A History of Psychology
IN AUTOBIOGRAPHY

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Psychologists have noted that the way people narrate their life stories reveals an overarching theme, often one characterized by acts of redemption or the overcoming of childhood challenges. Consumed in my youth by the everyday strivings and concerns of a schoolboy, athlete, and young scholar, I was much too busy to attribute any such heady existential theme to my life. But as I look back now, a narrative theme emerges to explain much of who I am and what I’ve done. My redemption theme has been to fulfill the dreams that my father abandoned as he sacrificed for the sake of our family.

My father was an educated, cultured man who got a degree in business administration from Ohio State University in 1922. He aspired to work overseas as an international business executive and, toward that end, even learned Spanish and German. After graduating, he accepted a job with an international company. Soon thereafter, however, my paternal grandfather suffered a severe heart attack, obligating my father to return to the village of Scio in eastern Ohio to take over my family’s small grocery and general store. In fulfilling his filial obligations, he soon was inextricably entangled by sick parents and the financial obligations of his own burgeoning family. During the Great Depression, he became entrapped into lifelong work that bored him
and exercised few of his talents. Ironically, this would-be international businessman died without ever traveling outside the United States. I believe his experience spurred me—with his encouragement—to seek a profession that I enjoyed and that afforded many opportunities for foreign travel.

I was born in our home in Scio, Ohio, on December 30, 1932, my parents third and last child. My sister, Shirley, was 6 years older, and brother, Robert, 2½ years older. We grew up in this small village of about 1,000 souls in the poverty belt of eastern Ohio. Our parents, Clyde Ward Bower and Mabelle Sue (Bosart) Bower, were energetic, middle-class Christians and reasonably happy notwithstanding the Great Depression. They had owned and operated my family’s store (Bower’s Merchandise Mart) in Scio since my grandfather’s heart attack in 1925. Scio existed in sleepy isolation from the bustling world outside except for a whiteware pottery factory that employed most of the town’s workers. A few others worked in nearby coal mines or on farms. When inexpensive Asian dishes flooded the market in the 1960s, the pottery factory closed, forcing many of its former workers to seek jobs in bigger cities. The 1999 census counted only about 800 Scio residents, with 1 in 6 living in poverty. Like many small towns, Scio was vanishing. It remains frozen in my memory as it was 60 years ago: an idyllic place for a child, yet too bucolic to contain my adult ambitions.

Genetic Selection

One of my greatest and most precocious accomplishments was my selection of parents. My father was a highly educated man whose wide interests ranged far beyond that small-town store. During adolescence, I didn’t appreciate how uniquely wonderful he was. He was my first educational role model. He devoured history books and was the only person I ever knew who read encyclopedias for pleasure. (I still have his 24-volume Encyclopaedia Britannica.) He had a classical record collection that entertained us in the evenings, had a strong singing voice, and always led the church congregation in belting out hymns. He was not athletic like his sons. I never saw him deliberately exercise. Yet he enjoyed frequent country hikes with us. He would explain the geologic formations we encountered and was delighted wherever we discovered Indian arrowheads. I now wonder how he worked such long hours and maintained such a kindly demeanor with everyone. He sacrificed and saved for our college educations. When a baseball scholarship paid my way through college, he safeguarded my “college money,” which he later gave my wife and me for a down payment on the Stanford house where we still reside. In my teenage years he developed mild symptoms of Parkinsonism and began taking medication. The drugs made him drowsy—a tragic condition for a man who so loved to read.

After I left for college, my father kept in touch with letters chronicling Scio news. He died suddenly at 63 from his second heart attack. I regret that I never told him the enormous influence that he had on my life as an academic and family man.

My mother’s family believed that they descended from immigrants who came to America in the 18th century from France. (The name Bosart was likely an anglicized rendering of Beaux Art.) My mother and her 14 siblings grew up on a farm near Columbus, Ohio. All but 2 of these siblings lived into their 80s or beyond. Having already taught elementary school in a one-room country schoolhouse, Mother proceeded to work toward a teaching credential at Ohio State University, where she met and fell in love with my father. According to the custom of the day, she married and moved to northern Ohio with my father soon after he graduated. There she taught elementary school without ever finishing college.

My mother was a warm, nurturing woman who took good care of her children and grandchildren. Besides raising the three of us, she worked with my father in the store attached to our house. She became adept at seamlessly juggling household chores and store customers. We children were expected to pitch in with the many chores. When store business was slow, my mother loved substituting in the local schools. Incredibly, she could teach practically any subject to any class. She taught reading and arithmetic to third graders, Latin to ninth graders, and algebra and civics to high school students. Even in her 90s she taught reading as a volunteer to second graders. Years later the school principal told me that Mom’s versatility made her the ideal substitute. Her love of teaching undoubtedly guided me toward a career in education. My mother lived to be 98 with all her wits.
Both my parents were deeply religious and tried to raise their children that way. My agnosticism, appearing in my teenage years and beyond, disappointed them. Yet they never nagged me about it, although I imagine they prayed daily for my salvation.

With 13 living siblings, my mother had a huge family extending to my many uncles, aunts, and cousins. Similarly, my father had many Bower and Gordon relatives in nearby aptly named Bowerston. We often visited the Bowerston relatives or had big family reunions at my grandmother's farm near Green Camp. I can't recall that any of these relatives, who were farmers or blue-collar workers, ever attended college. My mother's sustaining motto was this: "Whatever happens to us, we begin and end with our families."

**Small Town Childhood and Sports**

Despite the Great Depression and World War II, I spent a Tom Sawyer-esque childhood with my older brother Robert as my constant companion and plotter. We rode bikes, fished, hunted, swam, played ball, and boxed. We grunted through muscle-building exercises that promised to sculpt us into the muscle men advertised in pulp magazines. We even took up musical instruments at about the same time. We spent long childhood hours waiting on store customers and restocking the grocery shelves from supplies kept in the basement. I worked on nearby farms during the summer months when there wasn't enough work in the store. All this persuaded me to avoid careers in business, farming, and anything involving physical labor.

After seeing the movie *The Lou Gehrig Story*, at age 8, I resolved to become a professional baseball player. I devoted thousands of hours over the next 13 years to baseball and basketball. By age 10 I had learned to pitch baseball. Although I was not yet big enough to throw hard, I developed a big, sweeping curveball that I could deliver from four different positions—directly overhead, three quarters, sidearm, and underarm. This curveball was sufficiently devastating that by ages 11 and 12, I was recruited by Scio's adult semipro teams to pitch against other teams. After I hit my growth spurt around 13 or 14, I mastered a good fastball, yet in doing so, I somehow lost my sweeping, sweet curveball and henceforth had to base my pitching on speed rather than guile. Growing up is full of such trade-offs.

Despite coming from a small, rural school, I eventually was selected for all-Ohio state teams in both baseball and basketball, with several colleges offering me athletic scholarships. Becoming an accomplished athlete taught me that skill comes from long, hard practice and that most goals worth having require persistent effort. Playing competitive sports gave me confidence that I could succeed at something that's difficult to do. I learned to ride the roller coaster of emotional highs and lows, at times as a member of a team, other times alone. Athletes learn to endure the disappointments of sudden loss and grow accustomed to the randomness of life's outcomes. The difference between a pop fly and a home run is perhaps half a centimeter on a bat. The difference between a strikeout pitch and a ball that walks in the losing run is perhaps 1 centimeter as judged by a sometimes capricious umpire. In competitive sports, I experienced the satisfaction of exercising my skills to the utmost with the occasional exhilaration of being "on" or "in the flow" at the top of my abilities. One of my most memorable days occurred playing baseball at age 16. With Scio playing a rival town's semipro team, I hit home runs clear out of the park in three consecutive times at bat. Believe me, nothing compares to that.

In my junior high years, I also got hooked on music, thanks to my sister Shirley, who trained as a classical pianist. Inspired by two trumpet-playing friends, I took up jazz trumpet with a passion, idolized Louis Armstrong, and later played in the high school dance band and in a blues band for local dances. Regrettably, I gave up the trumpet when I went to college and decided I didn't have time to maintain my limited musical ability. Another life lesson I learned: You can't do everything well, so devote your time to your main passion and put aside competing activities that provide insufficient reinforcement. This lesson later guided me through evolving topics of psychological research.

I was a bright student in grade school but worked just enough to maintain adequate marks. Eventually, my family and several inspiring teachers fanned my intellectual interests. My greatest inspirations were two high school teachers, Virginia and Jim Wiggins, who took an interest in my mind rather than my pitching arm. Jim was vastly educated, thoughtful, and concerned with the larger issues of national
life; Virginia was vivacious, dazzling, brilliant, dramatic, and cosmopolitan. They got me to take my studies seriously, initially in chemistry, later in psychoanalysis. In many fireside chats we planned my future, eventually deciding that I should become a psychiatrist. That’s when I began to study the books of Sigmund Freud, Carl Jung, and Alfred Adler in earnest. Virginia Wiggins especially inspired me to set my sights on high intellectual achievements. Her influence had much to do with my graduating in 1950 as valedictorian of my Scio class—of 24 pupils, no less. My cultural interests had expanded beyond the confines of my boyhood village, and I was eager to move up to a larger “playing field.”

UNIVERSITY YEARS

Just out of high school, I turned down a few offers to play professional baseball or college basketball to accept a baseball scholarship at Cleveland’s Western Reserve University (WRU, now Case-Western Reserve). The Cleveland Indians baseball team was sufficiently interested in me to contribute to my scholarship with the understanding that they would have first option if I ever signed a professional contract. I excelled at pitching for 4 college years and in summer semipro leagues, and I was a decent long-ball hitter. I would have loved to pursue professional baseball, but my military draft board gave me only two options: continuing into graduate school or being conscripted into the U.S. Army and sent to Korea. Opting for graduate school was fortuitous. Furthermore, the failure statistics in professional baseball suggest that I might have wound up managing a bowling alley or selling used cars in Cleveland. So I learned early on to accommodate my dreams to the available options, which already greatly exceeded those imposed on my father.

Going to college in Cleveland was mind-blowing for a culture-starved, small-town boy. I had a voracious appetite for jazz bars, vaudeville, museums, art lectures, union hall socialist debates, symphonies, the teeming masses, political campaigns, dramatic theatre, ballets, the Cleveland Indians, and the Cleveland Browns football team. I drank in learning, sports, and arts like a dried-out dromedary at whatever cultural trough Cleveland offered. For my first several years, I couldn’t get enough of it. Cleveland’s informal education of my developing mind exceeded that of the university.

Intending to become a psychiatrist (which requires an MD), I enrolled in the premedical program. Among the required courses were two of the hardest, most mind-numbing courses I ever suffered through: comparative anatomy and comparative embryology. Both involved horrendous amounts of pure memorization of the muscles, bones, and nerves of assorted animals. After the exams, I promptly forgot most of it. My early life as a student was salvaged by the elective courses I took in literature, history, and especially psychology. Fittingly, the WRU psychology department at that time had a strong psychoanalytic bias. The head of the department, Calvin Hall, was a renowned Freudian who later wrote such books as A Primer of Freudian Psychology (1954) and the Hall and Lindzey text Theories of Personality (1957). Hall took an interest in me because of my Freudian ambitions and because he was an avid baseball fan. He tutored me on the fine points of psychoanalytic theory.

Besides pitching for WRU’s team, part of my agreement with the Cleveland Indians was to continue pitching during the summers in semipro city leagues. Because most games were played at night, I needed a day job. Because I had only read about “crazy people,” I thought it was time to meet a few. After my freshman year, I applied for a summer job at the Cleveland State Mental Hospital. Because I was big, the administrators assigned me to be an attendant on the back wards that housed the more deteriorated patients with psychoses. There I encountered no patients who had anything in common with the upper middle class patients with neuroses who Freud and his disciples described in turn-of-the-century Vienna. Most of the inmates were impoverished, poorly educated, mildly disoriented and withdrawn, abandoned by relatives, and anxious about their fate in the hospital system. Some were too depressed to maintain even rudimentary hygiene. Their condition and that of the wards generally were disheartening to me.

When some psychiatrists at the hospital learned that I was planning a psychiatry career, they invited me to sit in on their case conferences. There I noticed how often psychiatrists differed in their diagnoses of intake patients and what treatments, if any, patients should receive. This was in 1951, before psychoactive drugs were introduced on a
the premier program in philosophy of science, and I studied with such outstanding leaders as Paul Meehl, Herbert Feigl, Wilfred Sellars, and Michael Scriven. I also took classes in mathematical statistics and stochastic processes.

**RELIGION**

Throughout my late teens and early 20s, I was grappling with whether to believe any form of deism, including Christianity. This was the legacy of being brought up by God-fearing Christian parents who required me to attend stultifying church services. I resented being forced to dress up in my "Sunday finest" clothes (that were hot and scratchy) and attend services every Sunday. I was also annoyed by having to attend vacation Bible school during the summers while my buddies were outside playing baseball. So, during my first stirrings of adolescent rebellion, I decided to investigate the intellectual foundations of beliefs in deism—a quest that continued until my mid-20s.

I began reading tracts on atheism that destroyed the several arguments for God, the infallibility of Biblical revelations, and the divinity of Jesus of Nazareth: I read books by Thomas Paine, David Hume, Robert Ingersoll, H. L. Mencken, Bertrand Russell, Sidney Hook—whatever I could find in the county library. Later I read more sophisticated rational (atheist) philosophers such as Ernest Nagel, Michael Scriven, A. J. Ayer, George Smith, and Anthony Flew. I pestered with skeptical questions any minister or priest who would talk to me, hoping they would somehow persuade me to believe in their God. I discovered that many of them were unfamiliar with the details of arguments for or against the existence of God(s) and, in any event, were poor at convincing nonbelievers to their view. My growing skepticism distressed my parents; they were mollified somewhat by my agreeing to at least attend church services with them while living or visiting at home.

At the University of Minnesota, the popular approach taught by the professor of theology, Paul Holmer, was religious existentialism, especially the brand associated with Søren Kierkegaard. I attended Holmer's lectures and read some Kierkegaard, but Kierkegaard's "poetic subjectivism" led me eventually to conclude that he had simply

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large scale. The psychiatric staff was overwhelmed, and little could be done for the patients beyond therapeutic hot baths and some electroshock. I recall wheeling patients in and out of the electroshock room and questioning whether this could really be doing them any good.

**FLEETING THE MENTAL HOSPITAL**

With my ward experience, I was hired the following year as a research assistant to the hospital psychology staff. During that year, among other chores, I helped a WRU doctoral student collect data for his dissertation, which was a reliability check on the Blackey projective test with senile patients with psychoses. My testing interviews with patients were, by turns, hilarious, stupefying, depressing, and deeply distressing. I remember thinking, "This can't really be what meaningful psychological data should look like!" As a result, I abandoned my goal of becoming a psychiatrist; my experiences at the Cleveland State Mental Hospital were simply too disenchancing. Although I remained sympathetic to patients with mental illness, I felt that therapies based on better science were needed.

As if on cue, a new WRU professor arrived that year to arouse my interest in experimental psychology. Charles R. Porter was a newly minted Yale PhD who was enthusiastic about Clark Hull's behavior theory as hard science. That orientation fit into my developing conviction that applications of psychology in clinical work required fewer clinical case studies and more experimental evidence. Psychoanalysts would say I was exhibiting reaction formation in defending against my basic wish to help people with mental illness.

Porter recruited me to help him formalize Hull's theory into the language of symbolic logic and mathematics. In retrospect, it was a futile project, but it provided the kind of conversion experience that budding scientists require from their teachers. Through my association with Porter, I acquired interests in philosophy of science and methodology, topics of intense concern within psychology at that time. He also persuaded me to study higher mathematics, convinced that theoretical psychology would become more formalistic and quantitative. After graduation in 1954, I obtained a 1-year Woodrow Wilson fellowship at the University of Minnesota to further such study. Minnesota had
retreated from the battleground of reasoning: He and Holmer provided no new convincing reasons for belief in God; they even decried the attempt. Rather, they argued that reason was of no use for this decision and that the matter came down to a subjective leap of faith, rather like Blaise Pascal's famous wager. I couldn’t understand why anyone should abandon the reasoned path to enlightenment, especially on such an important and momentous claim as deism.

During this time, I was also occasionally meeting for religious discussions with my Minnesota teacher, psychologist Paul Meehl, who became both a friend and an enigma to me. On the one hand, Paul taught high-level seminars in philosophy of science and methodology of psychology; on the other hand, as a result of some transforming conversion experience, he had become a seriously devout Lutheran. We would discuss how religious truths of theology might be accommodated within the analytic philosophy of science we both respected. I regret that those discussions never came to any convincing conclusions and left me as skeptical as ever. In later life, my skepticism was sustained by a subscription to The Skeptical Inquirer. This journal publishes contributions written by kindred folk who investigate (and usually debunk) claims to such paranormal phenomena as poltergeists, ghosts, aliens from outer space, angels, spiritualism, miracles, faith healing, bleeding religious icons, dousing, the Bermuda Triangle, wheat field whorls, ESP in remote viewing, and other mysterious quirks of human gullibility.

DISCOVERING MATHEMATICAL LEARNING THEORY

My imprinting on rigorous theorizing through mathematics derived from interactions with my WRU professor, Charles Porter, and our admiration for the writings of Clark Hull. Although I was taking classes in higher mathematics at Minnesota, that quantitative imprinting lay dormant until I stumbled on a suitable releasing stimulus from psychology. In a class at the University of Minnesota, “Mathematics for the Social Sciences,” my professor lectured on a paper by William Estes on statistical learning theory (Estes, 1950). The paper grabbed my interest, and I rushed to the library to read it. This was exactly the kind of theorizing that Charles Porter and I believed psychology needed. I also stumbled on some papers by Bob Bush and Frederick Mosteller that paralleled Estes’s work; furthermore, their book, Stochastic Models for Learning (Bush & Mosteller, 1955), appeared that year to consolidate my interests. I was so taken with the approach that I conducted an informal weekly seminar on the book for interested graduate students at Minnesota.

Although I enjoyed and benefited from my year at Minnesota, I always intended to proceed to postgraduate study in psychology. I received fellowship offers from Harvard (with Bob Bush and Dick Solomon) and Yale (Neal Miller). After I talked with Miller on the phone, I knew that he was the one.

YALE GRADUATE SCHOOL

In 1955, I entered graduate school at Yale to study the psychology of learning as a research assistant to Neal Miller. I conducted collaborative research with Miller for 4 years. He was my master teacher, my guru. He was the perfect role model and father figure. He was dedicated, projected reverent values about science and its progress, had a profoundly inventive mind, was deeply involved in his own research, and encouraged me with his interest in my ideas. He influenced my approach to research more than any other teacher. Although he was about 20 years my senior, we formed a close and enduring friendship. I frequently wrote or called him for advice when I was struggling with some scientific or personal issue. I was greatly saddened by his death in 2002.

Nevertheless, my initial research with Miller came up empty. He and his associates had shown that injecting a tiny bit of saltwater into the ventricles of a cat’s brain would cause it to drink enormous amounts of water. If this were causing real thirst, then we reasoned that it should serve as a discriminative stimulus. So my task was to get the cats to turn one way in a T maze to get a reward (a free romp in the lab room) after they had just received a saltwater injection into the ventricles and to turn the other way after an injection of isotonic saline (same as body fluids). After struggling for many weeks to get the cats to move at all through the T maze (the romp proved insufficiently rewarding), I discovered that the saltwater injections were no longer causing excessive drinking, probably due to brain tissue damage around the injection area. So, with great relief, I was allowed to abandon
that project. This taught me early on that it was okay to abandon unproductive lines of research!

Next, Miller and I investigated the newly discovered reward effect from brain stimulation. Jim Olds had found that rats would press a lever avidly to get a brief, tiny jolt of electricity delivered to a spot in their brains (limbic system). Miller’s interest in motivation and reward prompted him to direct most of his lab group (15 people calling ourselves “Miller Industries”) to seek locations in the rat’s brain where electrical stimulation would evoke biological drives and/or rewards. During this time, I was learning how to implant indwelling electrodes to stimulate different areas of the brain and how to perform histology on brain slices. My first research publication reported the discovery of dual reward–punishment locations in the rat’s limbic system. My rats would press a lever to turn on the brain stimulation; but as the stimulation continued beyond 0.5 second, it became painful and the rat would learn to make another response to turn it off. Other tests proved that the continued stimulation was aversive and that the rats were not just turning off the stimulator to initiate another joy buzz. Although watching these turn-on, turn-off cycles was fascinating at first, I tired of them long before my rats ever did. E. R. Hilgard’s Introduction to Psychology (1957) textbook published a photograph of Neal and me testing one of my rats.

I gave my first speech at an American Psychological Association (APA) convention on this dual reward–punishment effect. As a graduate student I was anxious because Jim Olds, the founder of that field, was speaking just after me. I was greatly relieved when after my talk Olds popped up to say that he “could support these important observations.” The deepest professional dedication is hewn from such rewards, just as was my rats’ addiction to lever pressing for brain stimulation. So I’ve made a point of always rewarding without delay the experimental findings of my own students.

Miller knew that I was more interested in learning theory than in rodent reward and motivational hot spots. He encouraged me to follow my own experimental ideas, some involving his conflict theory, others applying reinforcement schedules to escape learning. So for my 2nd year I applied for and received a National Institute of Mental Health (NIMH) fellowship to support myself. I also worked with Yale’s principal learning theorist at the time, Frank Logan. A brilliant psychologist who received his doctorate under Kenneth Spence at Iowa, Logan developed the so-called micromolar theory of behavior. The gist of the idea (expressed in Hullian terms) was to treat each speed or amplitude of a given response (e.g., the rat running down a runway or pressing a lever) as a distinctly different response, which could be selectively strengthened or weakened by the reinforcement contingencies for that speed or amplitude. Logan theorized that people and animals adjust the speed of their responses to maximize their net utility, defined for each response speed as the quality of the reward minus the effort or punishment involved for performing at just that speed. The micromolar theory contrasted with the popular molar theory, which held that animals’ performance speed reflected the degree of arousal produced by their reward, independent of how fast they performed. I was intrigued with Logan’s attempt to quantify his brand of Hullian theory.

My other Yale professors were Bob Abelson (who taught multivariate statistics plus a small seminar in mathematical models), Carl Hovland (experimental design), Tom Corno (carpentry shop and psychophysics), Bob Cohen and Jack Brehm (social psychologists). Fellow graduate students who I recall included Phil Zimbardo, Tim Brock, Earl (Buzz) Hunt, Don Jensen, and Lyman Porter. Because we experimentalists were expected to build our own apparatus, stimulus generators, and recording devices, the shop course proved to be extremely useful. I learned some carpentry, metal bending, soldering, electrical circuitry, and how to use power tools. These skills would allow me to build my own equipment when I set up my animal lab at Stanford.

Marriage

In 1957, midway through graduate school, I married Sharon Anthony. We had met and fallen in love in the summer of 1952 at a 6-week Encampment for Citizenship gathering of about 100 college students in the upper Bronx. It was sponsored by the Ethical Culture Union, a liberal and enlightened sectarian society that established the annual encampment to teach grassroots democracy to putative future leaders of America. During our 5-year courtship, Sharon finished her undergraduate studies at Gustavus Adolphus College in Minnesota.
and earned a master's degree in theatre arts, specializing in directing, at Northwestern University.

At the time of our marriage, she was a theater director and assistant professor of dramatic arts at Louisiana State University. She relinquished her good university position and exchanged it for three part-time, lousy jobs in New Haven so we could be married and live together at Yale. Just as my parents never questioned Mother's dropping out of college in 1922 to go with my dad, in 1957 we never questioned that Sharon's choice was expected and, therefore, right. How attitudes about gender roles have changed for the better since those days! Fortunately, at Stanford, Sharon and I have been able to satisfy both careers: She quickly trained in Stanford's counseling psychology program, formed a successful communication consulting firm, and has published three self-help books on assertiveness and overcoming public speaking anxiety. We have remained happily married and raised three rewarding children who continue to enrich our lives: Lori, born in 1959; Tony, born in 1962; and Julia, born in 1964.

THE 1957 SOCIAL SCIENCE RESEARCH COUNCIL SUMMER WORKSHOP

In the summer of 1957, after my 2nd year in graduate school, I attended a workshop on mathematical learning theory sponsored by the Social Science Research Council (SSRC) at Stanford University. There, I had the opportunity to work with many psychologists who became key innovators in the mathematical psychology movement of the 1960s. Among them were Norman Anderson, Dick Atkinson, Bob Bush, William Estes, Eugene Galanter, David LaBerge, Duncan Luce, George Miller, Frank Restle, Saul Sternberg, and Patrick Suppes. These people were creating and testing the kinds of mathematical theories that predicted behavioral data with the level of precision that I had earlier dreamed about.

This was a formidable collection of intellects for a 2nd-year graduate student to move among. Because I hadn't heard of any of them except Bush and Estes, I presented my ideas as if I were among peers. Years later Bill Estes told me that I actually had been invited only to be a student observer and was, in his words, "expected to sit silently and be enthralled by others." That role definitely did not suit me. That workshop produced a 12-chapter volume, Studies in Mathematical Learning Theory (Bush & Estes, 1959), to which I contributed two chapters. During that time I formed a close intellectual kinship with Bill Estes that resulted in a continued exchange of ideas for many years.

One model I worked on that summer was the vicarious trial and error (VTE) model for choice point behavior (Bower, 1959). The term vicarious trial and error was coined by Edward Tolman to describe the way animals at the choice point in a T maze look back and forth between the two alternatives, perhaps vicariously comparing predictions about which choice will lead to reward. I formulated this as a simple random walk, with the animal starting from the neutral point, examining each of the alternatives, and making a final choice among them. Using some data I'd collected on rats learning to find food in a T maze, I was able to match this model to the probability distribution of VTEs and the choice probabilities as well as the way both indicators changed throughout learning. Estes and I later extended the VTE model to describe human choices among commodity options.

Back at Yale for the next 2 years, I worked increasingly with Frank Logan on his micromolar behavior theory. I did my dissertation with Logan and Miller as coadvisors, confirming predictions of the micromolar theory about how animals learn to adjust their response speed to minimize the time before a correlated, delayed reward can be obtained. I graduated in June 1959.

PARLAYING A WORKSHOP INTO WORK

My job at Stanford came about through my SSRC workshop acquaintance, Pat Suppes. He was an assistant dean at the time and had received SSRC funding to set up a Stanford program in mathematical modeling in the social sciences. Suppes urged the psychology head, Bob Sears, to hire me with the SSRC funds. After checking with his former Yale buddy, Neal Miller, Sears offered me an assistant professorship at Stanford 1 full year before I completed my doctoral dissertation. By sheer luck I dodged those dreaded job talks and interviews that are anathema to every job applicant. Apparently my performance at the SSRC Summer Institute, my correspondence with Estes
about my theories, and some kind words to Sears from Miller did the trick. At the time, such executive hiring decisions on college campuses were common; today they would set off an avalanche of litigation.

The summer after I completed my doctorate, I obtained a NIMH fellowship to work with my friend Larry Stein at a Pittsburgh VA hospital lab before proceeding to Stanford. With Larry, I learned how to use Skinner boxes to study practically any question about animal learning or motivation. These skills proved invaluable in setting up my animal laboratory at Stanford.

**My Evolving Research Program at Stanford**

In the fall of 1959, I began my first academic job in the Stanford Psychology Department, where I have spent my entire career. In 2005 I became an emeritus professor after 46 years of teaching and research.

My Stanford research can be classified into seven categories that track successive, if overlapping, periods of my professional life. These research categories are animal learning, mathematical models of memory, memory organization and mnemonics, human associative memory, emotional influences on cognition, models of category learning, and narrative memory and mental models.

**Animal Learning**

On arriving at Stanford, I began a program of research on conditioning and learning. I was given lab space in an old Quonset shack left over from World War II, sharing this space with other researchers, including Douglas Lawrence. Lawrence received his doctorate under Neal Miller at Yale and did important research showing acquired distinctiveness of cues in rats. Doug Lawrence, Ernest Hilgard, Bill Estes, and Leon Festinger were four of my main supporters at Stanford.

Thanks to my carpentry shop training, I was able to set up my own animal learning laboratories. I built runways, shuttle boxes, discrimination boxes, and Skinner boxes, all outfitted with elaborate electronic timing and counting circuits. I made most of this equipment from relays, variable capacitors, and stepping switches cannibalized from discarded pinball machines found at a junkyard. Later, NIMH grants allowed me to buy more sophisticated programming equipment for the Skinner boxes.

My research at Stanford began with studies of operant conditioning with an especial interest in interactions between Pavlovian and instrumental conditioning. In addition, I studied incentive motivation, frustration caused by reduced rewards, errorless discrimination learning, correlated reinforcement scheduling, reward contrast effects (how the performance sustained by a given reward schedule varied with its relation to other reward schedules the animal received in alternate stimuli), and the value of advanced (but useless) information about an upcoming reward. There is not enough space here to summarize this research, which is referenced in my vita on my Web site: http://psychology.stanford.edu/~gordon.

Besides teaching in the mathematical models program, I also taught graduate classes in learning theory and an undergraduate lab course on conditioning and learning. In that lab course, each student was assigned a rat to put through its paces on many operant conditioning procedures. A number of those students became my research assistants, and some went on to graduate school and careers in psychology.

My publications in animal learning led to early appointments as a consulting editor for the leading journals of the day: the *Journal of Experimental Psychology*, the *Journal of Comparative and Physiological Psychology*, and the Skinnerian *Journal of the Experimental Analysis of Behavior* (JEAB). This gave me the curious distinction of being the only person to have served on the editorial boards of both JEAB and *Cognitive Science*—two journals that could not have been further apart in their perspectives!

Among my many interests in those early years was behavior modification and behavior therapy. I taught courses in this area and lectured at behavior therapy conventions, where I became acquainted with the leading researchers of that movement. In 1976, my wife Sharon and I published a self-help book, *Asserting Yourself*, based on her work with clients. It became a self-help classic and is still in print some 30 years later (Bower & Bower, 1976/1991). My work and lecturing in this field prompted my appointment as a consulting editor to a clinical journal, *Cognitive Therapy and Research*. I was an oddity—the only person who had served simultaneously as an associate editor for a
clinical psychology journal and the *Journal of Mathematical Psychology*. However, the spectrum of my editorial jobs reflected the diversity of my research interests.

By the late 1960s, my animal research was being crowded out by my interest in mathematical models for human learning—a topic I'd learned absolutely nothing about in graduate school. Because I had had many rewards from animal research, it was with sadness that I closed down my animal lab around 1969 when the Stanford department moved from Cubberley Hall into the newly renovated Jordan Hall. I nonetheless kept up with the field by revising Ernest Hilgard's *Theories of Learning* textbook several times (see Hilgard & Bower, 1966, 1975; Bower & Hilgard, 1981). Even as I moved into other fields, I never abandoned my roots in animal learning.

**Mathematical Models of Learning**

In the early 1960s, Bill Estes and Dick Atkinson joined the Stanford faculty. Estes, Atkinson, Pat Suppes, and I formed the core of our mathematical psychology program. Throughout the 1960s, our program attracted students who became major contributors to the field, including (alphabetically) Bob Bjork, Jim Hinrichs, Douglas Hintzman, Steve Link, Elizabeth Lofrus, David Rumelhart, Rich Shiffrin, Jim Townsend, David Wessells, George Wolford, Jack Yellot, and Joe Young. These stellar students made those early days some of the most rewarding of my career.

Along with my animal learning research, I gradually began using mathematical models to theorize about human learning, in part because I could quickly collect large amounts of learning data from humans (see below). The dominant theory at the time proposed that human learning proceeded gradually by accumulation of increments in the probability of a successful response over training trials. However, under Bill Estes's leadership, several of us began looking for all-or-none or discrete stages as training proceeds in simple learning situations. We found them in a number of cases.

I started using the simplest one-step model to describe, for example, how a college student might learn a single paired associate in a list of pairs (e.g., pairing nouns arbitrarily with the digits 1, 2, or 3). The model assumes that subjects begin without knowledge of what number is paired with a given word; so prompted with the word, they can only guess at the correct digit, being correct with some chance probability \( g \) (.33 here). With each study trial, the subject might learn this specific association with probability \( e \), so he'll respond correctly to the word for the rest of the session. With probability \( 1-e \), he doesn't learn this pair on this occasion and so will remain in the guessing state, entering his next test-and-study trial on this pair.

Let me illustrate the allure of such a simple mathematical model. As noted before, I was earlier converted to the idea that psychological theories needed to become more quantitative and able to predict empirical results with far greater precision than such simple rankings as "Condition A will outperform Condition B on some behavioral indicator X." Theories that deliver quantitative predictions are more readily disconfirmed and modified; conversely, when they predict the observed data correctly, they gain far more credence than would a simple qualitative ordinal prediction. I illustrate with the simple all-or-none model outlined above.

Descriptively, the goal of any such model is to predict any aspect (or statistic) of a collection of data, assuming that real subjects are learning according to the process envisioned by the model. For example, suppose we test the model we recruit 30 college students to learn in one session 24 paired associates (24 words as stimuli randomly paired with the digits 1, 2, or 3 as responses). Suppose that each subject receives repeated test-then-study trials of the whole set of 24 items for 40 trials or until each subject learns (reliably stops making errors). Assuming the subjects and items are roughly comparable, we'd end up with \( 30 \times 24 = 720 \) sequences, each comprised of 40 bits corresponding to correct and error responses of a single subject to a single item (28,800 in total).

We can describe (but not explain) those data by many different statistics, the most common of which is the learning curve (probability of a correct response on any trial). But there are far more discriminating statistics, such as the probability distributions of (a) the number of errors before the first correct response, (b) the total number of errors per item, (c) the trial of the last error per item, or (d) the number of runs of errors of any length, say, three in a row on any item. Compared with the average learning curve, these statistics give far more revealing
snapshots of the underlying process that is generating the 28,800 observations.

The theoretical claim is that after estimating two numbers—c, the probability of learning an item per study trial, and g, the probability of guessing correctly before learning (probably .33 with three response alternatives)—the model can explain and predict practically any statistic of the data matrix. This grandiose claim sounds preposterous—720 sequences of correct and error responses, all captured by two theoretical numbers, c and g. What is astounding is that in some cases, this fantastic claim is true—that is, the predictions of the model are very close to a large number of the statistics of observed learning data. That, in turn, validates the theory from which the predictions are derived.

A strong prediction of this model is that whenever a subject makes an error on an individual pair (indicating that he or she has still not learned it), his or her future performance on this practiced-but-failed item would be identical to that with an equivalent new pair. For example, the probability of an error on the trial following that error on a given item should remain constant over practice trials, regardless of how many prior study trials the subject has had on this item. Moreover, over trials prior to the last error, the subject’s performance should remain near the chance level. I found a few cases of associative learning in which these startling qualitative predictions were upheld.

I also found that the model could be extended to describe hypothesis-testing behavior of subjects learning very simple classifications (concepts) in the standard trial-by-trial procedures that overtaxed memory. This approach directly opposed the then-popular incremental learning theories of discrimination learning. My collaborator on this concept identification research was my first postdoc, Tom Trabasso. Together, we applied the all-or-none model to many classification learning studies, reviewing many in our book Attention in Learning (Trabasso & Bower, 1968). It’s been said that this all-or-none model was in many respects the most simple, elegant, and powerful one of that era.

These results and those provided by Estes and Irwin Rock provoked much debate from subscribers to the incremental theory that postulated gradual accumulation of strength in stimulus–response habits. On the one hand, these debates unleashed a torrent of experimentation, with researchers (including me) finding that only in restricted cases was learning of the all-or-none type completed in a single random jump in an individual’s knowledge about a pair. On the other hand, the incremental model that expected continuous strengthening of associations over study trials was never able to fit any quantitative data whatsoever. It was surprising, however, that in cases in which the simple all-or-none data pattern did not strictly hold, we could observe one (perhaps two) intermediate stage(s) that arose between a subject’s initial complete ignorance and his or her later complete knowledge of a given item. So, as learning tasks became more complicated, we could add another stage to the theory to describe how learners overcame those added complications. These multistate models could fit in quantitative detail the data from a number of multtrial learning experiments. Clearly, my recurring theme was precision in predicting details of the behaviors of learners.

**Short-Term Memory**

As I weared of the incremental versus all-or-none debate, I gladly joined a group of researchers studying short-term memory. We were trying to explain single-trial, short-term memory data collected over retention intervals of a few seconds. For example, adults typically show very poor recall of three briefly studied, unrelated words after 10 to 15 seconds of doing distracting arithmetic. The group was also impressed by Brenda Milner’s famous amnesic patient, H.M., who could not learn new facts although his immediate memory was normal.

Following the lead of Donald Broadbent, a group including Dick Atkinson, me, and some of our students formulated mathematical descriptions of how information might be transferred from a limited-capacity, short-term memory store into a more permanent, longer term memory. Throughout 1963, I worked on both a time-decay queuing model and on fixed-space displacement models of how information in short-term memory might be lost (forgotten) before it could be successfully encoded into long-term memory. I presented one of these
models at a Russell Sage conference at Princeton in the summer of 1964. The model assumed that a presented item (e.g., a word or a word pair) would be registered in a sensory buffer, with attention transferring it into a short-term store. Once in the short-term store, rehearsal could maintain it there for a while or mnemonic coding might transfer its trace to a longer-term store. If the item were not transferred to long-term memory within a short time, it would decay or be bumped out of short-term memory and forgotten.

The flow of a single item over time through the system could be formulated as a moment-by-moment Markov process. This model generated consolidation and forgetting curves that showed the probable location of a single presented item over time since its presentation. These curves resembled those observed empirically in numerous experimental paradigms and provided explanations for many findings on short-term memory. Unfortunately, that model was locked up in the proceedings of this obscure Russell Sage conference, not to be published for 3 more years in a New York Academy of Sciences monograph (1967a). Ever since then, I have taught my students: Don't allow good ideas to be buried in obscure conference proceedings. Publish your best work quickly in major journals.

Throughout this time, I benefited from summer workshops discussing memory models with psychologists Dick Atkinson, Ben Murdock, Don Norman, Saul Sternberg, Nancy Waugh, and Wayne Wickelgren. We were all circling around the same set of general ideas. In 1965, Dick Atkinson and Richard Shiffrin (1965, 1968) made several improvements to the basic framework and used the model to predict quantitative results from many new experiments. That model guided research on short-term memory for many years.

In 1967, I published a chapter in which I worked out the consequences of the idea that the memory trace of an event has multiple components connecting together many attributes or descriptors (Bower, 1967b). These descriptors or properties could serve both to differentiate and to retrieve the memory trace from among other memories. This notion of multiattribute memory traces became popular among memory theorists and was later central in the models of Hintzman, Humphreys, Metcalfe, Murdock, McClelland and Chappell, Rumelhart and McClelland, and Shiffrin and Steyvers. It even penetrated into neuropsychological theories of the memory trace, as popularized in Damasio's idea of convergence zones (in the hippocampus) for different attributes of a given memory trace.

**Organizational Factors in Memory**

Most of the older work in verbal learning used lists of unrelated items as the materials to be learned. The major idea was that people use a divide-and-conquer strategy: They divide the whole set into small parts, learn the parts, and put them together again. In agreement with this philosophy, in the late 1960s memory research was following up on George Miller's ideas on chunking—that short-term memory was limited in terms of chunks, not the number of elements in the chunks. One of my chunking experiments, for example, showed that free recall of 30 familiar idioms like "kick the bucket" and "happy New Year" was as good as 30 single nouns and considerably better than if the words of the idioms were all mixed up into random triads.

An attractive idea was that a person's immediate memory (allegedly of seven items) could be greatly expanded by embedding chunks inside other chunks in a hierarchy. In an experimental demonstration, my students and I showed that subjects who briefly studied 112 words organized on a page into four conceptual hierarchies recalled about 70 words; this was about three times more than control subjects recalled who saw the same words presented in a disorganized scramble. As expected, subjects who studied the hierarchical organization used it as a retrieval plan, recalling from the top down, using recalled upper level categories to cue retrieval of subcategories below them. The comparison of 70 versus 7 items in immediate recall dramatically demonstrated how conceptual hierarchies and iterative cuing help people pack and unpack their memory to recall a large collection of items.

Throughout the late 1960s and early 1970s I attended annual summer conferences on verbal learning and memory. About a dozen of us would get together for a week at the Lake Arrowhead Conference Center near Los Angeles to present our research and have it critiqued by the group. This group included Delos Wickens, Endel Tulving, Leo
that learning accelerates when the elements are grouped into stable, familiar chunks.

**Mnemonic Devices**

My work on mnemonic devices was closely related to that on memory organization. Mnemonic devices are mental maneuvers used by stage magicians for centuries to help them memorize impressive amounts of material quickly. Surprisingly, only a smattering of laboratory research had ever been conducted on the principles underlying these mental gymnastics. So I began laboratory studies on mnemonics. Hearing of my research, the National Mnenomics Association (comprised mainly of magicians and stage mnemonists) invited me to speak at their convention, held at the Magic Castle in Hollywood. One example of a mnemonic trick is to convert arbitrary collections of items into meaningful themes and stories. One of my laboratory studies illustrated how effective this device can be. While studying lists of 10 unrelated nouns, some college students were instructed to compose a narrative story that linked the ordered words in a meaningful manner. After studying 12 such lists, these subjects later recalled nearly seven times as much material as did uncoached controls. These results illustrate the huge benefits of mnemonic aids. Another experiment found that short-term recall soared if students were instructed to quickly convert briefly presented nonsense trigrams into meaningful phrases, for example, CHS coded as Call Home Soon. These experiments made the point that recall is greatly enhanced when learners relate novel materials to more familiar, meaningful units. This notion of elaborative encoding made its way into educational applications, such as mnemonics for teaching foreign vocabulary.

Also, mnemonic techniques frequently emphasize converting learning materials into visual images. At the height of behaviorism, mental imagery was a taboo subject. Nevertheless, I wanted to see what happened when we had college students make up interactive images to learn unrelated word–word associates. We found that students who used imagery to study successive lists of paired associates recalled two or three times as many pairs as did un instructed controls. Following
Allan Paivio, I hypothesized that the advantage stemmed from the person’s entering meaningful traces redundantly in both verbal memory and a coordinated imagery memory. Beyond these memory advantages of imagery, our knowledge of the functional properties of mental imagery was greatly advanced through many ingenious experiments by Allan Paivio, Roger Shepard, and Stephen Kosslyn.

**HUMAN ASSOCIATIVE MEMORY**

Having studied the mnemonic benefits produced by subjects’ elaborative encoding of materials, I had no quarrel with the depth-of-processing proposal of Fergus Craik and Bob Lockhart (1972). However, I sought a more basic analysis of why elaborative encoding enhanced learning. That concern led John Anderson and me to propose a theory of how people use their conceptual knowledge to encode and remember new material, especially events and factual assertions. That theory was set forth in several articles and our book, *Human Associative Memory* (Anderson & Bower, 1973), mnemonically known as HAM.

To conduct that theoretical work, I needed to learn some psycholinguistics and computational modeling. I picked up the psycholinguistics by attending a summer institute run by the Linguistic Society of America. I took classes on transformational grammar from a Chomsky disciple, on semantics from George Lakoff and Charles Fillmore, on child language acquisition from Eve Clark, and on psycholinguistics from Herb Clark.

I had picked up the computational modeling component in 1963 by attending a summer institute at RAND Corporation run by Herb Simon and Allen Newell. They gave me such a deep appreciation for computational models that I’ve always urged my students to become conversant in this area. I promoted such modeling as a way to avoid a common pitfall of psychological theories. Too often our informal theories fail to specify sufficient steps showing how specific predictions or explanations follow from a general theory. A complete explanation of a behavioral phenomenon should enable us to see, step-by-step, how a verbal theory implies the behavior. Computational modeling forces us to fill in those steps explicitly. (Here again is my theme of precision in theoretical predictions of behavior.)

These summer educational excursions into computational modeling and language prepared me for the theoretical work that I did with John Anderson. We started with the ideas of a preformed, static semantic memory and question answering that Allan Collins and Ross Quillian had popularized. However, any useful theory of learning should describe the way a person’s preexisting concepts support acquiring new facts and events as well as answering questions about previously learned facts.

Anderson and I wanted to ground our work in association theory. We believed that people learn a new fact or episode by interassociating instances of preexisting concepts. Consider a statement (or event description) like “I met my friend John today in Vancouver.” We supposed that people record that statement (or fact) in their memory using preexisting concepts from their semantic memory of friendship, meetings, John, and Vancouver.

Anderson programmed a small parser that converted sentences to grammatical tree structures. We supposed that a person reading or hearing such a sentence would (a) set up a new memory unit for the underlying propositions, (b) create new instances of the preexisting concepts, and (c) link them together into subject–predicate structures. Thus, the grammar of the proposition recorded in memory tells the subject who’s doing what to whom, where, and when. This information enables learners to answer specific questions about the statement (event) from memory.

Anderson and I elaborated these ideas about proposition learning in our book *Human Associative Memory* (Anderson & Bower, 1973). We assumed that these associations would be strengthened by repetition and weakened by time decay and interference. The book reported many experiments on subjects’ memory for collections of novel factual assertions and showed that the HAM model fit those data in quantitative detail. Our aim in doing so was to link results from the human learning tradition with the ideas from computational linguistics, perceptual scene analysis, semantic memory, and question answering. Walter Kintsch (1974) was working on much the same connections. The HAM theory provided a parsimonious explanation for many memory phenomena, such as interference effects, recognition memory, and the relation of explicit memory to implicit memory tests. Moreover, it showed one way of moving memory research away from experiments
emotion such as sadness was conceived as a biologically innate structure or unit in the brain; evolution presumably endowed us with, say, six or seven different emotion units (fear, anger, elation, disgust, etc.). A given emotion unit could be turned on either by thoughts or by learned appraisals of external events. Once turned on, the emotion unit would serve as a powerful and persisting source of activation that spreads throughout its associative network. Recall that in HAM, associative networks are set up to record events and facts about the world. If an event like your dog's death caused you to feel sad, then the record of that episode would be recorded into memory associated with the sad emotion it evoked. Later, if you are feeling sad and asked about your dog, the memory of your dog dying should become more available to you at that moment than would happy or neutral memories about your dog.

Although we repeated the mood-dependent result three times before publishing it, later attempts by Jack Mayer and me produced spotty and sometimes null results. I was puzzled by these variable outcomes. During those disappointing days, I learned the value of a good mentor. I called Neal Miller who advised, “Keep working around the edges of the problem by trying new perspectives and variations. Eventually, the pieces of the puzzle should fall into place.” And so they did. Eric Eich and I independently kept experimenting until we could reliably produce emotion dependency. Eric (Eich, 1995) figured out that you must ensure that subjects experience strong moods and involve them actively in generating the materials they must later recall. We can now produce reliable and robust mood-dependent memory. As Neal said, “The best strategy is to keep the faith and believe that future work will correct the apparent discrepancies.”

Also, the emotion network model implies that when an emotion is aroused, it activates congruent associations, themes, and categories, making them more available to influence cognition. These mood-congruent influences on cognition have been observed consistently throughout many studies. For example, happy people remember more of the pleasant material in a mixed list of positive and negative material, whereas sad people remember more of the unpleasant material. Because themes and associations are primed by one’s mood, congruency shows up in people’s free associations, in the stories they compose about ambiguous human scenes, in their forecasts of future good or bad

EMOTIONAL FACTORS IN MEMORY AND COGNITION

My work on how emotion influences cognition began with a study of emotion-state-dependent memory in 1977. I was fascinated by the phenomena of drug-state-dependent memory—the exotic idea that people can have some memories that are accessible only when they are in a particular physiological state but not otherwise. I wondered whether strong emotional states would produce state-dependent memory.

Having learned hypnotic techniques from Ernest Hilgard, I knew that hypnosis could arouse strong emotions. In initial experiments, we used hypnotically induced happiness or sadness to demonstrate emotion-state-dependent memory. In our most successful demonstration, hypnotized college students learned two lists of unrelated words, one when happy and the other when sad. They then were tested for free recall of both lists when they were either happy or sad. When sad, people did better recalling the list they’d learned when they had been sad. Conversely, when happy they did better recalling the list they had learned when they had been happy. Such mood dependence occurs not only with word lists in the laboratory but also when happy or sad people recall autobiographic events from their lives (just as daily fluctuations in my mood as I write this autobiography probably cause my memory portrait to be more or less flattering!).

To explain the mood and memory results, I fell back on the HAM theory and proposed a simple associative network theory. A basic

Anderson later introduced major improvements that made the theory more powerful. In one major improvement, he introduced the concept of productions: These are the learned routines that move the cognitive system through its various as it works through tasks in pursuit of its goals. Anderson and his associates have applied that theory to many cognitive tasks, typically fitting in quantitative detail the behavioral data of people performing those tasks (e.g., Anderson & Labiere, 1998). His theory, dubbed ACT for “Adaptive Control of Thought,” far outstrips any other proposal as a comprehensive theory of cognition and memory.

using lists of unrelated items and toward dealing more rigorously with memory for coherent text.
things, and in their evaluations of such things as their health, career, and marriage. As expected, chronically depressed people exhibit the expected mood-congruent biases in their memory and judgments.

As I was working on mood-congruent memory, Australian social psychologist Joe Forgas visited Stanford. Joe helped me shift gears and extend the emotion research into studies of social cognition. One of our first experiments showed how people's mood influenced their online perception of their social interactions in a videotaped interview. People in a bad mood perceived themselves online as emitting more negative, antisocial acts; those in a good mood saw themselves emitting more positive, prosocial acts. Mood affects people's moment-to-moment interpretations of speech and ambiguous body language, even when they are describing video recordings of themselves.

Despite the wide-ranging impact of mood congruence in many demonstrations, research has also identified some conditions that moderate this effect. For example, mood-congruent influences are strongest when the judgments involve ill-defined, subjective considerations that come to mind as subjects compose their opinions on topics that they hadn't previously thought much about. In contrast, mood effects are weaker with strongly entrenched attitudes or when a subject is motivated to override his bad moods. These amendments and corrections to the mood-congruence story brought home an important lesson for me. Early reports about psychological phenomena are typically qualified as subsequent research reveals the complexities and interacting factors that moderate the initial effect.

**Connectionist Modeling of Category Learning**

In the late 1980s and early 1990s, research on human category learning came back into fashion, inspired in large part by popular ideas of Eleanor Rosch. The main tenets were that most natural categories as well as man-made ones are fuzzy, ill defined, and scattered around a central prototype, with different instances showing a graded resemblance to the prototype. Moreover, in classifying new instances, people were not following a rule but rather were making a probabilistic (best guess) decision between category alternatives. Learning experiments by Rosch, Michael Posner, Doug Medin, John Bransford, and many others persuaded researchers of the wisdom of this approach. Clearly, the simple model of category learning I had worked on in the 1960s (i.e., rule-based hypothesis testing) was inappropriate for dealing with these new data. Still, I wished to have a reasonably simple learning model that would learn ill-defined categories and show all the basic phenomena observed there.

My student, Mark Gluck, and I formulated a simple connectionist model of the category learning process. Categorization tasks to which we applied the model might require subjects to classify, say, 40 diagrammatic faces into two biological families based on their facial features (e.g., size of ears, nose, and chin); a different task might have subjects learn which medical symptoms of patients (e.g., body temperature or white blood cell count) were predominantly associated with one or another underlying disease. The model we proposed mimicked the learners' task: It would examine the series of stimulus patterns (facial features) one by one, predict the category for each, receive feedback about the correct category for each pattern, and then adjust the associative weights between the facial features presented on this trial and the correct category. A major assumption was that the individual weights would be adjusted trial by trial on the basis of the difference between the desired (correct) category versus that predicted by the collection of features available in the presented pattern. This idea (called error correction) came from modern work in Pavlovian conditioning from Robert Rescorla and Allan Wagner. This error-correction idea contrasted with the earlier notion (called the Hebb rule) that associations only required simple contiguous occurrence of any stimulus with the desired response, independent of other stimuli that might accompany its occurrence. A second assumption of our model was that the category response the subject gave to a stimulus pattern reflected the difference in the sum of weights from each stimulus feature to each category.

Gluck and I used this model to fit in quantitative detail results from several prototype-learning experiments. Among other things, the model predicted the observed learning curves and percentage correct for each stimulus pattern, and it gave graded typicality judgments with more confident responses to stimulus patterns closer to the prototypes of the categories. The model also showed (as did subjects) an unusual form of base-rate neglect, whereby rare combinations of stimulus features
produced far less accurate category judgments than were warranted by the statistics of the stimulus-to-category ensemble. A number of such results demonstrated that the Rescorla–Wagner conditioning rule extended to human category learning and explained some novel phenomena.

We showed that the simple model was essentially computing (converging to) a least squares multiple regression equation using the collection of stimulus features to predict the correct categories. It is known that such linear regression schemes cannot learn categories that depend on interactions between input features. An example of this type of category is one based on an exclusive disjunction rule, that is, members of the category have one critical feature or another but not both together. To handle all such matters, Gluck and I proposed a simple emendation of the model, namely, to include as input to the model conjunctions of the elementary stimulus features shown on a given trial (e.g., a frown and clenched teeth might together compose a conjunctive feature). We found that this augmented model was quite powerful in explaining a wide range of category learning data. Unfortunately, even the augmented model encountered some major flaws (experimental data it could not explain), leading Mark eventually to advance to more complicated models.

**Memory for Narratives**

The last area of research I investigated concerns narrative memory and mental models of narratives. Although Anderson and I had addressed the memory representation of single sentences, I wanted to understand the moment-by-moment cognitive processing by which people comprehend and remember connected sentences in a coherent text, such as a history book or a simple story. Impressed by the pioneering work of Roger Schank, Dave Rumelhart, and Walter Kintsch, my students and I began researching narrative understanding and memory.

This research evolved over time. First we explored the psychological reality of story grammars, that is, showing that people's remembering of a narrative varies with how well it conforms to (or violates) norms of the ideal story grammar or story schema. Next, we examined the way that readers' interests and perspectives influence how they interpret characters' actions and thus bias their recall of a story. Then, we examined the inferences that readers make to connect together and make coherent different parts of a story. Influenced by the theories of Roger Schank and Bob Abelson (1977), John Black and I (Black & Bower, 1980) explored the idea that readers use their knowledge of human goals and plans to explain and understand a story's major plot. By inferring causal connections between characters' goals and actions, readers extract a causal network that courses throughout the story and explains why actions and outcomes occurred. The causal network is the gist of what people remember from a story. This causal-networking theory was developed primarily by the research of Tom Trabasso (my first postdoc) and Art Graesser. So, as always, the field was aiming at a theory that allowed us to predict with reasonable accuracy the details of behavior—in this case, which aspects (assertions) of a narrative readers or listeners would judge as most central and important and would later remember.

Finally, a group of theorists led by Phil Johnson-Laird have urged researchers to examine how readers construct and use their mental model of the situation described in the story. The mental model that readers construct guides the interesting conceptual inferences that determine what they remember from a story. My postdoctoral students Mike Rinck and Dan Morrow and I studied how readers use the text moment by moment to update their developing mental model. In one line of work, we measured what happens when readers move their focus of attention within a previously constructed mental model, for example, when the narratives describe characters moving around through a well-known building. The focus of the inner eye is like a spotlight that follows the character around on an inner stage. We found, for example, that questions about objects close to the reader's focus within the mental model are quickly answered. In contrast, this priming diminishes for objects farther from the current focus and decays dramatically as the item or location fades out of the narrative focus. Readers also suppress this activation when a story describes a large gap of time between events, a large jump in story distance, or a change in the active status of the character's goal. These are stunningly quick, adaptive processes that occur outside the reader's awareness.
The findings in this line of research can be explained by the embodied cognition theories of a former student, Larry Barsalou (2003), and related ideas of Art Glenberg (1997), Rolf Zwaan (2004), Allan Paivio (1990; Sadoski & Paivio, 2001), and their associates. They propose a sensorimotor basis for knowledge representation and suggest that understanding sentences in a story requires the reader to mentally simulate the actions and perceptual experiences of the character. In some ways this approach is returning me to the earlier research done on mental imagery in memory. By giving special prominence to the subject’s perceptual-motor simulation as an avenue for his comprehension, these theories easily accommodate the findings of countless studies in the field.

POSTSCRIPT

For 50 years my career has taken me down these seven major highways that have traversed countless detours and diversions. Along the way I have picked up—or been picked up by—many exciting and interesting traveling companions. Looking back, this succession of research topics reminds me of learning theory itself, which constantly expands to encompass and explain more complex phenomena. Parts of my interests and knowledge have come from serving as a consulting editor for numerous professional journals and from editing for 25 years the annual Psychology of Learning and Motivation volumes.

Throughout my career, I have been fortunate to receive many professional honors, including several honorary degrees and election to the National Academy of Sciences, the American Academy of Arts and Sciences, the Society of Experimental Psychologists, and the American Philosophical Society. In addition, I have been elected president of the American Psychological Society, the Western Psychological Association (twice), the Cognitive Science Society, the Psychonomic Society, and the Experimental Psychology section of the American Psychological Association. I was identified as one of the 100 most important psychologists of the 20th century. In 1975, Stanford awarded me the Albert R. Lang Endowed Professorship Chair. I chaired the Psychology Department for 4 years and served as associate dean of Humanities and Sciences for 3 years. In 1992–1993, I served as chief scientific advisor to the NIMH director. During that time, my associates and I organized seven groups of psychologists, totaling about 50 individuals, to review significant contributions of basic behavioral research. John Kihlstrom and I edited and coauthored that report, which was presented to the U.S. Congress in the fall of 1993.

I have had extraordinary good fortune in both my professional and private life. I’ve been at Stanford during an ideal time for my career, and I’ve been honored to work with many extraordinary students and colleagues. My private life has been stable and rewarding, because Sharon and I have been happy together since 1957. We’ve lived to see our three talented kids lead productive lives, and now we take delight in our four beautiful grandchildren. A small-town Ohio boy could hardly have imagined a better life. In retrospect, I can see that in fulfilling my life’s goals, I have redeemed the broken dreams that my father abandoned, and I have done so in a manner that made my parents proud. That is a very satisfying thought. Looking back, I see my life as an unbroken string of happy memories stretching back to that kid from eastern Ohio dreaming of pitching in a crowded Yankee stadium with the bases loaded and striking out the fearsome Joe DiMaggio. I remember. Oh yes, I remember.

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