domjan, M. Attenuation and enhancement of neophobia for edible substances. In L. M. Barker, M. R. Best, & M. Domjan (Eds.), *Learning mechanisms in food selection*. Waco, Tex.: Baylor University Press, 1977.

Garcia, J., Clarke, J., & Hankins, W. G. Natural responses to scheduled rewards. In P. P. G. Bateson & P. Klopfer (Eds.), *Perspectives in ethology*. New York: Plenum Press, 1973.

Garcia, J., Ervin, F. R., & Koelling, R. A. Learning with prolonged delay of reinforcement. *Psychonomic Science*, 1966, *5*, 121–122.

Garcia, J., Ervin, F. R., Yorke, C. H., & Koelling, R. A. Conditioning with delayed vitamin injections. *Science*, 1967, *155*, 716–718.

Garcia, J., Hankins, W. G., & Rusiniak, K. W. Behavioral regulation of the milieu interne in man and rat. *Science*, 1974, *185*, 824–831.

Garcia, J., Hankins, W. G., & Rusiniak, K. W. Flavor aversion studies (with reply by M. E. Bitterman). *Science*, 1976, *192*, 265–267.

Garcia, J., Kimeldorf, D. J., & Koelling, R. A. A conditioned aversion towards saccharin resulting from exposure to gamma radiation. *Science*, 1955, *122*, 157–159.

Garcia, J., & Koelling, R. A. Relation of cue to consequence in avoidance learning. *Psychonomic Science*, 1966, *4*, 123–124.

Garcia, J., & Koelling, R. A. A comparison of aversions induced by X-rays, toxins and drugs in the rat. *Radiation Research Supplement*, 1967, *7*, 439–450.

Garcia, J., McGowan, B., Ervin, F. R., & Koelling, R. A. Cues: Their relative effectiveness as a function of the reinforcer. *Science*, 1968, *160*, 794–795.

Garcia, J., McGowan, B., & Green, K. Biological constraints on conditioning II. In A. Black & W. Prokasy (Eds.), *Classical* *conditioning: II. Current research and theory.* New York: Appleton-Century-Crofts, 1972.

Garcia, J., Palmerino, C. C., Rusiniak, K. W., & Kiefer, S. W. Taste aversions and the nurture of instinct. In J. L. McGaugh & R. F. Thompson (Eds.), *The neurobiology of learning and instinct.* New York: Plenum Press, in press.

Garcia, J., & Rusiniak, K. W. What the nose learns from the mouth. In D. Müller-Schwarze & R. M. Silverstein (Eds.), *Chemical signals.* New York: Plenum Press, 1980.

Gustavson, C. R., Kelly, D. J., Sweeney, M., & Garcia, J. Prey-lithium aversions. I: Coyotes and wolves. *Behavioral Biology,* 1976, *17,* 61–72.

Guthrie, E. R., & Horton, G. P. *Cats in a puzzle box.* New York: Rinehart, 1946.

Locke, J. *An essay concerning human understanding* (P. H. Nidditch, Ed.). Oxford, England: Clarendon Press, 1975. (Originally published, 1690.)

Malmo, R. B. Activation: A neurophysiological dimension. *Psychological Review,* 1959, *66,* 367–386.

McDougall, W. *Introduction to social psychology.* London: Methuen, 1908.

Miller, V., & Domjan, M. Selective sensitization induced by lithium malaise and foot-shock in rats. *Behavioral* and *Neural Biology,* in press.

Mitchell, D., Scott, D. W., & Mitchell, L. K. Attenuated and enhanced neophobia in the taste-aversion "delay of reinforcement" effect. *Animal Learning & Behavior,* 1977, *5,* 99–102.

Moore, B. R., & Stuttard, S. Dr. Guthrie and Felis domesticus Or: Tripping over the cat. *Science,* 1979, *205,* 1031–1033.

Notes and comments. *Animal Learning & Behavior,* 1978, *6,* 115–124; 1979, *7,* 562–563.

Palmerino, C. C., Rusiniak, K. W., & Garcia, J. Flavor-illness aversions: The peculiar roles of odor and taste in memory for poison. *Science,* 1980, *208,* 753–755.

Revusky, S., & Bedarf, E. Association of illness with prior ingestion of novel foods. *Science,* 1967, *155,* 219–220.

Rozin, P., & Kalat, J. Specific hungers and poison avoidance as adaptive specializations of learning. *Psychological Review,* 1971, *78,* 459–486.

Rusiniak, K. W., Hankins, W. G., Garcia, J., & Brett, L. P. Flavor-illness aversions: Potentiation of odor by taste in rats. *Behavioral and Neural Biology,* 1979, *25,* 1–17.

Skinner, B. F. *The behavior of organisms: An experimental analysis.* New York: Appleton-Century-Crofts, 1938.

Skinner, B. F. Are learning theories really necessary? *Psychological Review,* 1950, *57,* 193–216.

Smith, A. *An inquiry into the nature and causes of the wealth of nations* (E. Cannan, Ed.). New York: Random House, 1937. (Originally published, 1776).

Walcott, C., Gould, J. A., & Kirschvink, J. A. Pigeons have magnets. *Science,* 1979, *205,* 1027–1029.

Wilson, E. O. *Sociobiology: The new synthesis.* Cambridge, Mass.: Harvard University Press, 1975.

Young, P. Hedonic organization and regulation of behavior. *Psychological Review,* 1966, *73,* 58–86.