Due (at least partly) to a new generation of color-plated, illustrated, work-booked, web-sited textbooks, psychology seems to be advancing in the interdepartmental competition for students in introductory science courses, displacing “rocks for jocks,” and other shameless appeals from potentially less winsome sciences. Areas such as cognitive psychology are blessed with both new and old demonstrations of surprising optical illusions, blind spots, and limitations in visual attention. Research in development, learning, stimulus processing, social influence, and mental illness resonate with the personal experiences and interests of many students. For the more science minded, recent work in neurophysiology clarifies the role of executive function in everyday life and the disruptive effects of addictive drugs and stress.

However, in my role as a curmudgeonly experimentalist, I see a potential dark cloud within this silver lining. I am concerned that psychology is increasingly following other sciences in teaching introductory courses filled with received facts rather than emphasizing the role of scientific analysis and experimentation in discovering, testing, and improving such facts. My impression is that our laboratory courses focus more often on demonstrations of phenomena, spending their time teaching students how to do graphs and (Continued on Page 2)
The Behavioral Neuroscientist and Comparative Psychologist is the official newsletter of APA Division 6 — Behavioral Neuroscience and Comparative Psychology — and is published 3 times a year. Mailing addresses used are those appearing on the official APA roster and a separate Division roster. Corrections and changes of address should be sent directly to the APA Directory Office, 1400 North Uhle St., Arlington, VA 22201, and to the newsletter editor (see below).

As the official newsletter of Division 6, BNCP publishes official business, committee reports, news items, job announcements, information on technical issues, topics of current interest, and information about the professional activities of Division 6 members. News items and articles should be submitted to the Editor at the address below (preferably by email). Paid advertisements are not officially endorsed by Division 6. BNCP can better serve the needs of the members. The preferred method of submission is by email. Send correspondence and submissions to Eric P. Wiertelak via e-mail at wiertelak@macalester.edu. Postal mail should be sent to Eric P. Wiertelak, Department of Psychology, Macalester College, Saint Paul, MN 55105. Other contact information: Phone: (651) 696-6111; FAX: (651) 696-6348.

(Continued from Page 1) statistical analyses, and how to write experiments following the format specified by the perennially updating publication manual of the American Psychological Association. My concern is that we are beginning to lose the critical connection between facts and the experiments that generate them. The content and process of science are becoming separate fields.

To clarify my concerns, I will recount a brief history of my own student adventures in Biology, Chemistry, Physics, and Psychology. Each experience began with a one or two semester introductory course, and continued in an accompanying laboratory. The introductory course summarized the content areas, terms, history, and facts students were expected to learn for the tests. The laboratory course most often focused on how to use apparatus, collect and display data, and produce phenomena.

Biology Lab (Kinds and Functions of Bodies and Parts)

My college introductory biology laboratory provided students with an impressive variety of (mostly dead) organisms and parts. Animals ranged from paramecia to fetal guinea pigs and included tissues from bones to epithelial cells and preserved kidneys. Activities included looking through microscopes, drawing, dissecting, and memorizing. The laboratory was well organized with a few student assistants roaming the room trying to field questions and make helpful suggestions. The basic forms of evaluation were attendance, drawings, and a lab practicum in which you had to identify the point of interest in each of some 20 set-ups. It was a good, if somewhat in-your-face, introduction to tissues and dissection (probably designed primarily to train would-be doctors for medical school). The major drawbacks were the absence of an evolutionary emphasis and a bias toward idealization. I recall painstakingly drawing a paramecium (all drawing is painstaking for me, requiring constant comparing with the object I am drawing). I was really pleased that I persevered to render the paramecium with considerable accuracy, but I was subsequently shocked to get a C for my efforts. (Continued on Page 3)
(Continued from Page 2) Emboldened by the work I had invested, I approached the grader to find out what I had done wrong. She said my drawing didn’t look like a paramecium. I assured her that it looked like my paramecium. She pointed to a textbook drawing of a paramecium, and said they were supposed to look like that. I couldn’t argue with the textbook because I hadn’t seen the artist’s paramecium. Maybe it did look like that. Mine didn’t. I had read a little history and philosophy of science, and had been impressed by the tenet of always reporting exactly what you observed. It seemed to follow from my reading that biology was not a science, so I moved on.

Chemistry Lab (Cooking With a Recipe)

The Chemistry laboratory was busier. Everyone had a bunsen burner, spatula, test tubes, lists of instructions, and questions to answer. It was quickly clear that the procedures required care and precision, reminding me of learning to cook out of a book. In both cases I lacked basic information about what steps were critical and how sensitive the results were to deviations. The exercises were well organized and the desired end point was well characterized (e.g., production of a white precipitate after heating to a certain temperature). In short, chemistry lab taught me that to do science you needed a good cookbook that specified all the ingredients, their order and method of combination, and a clear description of the end point. I was not clear how all this related to the process of doing science.

Physics Lab (Cooking Without a Recipe)

The undergraduate physics lab actually played a key role in my deciding to become a scientist (though it failed at turning me into a physicist—I had a problem intuitively thinking in equations). Some aspects of the physics lab were like chemistry lab. You followed a list of instructions to set up the conditions to get a particular result (like a display of the absorption spectrum for an element). What made this laboratory exceptional, though, was the first assignment. Each student was presented with an inventory of objects: a length of string; four ping pong balls that could be tied to the string by a hook, each ball stuffed with a different amount of weight; a sturdy plastic protractor that could be attached to the wall (along with the string); and a stop watch. Our instructions were to establish the formula for the period of a pendulum, but without looking it up or consulting experts, friends, or previous lab takers.

I quickly made a pendulum out of the string and one of the bobs, suspended the string near the protractor, and plowed headlong into experiments and decisions: how to measure the period, where and how to release the bob, how long the string should be, how many swings to measure, and how to make up a data sheet and use the stop watch. I recorded some data, and then tried other string lengths, bob weights, drop heights, and number of swings. I realized at some point I wasn’t exactly answering the questions, so I started trying to put my data into the form of an equation that produced a time in seconds on one side, and collected the other variables on the other side.

In the midst of this surprisingly involving and difficult process it began to dawn on me that what I was doing related pretty directly to what it was like for people to do science. You have a question or at least an interest in some result, you manipulate possible contributing variables, you figure out a way to take, store, and recover the resultant data (otherwise you have to do the experiment again as memory fades), you try to summarize the data in a form that clarifies any regularities present, and you work to integrate the regularities with other information you have. I didn’t always keep adequate data notes, and I failed to grasp that in an equation relating independent and dependent variables must balance the units on both sides—an incredibly useful and obvious notion. In short, the experience was a knockout learning experience, and I was hooked.

Psychology Lab (Causal Models and Owning Your Experiment)

My experience in psychology added to the excitement of the physics lab in two ways. The first came during the teacher’s introduction of the idea of testing hypotheses that occurred the first day of class. A few minutes into the first lecture, the instructor (Graham Bell of Pomona College) picked up a “Chatty Kathy” doll out of a box on a table beside the lectern, and began to pull the string at the back of her neck with one hand while holding the doll in the other. With each pull (Continued on Page 4)
(Continued from Page 3) the doll uttered a short phrase, “Please give me a hug,” “I like you,” “Let’s be friends,” and so on. He told us to listen to what the doll said each time as the pulled string was released, and, based on these data, generate an hypothesis about the mechanism inside the doll that was producing the words. He pulled the string perhaps 20 more times, varying its angle and speed as requested by the audience. Then he told us to hand in our hypotheses before the next class, and he would open the doll on the spot and find out who had the most accurate hypothesis.

This was before the era of voice chips and small batteries, so it seemed clear to me that pulling the string must store mechanical energy, and probably also selected a particular utterance semi-randomly. I envisioned a small record in the doll’s head, the pulling of the string compressed a spring and dropped a needle on the record, and the spring, through a set of gears, turned the record. It was close enough. In the next class Kathy lost her head to a hack saw, and we examined the device. Notice that everyone was asked to develop their own hypothesis about the mechanism (and there were some doozies), and we checked these predictions by opening the doll, and that we still had to translate what we found into the behavior of the doll. The generalization to explaining behavior by neurophysiological mechanisms was clear.

A lab section run by a graduate also met each week. We were introduced to simple experiments, many of them with paper and pencil, and then required to make up and run our own experiment. This was, of course, before the running all studies became a disease requiring the protection of the subjects. I decided to test an interference paradigm in paired associate learning in which the control group worked a puzzle during the break between training and retention, while the experimental group learned a second set of paired associates. Then both groups were presented with the first word in each pair to test the subject retention of the associate. To my disappointment there was no difference in retention, but as I was writing up the experiment a few days later, it occurred to me there might not have been enough time to show differential forgetting. So I ran around giving everyone I could find a second retention test—and pleasingly, the data showed effects in the predicted direction. It wouldn’t pass a journal editor, but I had used my lack of results and a simple model of how acquisition might work to make a testable prediction that received some support. That was exciting.

Relevance to Teaching Science.

So what did my psychology and physics labs have that was missing from chemistry and biology? They both, to a degree, combined facts with the process of science in a way that allowed students to understand how facts emerge, and how they could be added to. Are we teaching psychology labs like that today? The answer is not as clearly “yes” as I would have hoped. To our credit, we do talk about hypotheses, and we teach students how to produce graphs and do modest statistics, but we seem most concerned with writing reports, and the lab often seems separated from the course.

My university, like most, has tried several combinations of courses and labs. There is no right way, but my personal favorite was a combination lab and lecture course for honors majors that met 4-5 days a week for 5 credits and earned the student 8-9 credits toward the major. It fell out of favor with the undergraduate chair, so it was taken out of circulation while I was on sabbatical. It cost the department two course credits to the instructor, and even at that level of “pay”, it was a lot of work. What it did, though, was provide opportunities for bringing together facts and process in at least three ways:

1. Because the students and professor saw each other 4-5 days a week and in the context of both a lecture transmitting information and a laboratory focusing on testing and producing conclusions, there was a common foundation for psychology, one that also helped bridge differences between natural and social science approaches and topics.

2. There were opportunities for exploring differences between assumptions and tests. I recall challenging the class (after a day of their being particularly passive note takers) to assume that all the authorities were dead, the text books gone, the planes and satellites grounded, and to prove to me that the earth was round. I even offered an A for the week to the first person who was successful in providing reasonable proof. I held out for 10-15 minutes as people worked their way toward proofs. On another day we were learning statistics and somehow started a discussion of the accuracy of astrological signs

(Continued on Page 5)
in describing a person. I had everyone hand in a list of ten of their most salient traits, along with their astrological sign. I made lists of 10 characteristics for each sign from a book on “sun signs” and (after consulting a statistician), calculated multivariate correlations between personal lists and astrologers lists for each sign. With the exceptions of three people, there was no relation. Interestingly, it turned out that these three people were big fans of sun signs, so possibly they knew their characteristics.

Finally, such a course can kick-start the involvement of smart students with psychology. By their sophomore year, many of these students were involved in research, and a surprising number of them went on to receive graduate degrees in psychology and other fields in the academy and in public and private practice. I can’t prove this wouldn’t have happened anyway, but in individual student’s reconstruction of their lives, this introductory class loomed as important.

Based on my experience, I am inclined to think psychology as science can be most effectively taught by paying attention to the process of science, especially experimentation. Science is as much a habit of mind as a set of facts and phenomena. If you have any thoughts on this topic, I’d be happy to hear them.

REPORT ON APA COUNCIL, FEBRUARY, 2008

Bruce Overmier
Council Representative for Division 6

The February Council meeting has some set agenda components such as passing the year budget, hearing the new President plans, hearing from candidates for the next election, the annual diversity report, etc. And, this year there were items of special interest to comment on.

Of primary interest likely is that the proposal for a new division of APA on “Qualitative Inquiry” failed to be approved. It was the position of Division 6 to oppose the proposed new division because it was judged from the positions of the proposed division’s leadership that it was inimical to behavioral neuroscience and comparative psychology (indeed one could well argue it was inimical to all of psychology). Nonetheless, it was almost approved, and I would guess that the proposal for this division will be back before Council again soon. If it is to be defeated a second time, our division will need to proactively enlist other higher numbered divisions against it. In contrast, a Candidate new division of Trauma Psychology (a proposal that Division 6 had supported) was approved as a permanent new division of APA.

Council also voted to add to the list of Council priorities “Promote and support the advancement of psychology as a scientific discipline.” This will facilitate proposals that scientists bring to the floor of Council.

As you recall, the President taking office this year is Alan Kazdin of Yale University. President elect is James Bray of Texas. Alan Kazdin announced his Presidential themes: Psychological Science Contributions to the Grand Challenges facing Society, Mitigating Interpersonal Violence, and PTSD and Trauma in Children and Adolescence. Each involves diverse interests, constituents, and members. An organizational concern of his is increasing membership and recruitment of new members of APA—especially among the science divisions.

CEO Norman Anderson outlined the coming year. It will include:
(1) The complete rebuilding of the APA website into a useful tool. The financing for
(Continued from Page 5) this expensive operation will be via the APA’s revolving credit line as the cheapest method.

(2) Bringing the growth of the operating budget into line with the growth of revenues. Heretofore, revenues have grown sufficiently to allow the wanted growth in operations, but now revenues are flattening and so must operations. Indeed, the plan is for future budgets to include an at least 1% surplus to guard against unexpected challenges on the revenue side.

(3) Strategic planning for the organization will be accomplished this year and reported on Feb 2009. It will provide a basis for choices among the many annually proposed new projects to be undertaken. Information collection for building the strategic plan will draw on members. IF YOU ARE ASKED FOR INPUT, DO RESPOND! FAILURE TO RESPOND WILL DISADVANTAGE SCIENCE WITHIN APA.

The APA budget for 2009 was approved at a level of $113,000,000 with a projected tiny surplus of just $333,000. Much of this budget involves the publications program. Science and lobbying for science gets a few million—likely more than we pay in dues.

The Council will again ask members to approve adding 4 new seats to Council to represent the 4 psychology associations composed of minority psychologists. It is doing so because it is believed that this is a critical step in increasing minority persons participation in our field and in joining APA.

Not on the Council floor but in parallel meetings, many of the representatives from the science focused divisions met to discuss how those divisions working with the Board of Scientific Affairs can build an agenda for science that can be taken to Council for actions that would enhance psychological science.

Also not on the Council floor, but of interest to all, were discussions of the work of an APA Task force on Council representation. The origin of this TF was in concerns by many states that they were not accorded an adequate number of seats/votes on Council. This of course arises because of the decision that all geographic units get a seat—even if they have as few as a dozen members (e.g., Guam). Those seats have to come from somewhere, and the current system takes them from a pool of seats for states, provinces, and territories. Any new system that proposes to add seats to states without increasing size of Council—as some are proposing—will weaken relatively the divisions. If you have views on this, you should write to the President, to the lead staff person, Judy Strassberger, or to TF Chair, Melba Vasquez.

Respectfully submitted,

Bruce Overmier
Council representative for Division 6

BNCP back issues are available online at http://www.apa.org/divisions/div6/newsletter.html
Conditioned taste aversion (CTA) has attracted the attention of scientists from many disciplines and perspectives, including animal behavior, psychopharmacology, neurobiology, behavioral neuroscience, and learning theory. My interest in CTA was motivated by my interest in learning.

The Pre-CTA Years

I was introduced to learning theory (or should I say “non-theory”?) in the mid 1960’s by Skinnerians at the Behavior Science Institute, directed by Neil D. Kent. I first attended the Behavioral Science Institute in 1963 at Grinnell College and then joined the staff when the Institute was moved to Western Michigan University. Summers at the Behavior Science Institute were marvelous for both the intellectual stimulation and the camaraderie. Visiting instructors included Nathan Azrin, Ogden Lindzey, Murray Sidman, Joe Brady, Fred Keller, Ron Hutchinson, Jack Michael, and Bill Hopkins. The science was exciting, and the enthusiasm of the faculty was infectious. But, they said nothing about conditioned taste aversions or much about Pavlovian conditioning.

For Skinnerians in the mid 1960’s, Pavlovian conditioning was “the other” type of learning that modified only reflexive and smooth muscle responses. Much of their excitement in those days stemmed from developing new applications of operant conditioning—applications that would significantly improve the lives of patients in psychiatric hospitals and mental retardation facilities, inmates in correctional facilities, and students at all levels from pre-school to college.

Skinnerians in the 1960’s were out to change the world. It was an exciting time and a lofty goal, and I was eager to sign on. I was particularly captivated by the emphasis on data collection and the insistence that even in complex applied settings, data has to drive decisions. Sidman’s Tactics of Scientific Research became my bible. I read the book several times and was glad to see a recent announcement that it is still in print. When I got the opportunity to design my first research project, I picked a topic that was central to the development of Skinnerian principles, namely his “superstition” experiment (Skinner, 1948). Skinner claimed that contiguity between a response and a reinforcer was all you needed to produce increases the rate of that behavior. A causal relation between response and reinforcer was superfluous.

I read Herrnstein’s treatment of Skinnerian superstitious behavior in the seminal Handbook of Operant Behavior, edited by Honig (1966) and was struck by the fact that all of the relevant data were obtained in studies with positive (food) reinforcers. To correct the imbalance, I used the Herrnstein approach to conduct a series of studies of superstitious behavior based on aversive stimulation—what I referred to as superstitious escape behavior (Domjan, 1969; Domjan & Rowell, 1969a, b). The studies showed, among other things, that rats could discriminate procedures in which their responses turned off shock versus procedures in which shock was turned off independent of their behavior. This type of contingency detection was also demonstrated in the learned helplessness experiments that were being conducted at the University of Pennsylvania at (Continued on Page 8)
the same time (e.g., Maier, Seligman, & Solomon, 1969). I wish I had been aware of that line of work, but I missed it with my focus on the Skinnerian literature.

In addition to the laboratory work, I also got involved in applied projects, during what were the formative years for the now vibrant field of applied behavioral analysis. Under the supervision of Louise Kent, I worked for about 8 months as a Program Development officer at the Fort Custer State Home for the mentally retarded. Although I enjoyed my activities there, laboratory work was more compelling, and on the advice of Ron Hutchinson, I entered the Ph.D. program in Biopsychology at McMaster University in the fall of 1969. Hutchinson recommended that program because it did not involve many required courses and permitted students to concentrate on their research. I had great respect for Hutchinson as a scientist. If McMaster was good enough for Hutchinson, it was good enough for me.

McMaster and My First Encounter with CTA

McMaster had a distinguished faculty in learning. Leo Kamin did all of his seminal work on conditioned suppression and the blocking effect at McMaster (Kamin 1965, 1969) and moved to Princeton just before I arrived. Others on the faculty included Herb Jenkins and Abe Black. The vacancy left by Kamin’s departure was filled by Shep Siegel, who got his Ph.D. at Yale with Allan Wagner a few years earlier. Another new faculty member was Jeff Galef, who just got his Ph.D. at the University of Pennsylvania with Paul Rozin. I have continued to cross paths with Siegel and Galef ever since, most recently in Austin, where each of them gave invited presentations at the 2007 meeting of the Pavlovian Society, during my term as President of that organization.

McMaster was an exciting place, at the cross-road of many of the key developments in learning theory in the 1970's. Herb Jenkins had just published his seminal paper on autoshaping, and the Rescorla-Wagner model was first presented there at a conference on Pavlovian conditioning (Black & Prokasy, 1969). Several years after that, Shep Siegel started work on what was to become his conditioning model of drug tolerance. My work in his lab included latent inhibition, backward and conditioned inhibition, and rabbit jaw movement conditioning.

Shep was not working on problems in taste aversion learning when I joined the lab, but there was a lot of discussion of Garcia’s CTA research in classes, seminars, and hallways. The blocking and relative validity effects that inspired the Rescorla-Wagner model served as one line of attack on traditional contiguity theories of associative learning. Garcia’s long-delay learning and cue-consequence specificity served as a major second line of attack. However, those pursuing the Rescorla-Wagner reformulation of learning theory never joined forces with investigators interested in CTA. They were just as skeptical of CTA as was everyone else in those days.

Jeff Galef began his landmark studies on the social transmission of food preferences soon after his arrival at McMaster and had a major role in stimulating discussions of the Garcia effects. His dissertation advisor, Paul Rozin, completed a beautiful series of experiments at Harvard that led to a re-interpretation of the Curt Richter cafeteria studies, which showed that rats fed a nutritionally deficient diet tend to select foods that help alleviate their dietary deficiency. Rozin demonstrated that this medicinal food choice is not driven by a specific hunger for the missing nutrients but by an aversion acquired to the nutritionally deficient food. Rozin’s data were clear but the acquired aversion interpretation needed firmer legs in learning theory. After all, illness caused by nutritional deficiency is slow to develop, and if an aversion is learned to the deficient food, why are aversions not learned to other cues that may also be present as the deficiency develops? Garcia’s long-delay learning and selective association effects provided the missing legs for Rozin’s story.

Rozin needed the Garcia phenomena to complete his reinterpretation of the specific hunger results and subsequently also used long-delay learning and selective associations to formulate a more comprehensive approach to adaptive specializations in learning (Rozin & Kalat, 1971). Garcia also (Continued on Page 9)
greatly benefited from Rozin’s attentions. By highlighting the Garcia phenomena at the University of Pennsylvania, Rozin brought the phenomena to the attention of only future investigators like Jeff Galef, as well as colleagues like Marty Seligman, who quickly jumped on the “constraints on learning” bandwagon with his concept of preparedness (Seligman, 1970; Seligman & Hager, 1972). Seligman has been a masterful popularizer throughout his career and helped make Garcia a household name.

Galef carried Rozin’s fascination with CTA to McMaster, where it faced close critical scrutiny by the learning faculty. Attention focused on two main issues. First, were long-delay learning and selective associations artifacts of poor experimental design? Second, if the effects were genuine, did they require abandonment of general process learning theory or just modifications of it? As I became steeped in these arguments, it became clear to me that what we needed was more data—not more argument.

The Role of Ingestion in CTA

The first question I tried to answer about CTA was what made it unique—what was different from CTA that made it different from other forms of Pavlovian conditioning? The most obvious difference to me was that in CTA the subject was in control of contact with the conditioned stimulus (CS). Rats had to drink a flavored solution to become exposed to it prior to receiving the illness-inducing agent (the source of the unconditioned stimulus or US). In all other Pavlovian paradigms, both the CS and the US are presented independently of the subject’s behavior. In fact, this response-independence of the CS and US is often emphasized as a distinguishing, if not defining, feature that sets Pavlovian conditioning apart from operant and instrumental conditioning. Perhaps violation of the response-independence rule produced some of the unique features of CTA.

If ingesting the CS was responsible for some of the unique features of CTA, then these features might be lost if the CS was presented without ingestion. But, how could we present taste without ingestion? I discussed these issues with another graduate student, Nancy Wilson, who had followed Jeff Galef to McMaster from the University of Pennsylvania, where she got her bachelor’s degree. Nancy was in Ed Stricker’s lab, and Abe Black’s lab was down the hall—not that these distinctions made much difference. The animal laboratories at McMaster were housed in old Army barracks at the time. The place was crowded, and students from various laboratories invariably ran into each other during the course of their daily activities.

Among other things, Abe Black’s lab was concerned with interactions between autonomic and instrumental behavior. To study this question, one of their techniques involved injecting rats with curare, which paralyzed the skeletal musculature without affecting sensory processing. The preparation was a bit tricky because curare also paralyzed the musculature required for breathing and the rats had to be maintained on a ventilator. The ventilator pump provided air through a nose cone that left the mouth of the rats unobstructed. Therefore, one could rinse the tongue with various taste solutions while the animal was prevented from licking and swallowing by the curare. Nancy Wilson and I adopted the curare procedure to study CTA in the absence of ingestion and found that rats conditioned while paralyzed with curare learned weaker aversions to saccharin than rats injected with lithium after ingesting saccharin in the normal fashion (Domjan & Wilson, 1972a, Experiment 1).

Our curare experiment was promising in isolating the role of ingestion in taste aversion learning, but one could argue that various unusual aspects of the curare preparation caused the reduced CTA. Therefore, we sought to develop a less invasive procedure for presenting taste without ingestion.

I was trying to condition jaw movement responses in rabbits in Shep Siegel’s lab at the time. That line of work did not go very well and Shep and I never published any jaw movement studies, but the procedures we used to elicit jaw movement in the rabbits suggested an alternative to curare for the taste aversion experiments. The rabbits were fitted with an oral cannula that allowed us to squirt a small [Continued on Page 10]
amount of water into the mouth using an infusion pump. Nancy Wilson and I adopted the cannula preparation and tried infusing taste solutions into the oral cavity of rats. We discovered that if the fluid was infused into the oral cavity at a slow rate (3 ml/min) while the rats were thirsty, the infusion would elicit licking and swallowing responses. Less licking and swallowing occurred if the infusion rate was increased, and no licking and swallowing occurred if the rats were not water deprived while getting the oral infusion at a high rate (46 ml/min). Under those conditions, the rat would simply open its mouth and let the fluid rush over the tongue and out of the mouth.

We repeated the taste aversion experiment using the oral infusion technique and again found that in the absence of ingestion, CTA was significantly weaker than if the rats received the taste solution under conditions that permitted ingestion (Domjan & Wilson, 1972a, Experiment 2). This was an all-or-none effect. If any of the infused fluid was ingested, the attenuation of CTA did not occur. Furthermore, it was an effect related to long-delay learning, since the lithium injection was administered 25 min after the CS exposure.

Other Applications of the Oral Infusion Technique

Nancy Wilson and I subsequently used the oral infusion technique in a replication of the Garcia-Koelling (1966) cue consequence specificity experiment. Garcia and Koelling were also concerned with the fact that tastes are usually encountered contingent on licking whereas audiovisual cues in most laboratory studies are presented independently of behavior. Their solution to this contrast was what has come to be known as the “bright-noisy water” technique. To make the audiovisual presentations comparable to lick-contingent taste stimulation, they presented audiovisual cues contingent on rats licking a drinking tube containing water. Nancy Wilson and I repeated the Garcia-Koelling experiment with a saccharin solution and a buzzer as the conditioned stimuli, but this time both types of CSs were presented in a response independent fashion. To meet some of the criticisms that were leveled against the Garcia-Koelling study, we repeated the experiment twice, once with the taste and buzzer presented as a compound before lithium poisoning or shock and the second time with independent groups that received only one of the CSs (Domjan & Wilson, 1972b).

After these initial experiments, I used the oral infusion technique extensively in a wide range of CTA studies. The technique was especially useful, if not essential, in studying manipulations that ordinarily would have induced different levels of drinking during a conditioning trial. While still at McMaster, I used the technique to study water deprivation and stimulus exposure parameters as in the CS preexposure or latent inhibition effect (Domjan, 1972). Subsequently, I used the technique in studies of the attenuation of flavor neophobia (Domjan, 1976), the blocking effect in CTA (Gillan & Domjan, 1977), the proximal preexposure effect (Domjan & Gemberling, 1980), and studies of CTA in pre-weanling rats (Gemberling & Domjan, 1982; Gemberling, Domjan, & Amsel, 1980; Gregg, Kittrell, Domjan, & Amsel, 1978).

Ingestion as a “Gating” Mechanism

I also continued to pursue my interest in the role of ingestion in CTA. After the initial taste-aversion experiments, I examined the role of ingestion in odor aversion learning. Odor aversion learning is interesting because odors can emanate from both food and nonfood sources. Garcia and his colleagues have argued that taste serves to “gate” odors into the ingestive system and makes possible illness-induced conditioning of odors (Rusiniak, Hankins, Garcia, & Brett, 1979). I found that drinking water during an odor conditioning trial facilitates the expression of an odor aversion without a distinctive taste (Domjan, 1973). This effect did not reflect an acquired aversion to water but seemed to reflect the conditioning of a configural cue that involved ingestion-related sensations. Thus, I have come to regard ingestion-related sensations as the “gating” mechanism to makes odors and other cues relevant to feeding.
Another category of stimuli that is “ambiguous” in the same sense as olfactory cues is tactile stimuli. Animal encounter tactile cues during the course of locomotor behavior. However, species that hold their food while ingesting it also encounter tactile cues related to ingestion. In a series of experiments with rats and monkeys, I demonstrated illness-induced aversions to tactile cues with delays as long as 30 min (Domjan & Hanlon, 1982; Domjan, Miller, & Gemberling, 1982). The experiments involved a discrimination between two foods that were identical in taste but differed in tactile cues. The role of visual cues was eliminated by conducted the experiments in the dark.

Long-delay odor and tactile aversion learning is remarkable because subjects are likely to encounter other odors and tactile cues during the delay interval, and these intervening stimuli could present concurrent interference for the target aversion. I think ingestion related cues play a major role in limiting such interference. Since the odor and tactile cues encountered during the delay interval are not related to ingestion, they are not “gated” into the ingestive system and are not effective CSs for subsequent food-induced illness. As Domjan and Hanlon (1982) noted, “perhaps the ingestive context serves to direct tactile information to a special ingestion-related memory mechanism in which information is segregated from other tactile stimulation and stored long enough for association with delayed toxicosis (p. 300). A similar process may be involved in learning aversions to temperature cues (Nachman, 1970).

Sexual Conditioning and Adaptive Specializations in Learning

After spending about 15 years studying taste aversion conditioning (working on various problems that I will not mention here), I changed the focus of my research to how learning occurs in the sexual behavior system. Although this involved a different species (to the coturnix quail) and behavior systems, I remained very much interested in adaptive specializations in learning and the fundamental challenges that CTA presented to learning theory. Sexual conditioning was a fairly unexplored area at the time. My hope was that at the broad theoretical level my work in sexual conditioning would help us better understand specialized learning effects. However, when I started the work with quail I had no way to predict that any of the phenomena that I might encounter would hark back to some of my CTA experiences. As it has turned out, my research has come full circle. Some of the most interesting things that my collaborators and I have found in sexual conditioning have a striking resemblance to some of the special features of CTA.

My early studies of sexual conditioning followed the standard format of pairing an arbitrary cue with copulatory reinforcement that served as the US (Domjan et al., 1986). The work went well, and I had no trouble attracting students and grant funds. One of the people who was attracted to the work was Falih Köksal, who joined my lab in 1992, on sabbatical from a faculty position at Bogaziçi University in Istanbul, Turkey. We had been doing various experiments testing the responses of male quail to taxidermically prepared females and taxidermic models of various female body parts. This line of work had identified the head and partial neck feathers of a female as “sign stimuli” for male social affiliative behavior (Domjan & Nash, 1988). However, these head and neck cues were not unconditioned stimuli. Rather, their effectiveness required prior sexual experience in which the female head+neck cues were paired with copulatory reinforcement. Copulation in quail begins with the male grabbing the back of the female’s head and then mounting on her back and making cloacal contact. Thus, the natural sequences of events that ensue during copulation can serve to pair head+neck cues with sexual reinforcement. One can also condition such cues by preparing a taxidermic model of a female’s head and neck and using this as a CS paired with copulation with a live female.

Even though the head+neck cues are not fully effective unconditioned stimuli, it seemed to Falih Köksal that they were highly relevant to the sexual behavior system since they were a part of what a male naturally encountered as it copulated with a female. He reasoned that if female head+neck cues were highly relevant to sex, then the conditioning of these cues should be very difficult to block using the
Kamin blocking design. In contrast, an arbitrary cue should be readily susceptible to the Kamin blocking effect. This prediction reminded me of the early claim that CTA could not be disrupted by the Kamin blocking procedure, also because taste was highly relevant to the US employed (Kalat & Rozin, 1972). That claim has turned out to be incorrect (e.g., Gillan & Domjan, 1977). Nevertheless, I thought that Falih’s prediction was well worth testing. I am glad we went forward because the results turned out great and opened up an important new line of research for the lab. As Falih predicted, the conditioning of female head+neck cues could not be blocked by the presence of previously conditioned audiovisual cue. However, the conditioning of a comparable CS object that did not include naturalistic quail cues was readily blocked (Köksal, Domjan, & Weisman, 1994).

Falih and I were pleased to find that conditioning of a sexually relevant CS was resistant to the blocking effect but I did not fully appreciate the significance of the results until we obtained a series of related findings. Brian Cusato and Mark Krause joined the lab and conducted a series of comparisons of sexual conditioning with a naturalistic CS (that included female head+neck cues) versus an arbitrary CS of similar size and shape without female cues. The results of these experiments showed that conditioning occurs much faster with a naturalistic CS (reminiscent of the emphasis on one trial learning in CTA). Furthermore, the conditioning is much robust in a variety of respects. A broader range of conditioned responses (including consummatory sexual responses) develop as a result of conditioning with a naturalistic CS than with an arbitrary cue. A naturalistic CS is also resistant to habituation, supports better second-order conditioning, and shows very little extinction (see Domjan, Cusato, & Krause, 2004, for a review).

Perhaps the most striking finding was obtained by Chana Akins, a former student of mine who had taken a faculty position at the University of Kentucky. She found that as you increase the CS–US interval from 1 min to 20 min, conditioned responding drops out if the CS is an arbitrary cue. However, sexual conditioning (in relation to an unpaired control) remains robust with a CS–US interval if CS is a naturalistic cue (Akins, 2000). This latter finding is reminiscent of long-delay food aversion learning and has encouraged me to develop a unified framework for thinking about CTA, sexual conditioning, and other examples of what might be called ecological learning (Domjan, 2006, 2008).

**CTA and Sexual Conditioning as Ecological Learning Paradigms**

My experiences with CTA and sexual conditioning have convinced me that to understand Pavlovian conditioning we have to consider how such learning occurs in nature. Textbooks describe Pavlovian conditioning as the pairing of an arbitrary or “neutral” stimulus (the CS) with an unconditioned or biologically powerful event (the US). However, if a stimulus is truly arbitrary or neutral, it will not happen often enough (if ever) in conjunction with a US to enable the development of association. For Pavlovian conditioning to occur in nature, the CS has to have an inherent or pre-existing relation to the US. For example, the CS might be some feature of the US that is evident at a distance or before the subject comes in intimate contact with the US. Such a pre-existing relation is necessary for the CS to be reliably paired with the US outside the laboratory and to produce the important anticipatory conditioned responses that make Pavlovian conditioning an adaptive process. (Domjan, 2006, 2008).

CTA was not an ecological learning phenomenon when it was first examined by John Garcia and Jim Smith. Their early experiments involved rats drinking saccharin before being exposed to ionizing radiation. Neither the saccharin nor x irradiation are natural events that rats are likely to encounter outside the laboratory. However, Garcia moved to a more ecological interpretation of CTA as he learned more about it, and that approach was certainly central to how others, like Paul Rozin, thought about the phenomenon.

I think CTA is a beautiful example of ecological learning. CTA no doubt evolved
as a process to reduce the ingestion of poisonous foods. A poisonous food is a multifaceted stimulus object. Some of its features (e.g., odor) are evident at a distance, whereas others (texture and taste) are encountered only with more intimate contact. The most intimate contact is to swallow something, which then activates the aversion-inducing features of the poisonous food. CTA outside the laboratory does not require the interventions of an experimenter because pairings of the CS and US are natural contingencies of the environment. The CS and US “belong” with each other, to use Thorndike’s term, or are “relevant” to each other because they are features of the same object, and the CS–US interval is determine by how the subject interacts with the object.

Sexual behavior also involves interacting with a complex multifaceted object—in this case a potential sexual partner. Numerous features of the partner are evident at a distance, and each of these can become potential cues (CSs) for subsequent olfactory, tactile, and other cues that serve more as unconditioned stimuli. Considering that sexual conditioning improves the efficiency and effectiveness of copulatory interactions (e.g., Mahometa & Domjan, 2005; Matthews, Domjan, Ramsey, & Crews, 2007), it is not a stretch to suggest that sexual conditioning evolved to take advantage of naturalistic conditioned stimuli that precede copulatory interaction and that is why learning involving such cues is more robust and occurs over longer delays.

References


Domjan, M., & Wilson, N. E. (1972). Contribution of ingestive behaviors to taste-aversion learning in the rat. Journal of Comparative and Physiological Psychology, 80, 403-412. (a)

Domjan, M., & Wilson, N. E. (1972). Specificity of cue to consequence in


Exceptional Programming Planned for Boston, Aug 14-17

Programming for the 2008 APA convention in Boston is now complete, and this year will be truly exceptional! Thanks to the efforts of Program Committee Chair Christina Williams, Division 6 has a lineup of excellent symposia, talks, and posters. Symposia topics include memory for visual objects, psychological well-being for primates and non-primates, developmental plasticity & risk and resilience, estrogen and cognitive aging, episodic memory in animals, and neural plasticity and memory consolidation dependent on sleep. And don’t forget the great programming that our allied divisions are mounting, including Divisions 3 (Experimental), 25 (Behavior Analysis), and 28 (Psychopharmacology).

Our invited addresses this year feature Stephen Suomi, Division 6 President William Timberlake, Matthew Wilson, Howard Eichenbaum, Michael Beran, and Aaron Blaisdell. Watch this space in the summer edition of BNCP for specific day/time scheduling information and updates on other programming of interest to Division 6 members at the annual convention -- plan to attend, and bring your students!

Make a Difference: CARE Makes It Easy!

*Nancy Dess*

Ever wondered how you can help advocate for the health of behavioral neuroscience and comparative psychology? What to do, and who has the time? APA’s Committee on Animal Research & Ethics (CARE) is making stewardship easier by providing ideas and products. Visit the CARE website at http://www.apa.org/science/rcr/care.html and you will find a “Top Eleven” list of advocacy ideas, from really simple to some requiring a bigger commitment, all worthwhile. You also will find ready-to-go materials for use in the Exploring Behavior Outreach Program, including modules on sex, drug abuse, sleep, eating, and spatial navigation featuring work with nonhuman animals. Download, and you and your students are ready to visit a G8-10 classroom with an exciting presentation! And stay tuned -- CARE’s third-in-a-series short videotape on lab animal and human research on touch, attachment and aggression will roll out shortly, joining its two successful predecessors (Perception & Action, Psychopharmacology) in classrooms across the country. A lot of us doing a little will do great things for the field.
Announcements

A Faculty for Undergraduate Neuroscience Workshop

The Undergraduate Neuroscience Education: Interactions, Interdisciplines, and Curricular Best Practices
Macalester College, St. Paul, Minnesota
July 18-20, 2008

Faculty for Undergraduate Neuroscience (FUN), in partnership with Project Kaleidoscope (PKAL), has sponsored four previous workshops focused on developing and sustaining undergraduate neuroscience programs. FUN and PKAL are pleased to announce a fifth FUN workshop July 18-20, 2008 at Macalester College, St. Paul, Minnesota.

At the first (1995) workshop participants developed four blueprints to guide faculty in their efforts to enrich undergraduate curricula by developing interdisciplinary courses and programs in the emerging discipline of Neuroscience. Using these blueprints as a foundation, participants at the second (1998) and third (2001) workshops explored cutting-edge exercises designed to build investigative, discovery-based laboratory experiences and launch regional meetings emphasizing undergraduate neuroscience research. The fourth (2005) PKAL/FUN workshop also featured new laboratory experiences, developed leadership skills, and revisited the four original curricular blueprints—adding a fifth blueprint in neuroscience studies to address the directions that neuroscience is headed in the coming decades.

The 2008 FUN workshop will explore a wide range of topics, including new laboratory experiences emphasizing discovery-based learning, incorporating community outreach into neuroscience education, and programming focused on the development of interdisciplinary coursework and curricular innovations to ensure that undergraduate education in neuroscience remains vibrant well into the future.

More information and registration details are available at the FUN website: FUNFACULTY.ORG. Please mark these important dates in your calendar, and plan to attend!

Questions about the FUN Workshop?
Email: Wiertelak@macalester.edu
Office Phone: 651-696-6111

Do You Teach Undergraduate Neuroscience?

Call for Submissions:
Journal of Undergraduate Neuroscience Education (JUNE)

JUNE is an electronic journal that publishes peer-reviewed reports of innovations in any area of undergraduate neuroscience education related to the mission of advancing undergraduate neuroscience on topics such as novel pedagogy and original laboratory exercises. All articles should be written for an audience of college faculty and include references to relevant literature, supplies, and/or supplemental materials such as animations, websites, etc. Figures and qualitative or quantitative assessment of pedagogical outcomes are also encouraged wherever appropriate. JUNE also invites submissions as letters to the editor and reviews of textbook, curricular, equipment, or media.

JUNE is a publication of Faculty for Undergraduate Neuroscience (FUN) and is free to read and download. Visit JUNE today at [www.funjournal.org/default.asp] or follow the links from the FUN website, [funfaculty.org].

Inquiries regarding submissions should be directed to Gary Dunbar, JUNE Editor-In-Chief at any stage in the writing process.
Gary.dunbar@cmich.edu; 989-774-3282 (phone); Dept of Psychology, Central Michigan University, Mt. Pleasant, MI 48859.